MEDIA ROLE MODELS AND BLACK EDUCATIONAL ATTAINMENT: EVIDENCE FROM THE COSBY SHOW

Kirsten Cornelson*

March 14, 2018

Abstract

This paper assesses the influence of role models on educational attainment by examining the impact of a popular 1980's sitcom: *The Cosby Show*. The show portrayed an upper-middle class black family, and frequently emphasized the importance of a college education. If role model effects exist, young black people should have responded more strongly to this message. I test this hypothesis by relating changes educational attainment to city-level *Cosby* ratings, using Thursday NBA games as an instrument. I find that *Cosby* increased years of education by one-tenth of a year among the black sample, but had no effect for whites. *(JEL codes: I21, J15)*

*University of Notre Dame, 3060 Jenkins-Nanovic Halls, Notre Dame IN, 46556; kcornels@nd.edu. I gratefully acknowledge the support of Frank Peers, who helped fund this research through the Tom Easterbrook Award, and Nielsen Media Inc., who provided me with data. I would like to thank Michael Baker and Dwayne Benjamin for additional funding, and for their helpful suggestions and comments. Robert McMillan, Aloysius Siow, Uros Petronijevic, Adam Levecchia, and Michael Gilraine also provided valuable feedback.

1 Introduction

Black-white education gaps are a striking feature of the U.S. economy. White men and women obtain about one more year of education, on average, than their black counterparts. For cohorts born in the early 1980's, white women (the group with the highest educational attainment) were about five percentage points more likely to graduate from high school than black men (the group with the lowest attainment); for college completion, the same gap was a full twenty percentage points. In spite of the large increase in the return to college over the past sixty years (Goldin & Katz 2008), college attainment has stagnated or declined over this period for both black men and black women.

There is a great deal of research attempting to account for these gaps and their patterns over time. A recurring theme in this literature is the possibility that role model effects may help explain the persistence of black-white (as well as male-female) education gaps. Papers by Thomas Dee (Dee 2004, Dee 2005, Dee 2007) and Fairlie, Hoffman & Oreopoulos (2014), for example, show that students perform better, and are more positively evaluated, in classrooms with same-race and same-gender teachers. Kling, Liebman & Katz (2007) and Clampet-Lundquist et al. (2011) suggest that the lack of positive effects for adolescent boys in the Moving to Opportunity studies may be explained by changes in contact with father figures. Evans, Garthwaite & Moore (2012) suggest that role model effects may have amplified the impact of crack cocaine on black men's high school graduation rates. Autor & Wasserman (2013) argue that men's declining educational outcomes may be due, in part, to the increasing prevalence of single motherhood (particularly in the black community) and the resulting lack of close male role models for young men raised in these households. In all of these cases, however, it is difficult to rule out the possibility that differences in parental or teacher inputs are driving the results.

Role models do not, of course, have to take the form of teachers or parents; they need not even exist in real life. Popular belief suggests that children learn from and mimic the characters they observe on television, in movies, and in video games. Recent empirical

evidence supports the claim that people are influenced by what they see on television, even when it is primarily comprised of fiction and has little informational content. Jensen & Oster (2009) show that rural villagers in India dramatically changed their attitudes and behavior towards women once satellite television was introduced. As the most popular programs were soap operas and game shows, this was unlikely to be driven by exposure to educational or informational programs. Similarly, Eliana La Ferrara & Duryea (2012) show that exposure to soap operas in Brazil was associated with reductions in fertility. Kearney & Levine (2015*b*) show that MTV's Sixteen and Pregnant influenced teen fertility outcomes, while Kearney & Levine (2015*a*) show that exposure to Sesame Street may have improved educational outcomes for poor and minority children. These studies do not provide direct evidence about role modeling, because they do not distinguish between alternative channels through which learning from television occurs. They do suggest, however, that television is powerful enough to tell us about role model effects, given the right setting.

In this paper, I use the introduction of a popular television show in the 1980's - *The Cosby Show* - to estimate the impact of television role models on the educational decisions of young people. The show depicted the family life of the fictional Huxtables, a black family headed by Heathcliff (a doctor) and Clair (a lawyer.) The show's focal point is Cliff, played by show creator Bill Cosby, who guides his five children through their struggles with school, dating and other day-to-day issues. The show placed an unusually strong emphasis on education and family values. The parents frequently discuss the importance of a college education with their children, and the three oldest Huxtable children all go to college during the course of the show.

