GROUP REPLICATION PROJECTS

In groups of three (3) you will choose a paper from the attached list. As a group you will produce an approx. 2,500-word (maximum) report that (1) summarizes the original study; (2) produces a motivating figure (graphic); (3) replicates the main quantitative finding; and (4) proposes a potential extension of the study.

DUE: Thursday, December 7, 11:59 PM EST

The structure of this replication report will be as follows:

1. ***Purpose***. This (less than 800 word) section serves as the introduction to your report and includes a summary of the published paper that your group has chosen to replicate. After briefly introducing the purpose of your report (to replicate the results of the publication) this section should read like an <u>executive summary to the original published article</u>. In other words, it should have three subsections:
   a. *Motivation*. What is the research question pursued by the original authors? Why is this a compelling research question? What are the potential policy implications?
   b. *Data and Methods*. What is the source of the original data? What methods did the original authors use? You may summarize in general terms here – some of the authors use econometric methods beyond the scope of this course. Do the best you can to explain what the authors did and the data and methods they used.
   c. *Results and Conclusions.* Summarize the general findings of the article then clearly state the main empirical result that your group will attempt to replicate.

2. ***Replication Data and Methods.*** This (less than 500 word) section will describe the data you will be using to replicate the above result. How does it differ than the data used in the original study (some groups will only have a subset of the original data)? If possible, explicitly compare summary statistics from the original data with summary statistics of your replication data. If this is not possible, provide summary statistics only for your replication data.

3. ***Replication Results***. This (less than 800 word) section will include (1) one figure (scatterplot, bar chart, etc.) that motivates the research question under investigation, (2) the replication (regression) results table, and (3) a brief discussion of those results. The figure may be a replicate of a motivational graphic used in the original study or an entirely new figure. The regression results table must be formatted (using outreg2 or other method) to make it easily readable and consistent with disciplinary conventions. Finally, the discussion should focus on the internal validity of your replication results and how they are consistent with (or not) the results in the original study.

4. ***Potential Extension***. This (less than 400 word) section will include a brief discussion of potential extensions to the phenomena explored in the original study. What extensions to the original research questions could improve our understanding of the causal mechanisms in question? Has a confounding factor been ignored? Would a different dataset yield different results? Some of these papers are several decades old, would current data and context yield similar results, or not?

Formatting and Submission Guidelines

Submission of your project will be both in class (hard copy) and electronic:

1. **Each group will <u>submit a hard copy of the final report in class on the due date: Thursday, Dec. 7</u>.**

   The final report should be no more than 2,500 words (4 – 6 pages double-spaced) and be formatted as follows:

   - 12 pt. font
   - 1-inch margins
   - double-spacing
   - page numbers in lower right-hand corner
   - title and group member names on first page (or title page)
   - Chicago or APA style citations (if necessary)

   In general, I expect that many of you will not need to cite any research other than the original study, but if you do (which is fine) be sure you cite those sources appropriately.

2. **Each group will <u>upload a zipped folder</u> (via the Group Replication Project Assignment link on Moodle) with the following structure <u>by 11:59 pm EST on the due date</u>:**

   Bentley-James-Mia-298-Replication (last names of group members alphabetically and connected with no spaces by hyphens-298-Replication)**.zip**

   - *finalpaper.docx*   (final replication report in Word format)
   - *finalpaper.pdf*   (final replication report in pdf format)
   - *readme.pdf*   (optional, one page explaining how to reproduce replication results)
   - **Data**
     - *replicationdata.dta*   (for example *smoking.dta*)
   - **Analysis**
     - *Analysis.do*   (do file that produces figure and regression results)
     - *Analysis.log*   (log file logging results of Analysis.do)
     - *Figure1.gph*   (copy of any figures)

   NOTE: folders are in **bold** and files are in *italics*

REPLICATION PAPERS

1. **Almond, D., Chay, K. Y., & Lee, D. S. (2005). The costs of low birth weight.** *The Quarterly Journal of Economics*, *120*(3), 1031-1083.