The Cosby Show was immensely popular. At its peak, nearly one-third of Americans tuned in to watch it on any given Thursday night. The show therefore had the potential to influence a large number of young people. My hypothesis is that it had a particularly strong impact on black children and teenagers, who would identify most with its main characters.

I test this hypothesis by relating educational attainment among cohorts who were children

or teenagers during *The Cosby Show*'s run to city-level Cosby Show Nielsen ratings. Of course, Cosby Show ratings are unlikely to be random. In order to address the potential endogeneity of Cosby Show popularity, I instrument for Cosby Show ratings using Thursday night NBA games. I show that Cosby Show ratings were significantly lower in cities where NBA teams tended to play frequently on Thursday nights over this period. Using a combined instrumental variables and difference-in-difference strategy, I show that Cosby Show exposure appeared to increase the educational attainment of black cohorts born in the 1960's or later, relative to earlier cohorts in the same cities. Overall, I estimate that Cosby increased years of education by around 0.1 years in this sample. In contrast, there appears to be little response among young white people.

The IV strategy requires that Thursday night NBA games have no effect on educational attainment, other than through exposure to *The Cosby Show*. In support of this assumption, I show that Thursday NBA games did not have similar effects in the years leading up to (1976-1984) or following (1992-2000) *The Cosby Show*'s run. I also show that Thursday night NBA games that were less likely to conflict with *The Cosby Show* because of their timing had a weaker effect on both ratings and education. These findings strongly suggest that my results are driven by Cosby exposure. I also provide evidence that the effects were driven by role-modeling, rather than through reduced discrimination.

The next section provides background on *The Cosby Show*. Section 3 presents my data, and describes my empirical strategy. I then present my main results in Section 4, and perform a number of placebo and robustness tests, described in Section 5. In section 6, I adjust the magnitude of the IV estimates to account for measurement error in the Cosby exposure index. Section 7 provides evidence that my results are not driven by reduced discrimination, which suggests that role-model effects are at work. Section 8 concludes.

2 Background

The Cosby Show premiered on NBC in 1984, and was an instant critical and commercial success. Critics called it "the best, funniest and most humane show of the new season" (Shales 1984) and "a rare commodity – a truly nice development in a medium that seems increasingly preoccupied with trash." (O'Connor 1985). The show was also popular among viewers, vaulting to third place in the Nielsen ratings in its first season, and holding the number one spot for the following five years.

The show followed the lives of Clair and Heathcliff Huxtable and their five children: Sondra, Denise, Theo, Vanessa and Rudy. Episodes of *The Cosby Show* frequently showed the Huxtable parents correcting their children's behavior, discussing and enforcing household rules, and attempting to guide their children towards good decisions. By portraying the family in this way, the creators of the show were consciously attempting to provide role models for both parents and children. Alvin Poussaint, a Harvard professor of psychiatry who acted as psychological consultant on the show, remarked:

"We'd like to think [the show] has influenced black family life and family life in general - that it's changed attitudes and values a bit within families, changing white attitudes toward black families and providing role models for black youngsters. I hear from mothers and fathers who say they learn how to be better parents. A lot of parents drag their kids to the TV set to watch it because they believe it has good values and good lessons." (Martin 1989)

One of the values that the show consistently promoted was the importance of education. A large number of the episodes revolved around the children's day-to-day issues at school. A synopsis of an episode from season 1 is typical of the series:

"Vanessa prepares for the science fair. She believes that her project is very good, but Denise warns that she may need to work harder to keep up with the kids in her new advanced class. Vanessa is shocked by the quality of the other kids' projects, and begins to feel embarrassed. She finishes 14th in the competition, and alienates best friend Janet by accusing her

of getting help from her parents. Vanessa assures Cliff that she is capable of succeeding in the class, but needs to put in more of an effort. [...]" (Tv.com)

Other episodes focused on the college decisions of the older children. The eldest daughter, Sondra, is already at Princeton when the show begins and later decides to attend graduate school. The next two children, Denise and Theo, graduate from high school and go to college during the course of the show.

There have been no large scale, credible studies that investigate whether *The Cosby Show* had a long term effect on the behavior of young people. Rosenkoetter (2009) links Cosby viewership to pro-social behavior in children; Mattabane & Merrit (2014) show that Cosby Show viewership is correlated with attendance at historically black colleges. Both studies are quite small scale, however, and neither attempts to control for the endogeneity of Cosby Show exposure. Other research on *The Cosby Show*'s impact has generally focused on its effect on racial attitudes. Most notoriously, a study funded by Bill and Camille Cosby found that white viewers used the show to reaffirm their beliefs that anyone could "make it" in America, and that racial discrimination was not a major barrier to success for blacks (Jhally & Lewis 1992). As I show below, any change in white attitudes induced by the show did not appear to change the labor market prospects of young black people.

3 Data and empirical strategy

3.1 Data

3.1.1 Educational outcomes

In order to link Cosby Show exposure to educational outcomes, I use data from the public use Census samples from 1950-2010, and the 2015 5-year American Community Survey, provided by the IPUMS website (Ruggles et al. 2015). For each city, I calculate average educational attainment for respondents born from 1935-1984 who over age 30 at the time of the survey.

I impose the age restriction to ensure that I am measuring completed education. Note that respondents are assigned to the city in which they currently live, which may not be the same city they lived in as a child or teenager. This will introduce some measurement error into my estimates. To minimize this error, I restrict the sample to individuals living in their state of birth.

My empirical strategy will examine changes in educational attainment across cohorts within a city. I define a city as a Designated Market Area (DMA), which is the unit of measure for my Nielsen ratings data (described in detail in the next section.) There are 190 DMAs in the Nielsen data, which I match to Census metropolitan areas by hand. DMAs typically comprise several Census metropolitan areas. I have 122 DMAs that can be linked to metropolitan areas in all Census samples.

My primary dependent variable is years of education, which I construct based on the IPUMS variable "educ".[1] I will also examine effects across the educational distribution, using variables capturing the proportion of respondents falling into four educational categories (no high school, high school, some college and college graduate.

Figure 1 shows the evolution of average years of education over successive cohorts and by race. For both blacks and whites, educational attainment rose rapidly for cohorts born from 1935-1950, and more slowly thereafter. The black-white gap remained relatively steady at about 1 year over this time period. This persistence is somewhat surprising in light of the large changes to the legal and social environment facing young black people over this time period. One potential reason for this persistence is the presence of role-model effects, which would make educational attainment relatively difficult to change across cohorts; my analysis sheds light on the potential importance of this channel in explaining ongoing racial achievement gaps.

[1]The coding of the years of education index is: 7 years for individuals with grade 8 or less ; 9, 10, 11 years for grades 9-11, respectively; 12 years for high school graduates or those with 12 years of education; 13 years and 14 years for those with one or two years of college; 14 years for those with a college degree; and 18 years for those with more than a college degree.

3.1.2 Cosby Show exposure

I construct a measure of average Cosby Show exposure using Nielsen ratings. These ratings measure the average proportion of a city's population that watched each episode of the show over the course of a ratings period. I use data from 'sweeps', which are conducted across a large number of cities four times per year, in February, May, July and November.[2] Nielsen Media Inc provided me with the ratings for each sweeps month throughout *The Cosby Show*'s run.[3] *The Cosby Show* had an average rating of 21.6 across all ratings months during its run, indicating that about one-fifth of Americans watched *The Cosby Show* on a given Thursday night. Its popularity peaked in its third season, when over 30% of television viewers watched an average episode.

For ease of interpretation, I convert the Nielsen ratings to an exposure index that captures the average number of hours of *The Cosby Show* watched by residents of a city over a particular time period. I first multiply the Nielsen rating for each city and month by two hours - the maximum possible exposure available during a four-week month. Because *The Cosby Show* was in reruns in July, I drop observations in this month. I then add up this exposure over all seasons to get a city-level measure of exposure.