ABSTRACT
Low birth weight (LBW) infants experience severe health and developmental difficulties that can impose large costs on society. However, estimates of the return to LBW-prevention from cross sectional associations may be biased by omitted variables, such as genetic factors. To address this, we compare the hospital costs, health at birth, and infant mortality rates between heavier and lighter infants from all twin pairs born in the United States. We also examine the effect of maternal smoking during pregnancy—the leading risk factor for LBW in the United States—on health among singleton births after controlling for detailed background characteristics. Both analyses imply substantially smaller effects of LBW per se than previously thought, suggesting two possibilities: 1) existing estimates overstate the true costs and consequences of LBW by at least a factor of four and by as much as a factor of twenty; or 2) different LBW-preventing interventions have different health and cost consequences, implying that policy efforts that presume a single return to reducing LBW will be suboptimal.

DATA
The dataset **birthweight_smoking.dta** is a subset of the data used in this paper - from the 1989 linked National Natality-Mortality Detail files - and includes information on 3000 births in Pennsylvania in 1989, including infant birthweight, the mother's age, education, marital status, and behavioral attributes during pregnancy (smoking, drinking, and prenatal visits).

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2. **Angrist, J. D., & Evans, W. N. (1998). Children and Their Parents' Labor Supply: Evidence from Exogenous Variation in Family Size.** *American Economic Review*, 88(3), 450-477.

ABSTRACT
Research on the labor-supply consequences of childbearing is complicated by the endogeneity of fertility. This study uses parental preferences for a mixed sibling-sex composition to construct instrumental variables (IV) estimates of the effect of childbearing on labor supply. IV estimates for women are significant but smaller than ordinary least-squares estimates. The IV are also smaller for more educated women and show no impact of family size on husbands' labor supply. A comparison of estimates using sibling-sex composition and twins instruments implies that the impact of a third child disappears when the child reaches age 13.

DATA
The dataset **fertility.dta** contains data on 254,654 women between the age of 21 and 35. The data in **fertility** are a subset of the data used in the Angrist-Evans paper. (The file **fertility_small** contains data on a 30,000 randomly selected women from the **fertility** data set. This smaller dataset is provided for students with memory limitations on their computer software).

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3. **Bertrand, M., & Mullainathan, S. (2004). Are Emily and Greg more employable than Lakisha and Jamal? A field experiment on labor market discrimination.** *American Economic Review*, *94*(4), 991-1013.

ABSTRACT
We study race in the labor market by sending fictitious resumes to help-wanted ads in Boston and Chicago newspapers. To manipulate perceived race, resumes are randomly assigned African-American or White-sounding names. White names receive 50 percent more callbacks for interviews. Callbacks are also more responsive to resume quality for White names than for African-American ones. The racial gap is uniform across occupation,

industry, and employer size. We also find little evidence that employers are inferring social class from the names. Differential
treatment by race still appears to still be prominent in the U.S. labor market.

DATA

The dataset **names.dta** contains resume, call-back and employer information for 4,870 fictitious resumes sent in response to employment advertisements in Chicago and Boston in 2001, in a randomized controlled experiment conducted by Marianne Bertrand and Sendhil Mullainathan, and used in the above paper.

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4. **Case, A., & Paxson, C. (2008). Stature and status: Height, ability, and labor market outcomes.** *Journal of Political Economy*, *116*(3), 499-532.

ABSTRACT

The well-known association between height and earnings is often thought to reflect factors such as self-esteem, social dominance, and discrimination. We offer a simpler explanation: height is positively associated with cognitive ability, which is rewarded in the labor market. Using data from the United States and the United Kingdom, we show that taller children have higher average cognitive test scores and that these test scores explain a large portion of the height premium in earnings. Children who have higher test scores also experience earlier adolescent growth spurts, so that height in adolescence serves as a marker of cognitive ability.

DATA

The dataset **Earnings_and_Height.dta** are taken from the US National Health Interview Survey for 1994 and include information on 17,870 workers. They are a subset of the data used in Case and Paxson's paper above.

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5. **Clay, K., Troesken, W., & Haines, M. (2014). Lead and mortality.** *Review of Economics and Statistics*, *96*(3), 458-470.