The first column of Table 1 shows summary statistics on Cosby exposure and its relationship to city characteristics (excluding the education of older cohorts, which I discuss in detail in the next section of the paper.) The average exposure over all cities is approximately 10 hours, or 20 half-hour episodes. This ranges from around 3.6 hours in Longview, Texas to over 16 hours in Alexandria, Louisiana. The regression results show that Cosby was relatively more popular in cities with a higher proportion of black people and a lower proportion

[2]The sweeps ratings are constructed through the use of diaries, which record what was on the television for each 15 minute segment throughout the day. Currently, Nielsen ratings include viewership of shows that have been previously recorded. I have not been able to discover whether this was standard practice in the 1980's. About 21% of households in the U.S. had a VCR by 1985 (Television Bureau of Advertising 2012). Time shifting is not a major concern for my analysis, however. If the ratings do not include viewership of taped episodes of *The Cosby Show*, my first stage will overestimate the impact of the NBA on Cosby Show viewership. In this case, I should not expect to see a strong second stage.

[3]Nielsen data are proprietary and are licensed to researchers under strict non-disclosure agreements.

of immigrants. It was also more popular in cities with a higher proportion of children, and a lower proportion of people in middle-age; these latter relationships correspond well to the general television watching behavior of these age groups.[4]

A key limitation of the Nielsen data is that they are not demographic-specific. They capture the viewership behavior of all residents of a city, not the viewership of my population of interest (young black and white people.) This issue will tend to bias my estimates, because the observed first-stage relationship will be different from the true first stage relationship. In a later section, I use cross-city variation in demographics to attempt to understand whether the first stage relationship varies significantly by race and age group, and to correct the magnitude of my estimates to account for this error. I ignore this issue in the main results section, however, and simply use this section to establish that NBA-driven exposure to *The Cosby Show* is significantly related to declines in educational attainment. (Note that the statistical significance of my results is not affected by this issue, because is driven by the underlying relationship between Thursday NBA games and educational attainment.) It is important to note, however, that the magnitude of the results in this section of the paper are meaningless, and should not be interpreted as the true "per-hour" effect of *The Cosby Show*.

3.1.3 NBA games

In order to address the endogeneity of Cosby Show exposure, I use a potential instrument that captures a source of competition for Cosby viewership: Thursday night NBA games. To construct the NBA games measure, I first create a team-level count of the number of games played on Thursday nights over all seasons of *The Cosby Show*, during the regular season. I assign each city the three geographically closest NBA teams, and add up all Thursday games played by these three teams.[5]

[4]According to the General Social Surveys from 1972-1990, individuals under 20 spent about 3.6 hours per day watching TV, compared to 2.8 for individuals aged 21-60 and 3.4 for individuals aged 61 and over.

[5]I eliminated one outlier team, Houston, that appeared to significantly influence the first stage regression coefficients. Cities closest to Houston were assigned the next three closest teams in its place.

Note that since all teams play the same number of games in the regular season, there is no variation across cities in the total number of NBA games that are played over this period. This eliminates any direct effect of NBA games on educational attainment (which could occur, for example, if NBA games crowd out homework.) Instead, the effect of the instrument can be interpreted as capturing the effect of a Thursday night NBA game, compared to a game on another night of the week.

The second column of Table 1 shows summary statistics on Thursday NBA games, and the relationship of this variable to city characteristics (other than the education of older cohorts, which is discussed in the next section.) The typical city in my sample experienced 220 Thursday NBA games over this period. This ranges from 163 Thursday games in Buffalo, New York, to 297 Thursday games in Bend and Eugene, Oregon. A regression on city characteristics shows that Thursday games were unrelated to most demographic variables. The only significant predictors of Thursday NBA games are Census division fixed effects; this is to be expected, given the strong geographic correlation in the instrument. Importantly, the lack of a relationship between the same city characteristics that significantly predict Cosby popularity and Thursday NBA games provides evidence that the NBA schedule was not manipulated to minimize disruption to Cosby viewership. If this was the case, Thursday games should have been placed in cities where Cosby exposure was predicted by network executives to be unusually low (to prevent cannibalization.) There is no evidence of such a relationship in Table 1.

3.2 Empirical strategy

My empirical strategy relies on a combined instrumental variables and difference-in-differences framework. The fundamental problem that motivates this strategy is the potential endogeneity of *Cosby* exposure with respect to educational attainment. People with different educational propensities may have been differentially attracted to *The Cosby Show*, although the direction of this relationship is not clear. On one hand, we would expect Cosby viewership

to increase with overall television viewing time, which is negatively related to educational attainment[6] On the other hand, it is possible that *The Cosby Show* may have disproportionately attracted viewers who shared the Huxtables' belief in the importance of education, which would lead to a positive correlation between education and Cosby exposure.