ABSTRACT

This paper examines the effect of waterborne lead exposure on infant mortality in American cities over the period 1900 to 1920. Variation across cities in water acidity and the types of service pipes, which together determined the extent of lead exposure, identifies the effects of lead on infant mortality. In 1900, a decline in exposure equivalent to an increase in pH from 6.675 (25th percentile) to 7.3 (50th percentile) in cities with lead-only pipes would have been associated with a decrease in infant mortality of 7% to 33%, or at least twelve fewer infant deaths per 1,000 live births.

DATA

The datafile **Lead_Mortality** contains data on 172 U.S. cities in 1900. These data were provided
by Professor Karen Clay of Carnegie Mellon University and are a subset of the data used in her
paper with Werner Troesken and Michael Haines above.

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6. **Dahl, G., & DellaVigna, S. (2009). Does movie violence increase violent crime?.** *The Quarterly Journal of Economics*, *124*(2), 677-734.

ABSTRACT

Laboratory experiments in psychology find that media violence increases aggression in the short run. We analyze whether media violence affects violent crime in the field. We exploit variation in the violence of blockbuster movies from 1995 to 2004, and study the effect on same-day assaults. We find that violent crime *decreases* on days with larger theater audiences for violent movies. The effect is partly due to voluntary incapacitation: between

6 P.M. and 12 A.M., a one million increase in the audience for violent movies reduces violent crime by 1.1% to 1.3%. After exposure to the movie, between 12 A.M. and 6 A.M., violent crime is reduced by an even larger percent. This finding is explained by the self-selection of violent individuals into violent movie attendance, leading to a substitution away from more volatile activities. In particular, movie attendance appears to reduce alcohol consumption. The results emphasize that media exposure affects behavior not only via content, but also because it changes time spent in alternative activities. The substitution away from more dangerous activities in the field can explain the differences with the laboratory findings. Our estimates suggest that in the short run, violent movies deter almost 1,000 assaults on an average weekend. Although our design does not allow us to estimate long-run effects, we find no evidence of medium-run effects up to three weeks after initial exposure.

DATA
The dataset **Movies.dta** contains data on the number of assaults and movie attendance for 516 weekends from 1995-2004. These are an aggregated version of data provided by Gordon
Dahl and Stefano DellaVigna that were used in their paper above.

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7. **Evans, W. N., Farrelly, M. C., & Montgomery, E. (1999). Do workplace smoking bans reduce smoking?.** *American Economic Review*, *89*(4), 728-747.

ABSTRACT
In recent years workplace smoking policies have become increasingly prevalent and restrictive. Using data from two large-scale national surveys, we investigate whether these policies reduce smoking. Our estimates suggest that workplace bans reduce smoking prevalence by 5 percentage points and daily consumption among smokers by 10 percent. Although workers with better health habits are more likely to work at firms with smoking bans, estimates from systems of equations indicate that these results are not subject to an omitted variables bias. The rapid increase in bans can explain all of the recent drop in smoking among workers relative to nonworkers.

DATA
The dataset **Smoking.dta** is a cross-sectional data set with observations on 10,000 indoor workers, which is a subset of a 18,090-observation data set collected as part of the National Health Interview Survey in 1991 and then again (with different respondents) in 1993. The data set contains information on whether individuals were, or were not, subject to a workplace smoking ban, whether or not the individuals smoked and other individual characteristics and was used in the above paper.

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8. **Hamermesh, D. S., & Biddle, J. E. (1994). Beauty and the labor market.** *The American Economic Review*, *84*(5), 1174.

ABSTRACT
We examine the impact of looks on earnings using interviewers' ratings of respondents' physical appearance. Plain people earn less than average-looking people, who earn less than the good-looking. The plainness penalty is 5-10 percent, slightly larger than the beauty premium. Effects for men are at least as great as for women. Unattractive women have lower labor-force participation rates and marry men with less human capital. Better-looking people sort into occupations where beauty may be more productive; but the impact of individuals' looks is mostly independent of occupation, suggesting the existence of pure employer discrimination.

DATA
The dataset **beauty.dta** includes of subset of the data used by Hamermesh and Biddle in the above paper. The data includes wages and other characteristics for 1,260 workers from the 1977 Quality of Employment Survey.