Table 2 explores the relationship between educational attainment among cohorts born from 1935-1959 (who were too old to be affected by Cosby), and Cosby exposure. The table shows that Cosby viewership was significantly higher in cities with low educational attainment among both blacks and whites, with a stronger and more robust relationship for blacks. The negative correlation between educational attainment and Cosby exposure is likely to be driven by the tendency of individuals with lower educational attainment to watch more television.

One possible approach to addressing this endogeneity problem is to use a difference-in-differences strategy that compares *changes* in educational attainment for "treated" cohorts (those young enough to be influenced by Cosby), compared to older "untreated" cohorts in the same city. This does not fully eliminate the endogeneity problem, however. In particular, as Table 1 makes clear, Cosby ratings were strongest in cities with many young people; this suggests that younger cohorts were disproportionately represented among Cosby viewers. Even among cities with similar educational attainment for older cohorts, we should therefore expect Cosby exposure to be higher in places where the educational propensity of younger cohorts was lower. This will tend to mask any positive effect of *The Cosby Show*.

An alternative empirical approach uses a potential instrument for Cosby exposure: Thursday night NBA games. The idea is that Thursday NBA games provide a source of competition for Cosby viewership, which should lower Cosby ratings. The NBA schedule also appears to be plausibly random with respect to educational attainment. The schedule is made each year using an algorithm which ensures that each team plays every other team in its division and conference a set number of times, and which also takes into account teams' preferred

[6]According to the General Social Surveys from 1972-1990, each additional year of education is associated with about a 10-minute decline in television time each day.

dates and veto dates as well as travel times and the number of back-to-back games each team must play. While television networks do have a chance to provide input at the end of this process, the large number of constraints on the final schedule should limit their ability to optimize with respect to viewership.

Unfortunately, as shown in Table 2, Thursday NBA games do not appear to be exogenous with respect to the educational attainment of earlier cohorts. Cities with higher education levels among blacks and whites also have more Thursday NBA games. Unlike with Cosby exposure, however, this relationship appears to be primarily driven by regional correlation in both the instrument and outcome variables: the relationship disappears as geographic controls become increasingly more detailed. While it is possible to include these geographic controls in a regression, this strategy also eliminates a great deal of the variation in the instrument. A regression of Thursday NBA games on the 8 Census division indicators has an R^2 of 0.56; the state indicators have an R^2 of around 0.8.

For this reason, I instead pursue a combination of the IV and difference-in-differences strategies. Even if the instrument is correlated with educational attainment among older cohorts, it may still be valid to examine whether the instrument is associated with *changes* in this relationship over time. The regression I use to examine this question is the following:

$$y^r_{ct} = \alpha^r + \beta^r_1 Exp_c + \beta^r_2 Exp_c * Post_t + \mathbf{X'_c}\gamma^{\mathbf{r}} + \Sigma_t \mathbb{1}_t + \epsilon^r_{ct}$$

In this equation, y^r_{ct} is a measure of the educational attainment of birth cohort t in city c, and Exp_c is my measure of Cosby Show exposure for city c. $Post_t$ is an indicator for whether individuals born at time t were young enough to be influenced by Cosby Show exposure. I will use 1960 as the birth year that divides "treated" from "untreated" cohorts; this birth cohort was aged 24 at the time Cosby began. Note, however, that there may be some effects of Cosby on college attainment for slightly older cohorts: around 17% of college graduates in this era obtained their degrees after age 25. In a more detailed set of regressions, I will examine whether there were any changes in the 1955-1959 cohort, and will show that this did

not appear to be the case. X_c represent city level covariates, which may include geographic fixed effects and/or the same set of covariates shown in Table 1; in some specifications, I also allow the effects of these covariates to differ by cohort. $\mathbb{1}_t$ are birth year fixed effects. The subscript r on all coefficients arise because I run the regressions separately by race.

For this strategy to uncover a causal effect of *The Cosby Show*, it must be the case that i) the instrument is uncorrelated with trends in education prior to the treatment period, and ii) there is no reason to believe that Thursday NBA games were high in cities where education was poised to fall even in the absence of *The Cosby Show*. The first assumption is easy to test. In Table 3, I show estimates from a regression of years of education on Thursday NBA games and Thursday NBA games interacted with a trend among cohorts born from 1935-1959. For completeness, I also show the same analysis using Cosby exposure and an exposure-specific trend in Panel 1, with the analysis for games in Panel 2. For exposure, there are significant and positive pre-trends for both blacks and whites in a regression with no additional controls. Including the covariates shown in Table 1 eliminates these trends for both blacks and whites. Although there are no pre-trends in exposure once covariates are added, we might still be concerned that a decline in education among later cohorts in cities with high Cosby exposure is driven by an unobserved third variable: low educational propensities among young viewers drive higher television watching, and therefore higher Cosby exposure. This is why I turn to the instrument. The second panel of Table 3 shows the same regression using NBA games and games-related trend. While there are some significant negative pretrends for the white sample in the raw version of the regression, this relationship disappears once Census division fixed effects are added.[7] There are no significant pre-trends for blacks in any specification. To ensure that pre-trends aren't driving my results, I will add Census division fixed effects to all of the IV regressions. I will also examine the sensitivity of my estimates to including controls for pre-trends.

[7]While this is impossible to see given the level of detail in the table, the coefficients on the trend terms fall by an order of magnitude, in addition to becoming statistically insignificant, once the division fixed effects are added.

The second assumption, that Thursday NBA games are not related to declines in educational attainment for any reason other than through Cosby exposure, is impossible to test precisely. However, I am able to provide additional evidence that I am uncovering a causal effect in the robustness and placebo section. It is also important to emphasize that the likely direction of any bias in this version of my regressions will work against my finding an effect of *The Cosby Show.* I will be looking for a negative relationship between Thursday NBA games and the educational attainment of young cohorts, relative to older cohorts. If networks manipulated the NBA games schedule to minimize cannibalization of Cosby, they should have allocated games to cities where Cosby had lower predicted ratings. These would be cities where educational propensities were relatively *high* among younger viewers. Therefore, this combined empirical strategy is not subject to the same problems that arise in the pure difference-in-difference regression related changes in education to Cosby exposure.

4 Results

4.1 OLS results

Table 4 shows the results from running an OLS version of my main regression equation separately by race. The dependent variable in these regressions is the composite variable "years of education". The first column for each race shows the results with no additional controls, other than birth year fixed effects. These results suggest that Cosby exposure is associated with an increase in educational attainment among younger cohorts, compared to older cohorts, for both blacks and whites. The remaining columns, however, suggest that this finding is entirely driven by the positive pre-trends in educational attainment associated with Cosby exposure. Either controlling for pre-trends or reducing them by including Census division fixed effects results in statistically insignificant coefficients on the exposure by post coefficient (for whites) and a negative, statistically significant coefficient for blacks.

Figure 2 shows the regression results visually. To construct this picture, I group indi-

viduals into 5-year birth cohorts and run the OLS regression separately within each cohort (normalizing the correlation in the pre-Cosby sample to be zero.) The images on the left show the results for the white sample, while the images on the right show the results for the black sample. For all images, the regression results include census division, birth year and division by birth year fixed effects; the images on the bottom additionally control for pre-trends related to Cosby exposure. The images show that there is very little change in educational attainment for whites that is related to Cosby exposure. For blacks, there is a positive pre-trend, which reverses starting for cohorts born from 1970-1974.

The OLS results suggest that Cosby exposure was related to declines in educational attainment among young blacks, relative to what we would predict based on trends in older cohorts. As noted in the previous section, this result could be driven by reverse causality, with low educational propensities causing high Cosby exposure. This is why I use the instrumental variables strategy.

4.2 First stage results

Table 5 shows the results from first stage regressions of exposure and exposure interacted with "post" on Thursday NBA games and Thursday NBA games interacted with "post". The coefficient on games in the exposure regression is around -0.021 (with no Census division fixed effects) and -0.025 (with Census division fixed effects), with both coefficients significant at the 1% level. The Sanderson-Windmeijer multivariate F-statistic (a test of weak identification) is 15.3-16.3, and rejects the hypothesis that the instruments are weak.

Note that if the sample were perfectly balanced, with positive numbers in every city by birth year cell, there would be no difference in the first stage relationship between the black and white samples, or between the "pre" and "post" samples. In particular, the coefficient on "games by post" would be precisely zero in the regression with exposure as a dependent variable, and similarly for the coefficient on "games" in the regression with "exposure by post" as the dependent variable. Because the sample is not perfectly balanced, there are

slight differences in the first stage coefficients in the pre- and post- cohorts (as well as between races), which allows both coefficients to be estimated. Nonetheless, the level of variation is sufficiently low that the covariance matrix of the first stage regression becomes unstable; for this reason, the standard errors on "games" are missing for blacks in the second regression equation (with exposure by post as the dependent variable.) This does not affect the construction of either the weak instrument tests or the standard errors in the second stage regressions, however.[8]

4.3 IV results

Table 6 shows the results from an instrumental variables regression, where "exposure" and "exposure by post" are instrumented with "games" and "games by post". The dependent variable in this table is years of education. The regression specification with no additional controls suggests that Cosby exposure was associated with a large and significant increase in educational attainment among young black cohorts. For whites, this result appears to be mostly driven by pre-trends in the games variable, however. Controlling for pre-trends eliminates the result, while including Census division fixed effects (which also eliminates the pre-trend, as shown in Table 3) reduces it substantially. Doing both together results in a small positive, but statistically insignificant coefficient on the term "exposure by post".

For blacks, the results are also somewhat sensitive to the inclusion of pre-trends in the raw version of the regression (although the coefficient remains positive.) Once Census division fixed effects are included, however, the effect of Cosby exposure remains positive and significant, regardless of whether pre-trends are included or not.

Figure 3 shows the results visually. The figure is constructed in an identical manner to Figure 2, with Census division fixed effects included and the results broken down by race

[8]To see that this issue is not a problem for inference, not that an alternative way to present the results is to simply use the reduced form regression relating years of education to games and a games by post interaction. In combination with a city-level first stage regression establishing a negative relationship between games and exposure, along with the exclusion restriction, this would establish my key results. There would be no problem with constructing the standard errors in any of these equations.

and the inclusion of pre-trends. There is a slight, statistically insignificant increase in years of education associated with Cosby exposure in the white sample. This disappears once the pre-trend is included, although it is worth noting that this procedure eliminates any potential effect for the 1954-1959 cohort, which appears to be present in the top figure. As this set of cohorts was aged 25-29 when Cosby began, it is possible that this "pre-trend" is capturing a true effect. The results are less ambiguous for blacks, with a statistically significant effect present regardless of whether pre-trends are included or not.

Table 7 shows how the effects of Cosby exposure varied across the educational distribution. The first column replicates the years of education results with division fixed effects and no pre-trends, while the next four columns examine the impact on four mutually exclusive education categories: no high school, high school, some college, and bachelor's degree or more. For whites, the effect is present across the distribution. The largest effect is a reduction in the "no high school" category, but there is also relatively large (statistically insignificant) increase in college completion, with smaller increases in the middle-education categories. This is suggestive of a pattern of upgrading across the education spectrum. For blacks, the increase in education appears to be focused on the middle-to-upper end of the education spectrum. There is little change in the "no high school" category, but evidence of upgrading from high school attainment to some college or college.

5 Additional evidence and robustness

A key assumption for my empirical strategy to capture the causal effect of *The Cosby Show* is that there is no other reason why Thursday NBA games would be associated with a decline in educational attainment for cohorts born after 1960, relative to older cohorts. In this section, I present the results of two exercises that provide additional suggestive evidence that my results are working through Cosby exposure. I also examine the sensitivity of my results to alternative data choices and specifications.

5.1 Additional evidence on causal effects

The first exercise is a placebo test, examining whether Thursday night NBA games in the years before or after *The Cosby Show*'s run (1976-1983 and 1993-2000) are associated with changes in educational attainment. This directly tests the proposition that Thursday night NBA games have some causal affect not related to *The Cosby Show*. It also indirectly tests whether networks manipulated the schedule to minimize cannibalization of *The Cosby Show*, in a way that was related to changes in educational attainment. Because Thursday nights were popular television nights throughout this time period (particularly in the 1990's), networks had similar incentives to manipulate the schedule in the years before and after *Cosby* as well. To the extent that this manipulation is driving my results, I should see an "effect" of Thursday games in the pre- and post-Cosby era as well.

An important feature of the NBA schedule is that it is strongly serially correlated over time: teams that play on Thursday nights in one time period are significantly more likely to play on Thursday nights in other time periods as well. A regression of Thursday NBA games from 1984-1992 on Thursday games in the period 1976-1983 and 1992-2000 has a coefficient of 0.5 and an R^2 of 0.501. For this reason, I examine whether there is a correlation between pre/post-Cosby games and educational attainment controlling for Cosby-era games.

The third and fourth columns of Table 8 confirms that Thursday NBA games in the pre- and post-Cosby era are not related to Cosby Show ratings. The first two columns replicate the basic first-stage regressions at the city-level, while columns (3) and (4) show what happens when a measure of pre/post-Cosby era games is added to the regression. Column (4), which is my preferred specification with division fixed effects, shows that Cosby-era Thursday games continue to be negatively and significantly associated with Cosby exposure, while games in the pre/post era had no effect.

Table 9 shows that games in the pre/post era are also not associated with changes in educational attainment. The first and third columns show the baseline results in reduced form, from a regression of years of education on Cosby-era Thursday NBA games and its in-

teraction with "post" (along with Census division, birth year and division by birth year fixed effects.) The second and fourth columns show what happens when pre/post-Cosby Thursday NBA games are added to the regression. For both blacks and whites, the addition of games in the pre/post era to the regression causes the standard errors on the main coefficients (on Cosby-era games interacted with post) to rise substantially; this is due to the strong correlation between these two measures. Changes in the coefficients are suggestive, however. For whites, the coefficient on Cosby-era games by post falls from -0.002 to -0.001, which is about the same as the coefficient on pre/post-Cosby era games. For blacks, however, the coefficient for Cosby-era games remains stable at -0.003, which is approximately 4 times as large as the coefficient on pre/post games. Although the coefficient is no longer statistically significant due to the increase in the standard errors, the fact that it is little changed from the baseline regression suggests that my results are driven by something unique about Thursday games in the Cosby-era.

In a second exercise, I examine whether "conflicting" games - those that started around 7 or 8 pm, when *The Cosby Show* began - have a differential impact on Cosby exposure and on educational attainment, compared to Thursday games more generally. NBA games typically start at 7, 7:30 or 8 in the time zone in which they are played. This implies that some away games will start either later or earlier in the evening for local residents, depending on where they are played. If conflicting games have a stronger first- and second-stage effect than other games, this provides additional evidence that the effect of Thursday games is operating through *The Cosby Show*.

The last two columns of 8 shows how the first stage changes when a separate measure of conflicting games is added to the first stage regression. The coefficient on "games" captures the effect of all games, while the coefficient on "games, conflicting" captures the additional effect of games that began at 7 or 8 pm. In the version of the regression without Census division fixed effects, the effect of conflicting games is significantly different than the effect of other games, and is about three times as large. Once division fixed effects are added

to the regression, this difference shrinks and becomes only marginally significant; however, conflicting games still have an effect that is about twice as large as other games.

Table 10 examines whether the reduced form impact of NBA games on education are different for conflicting games. Although my preferred specification includes Census division fixed effects, I also show results for the specification without these controls, because there is a more strongly differential first-stage effect in this version of the regressions. (Recall that Census division fixed effects make very little difference to the estimates for the black sample.) For whites, there is no differential effect of conflicting games vs. non-conflicting games in either specification. For blacks, conflicting games have a significantly differential effect in the version without regional controls; the effect of a conflicting games is approximately twice that of a non-conflicting game. This difference disappears once division fixed effects are added, which could reflect the weaker first stage for conflicting games in this case.

5.2 Robustness

Table 11 shows that the results are robust to a variety of alternative specifications. The first column replicates the baseline IV and first stage results for the black sample. In column (2), I add the city-level covariates listed in Table 1 to the regressions, along with their interactions with birth year fixed effects. While this procedure increases the standard errors on the IV estimates, the point estimate on exposure interacted with post is identical to column (1). In the third column, I no longer exclude individuals living outside their state of birth. While the coefficient on exposure by post remains positive, it is much smaller and no longer statistically significant. This would be consistent with measurement error in the assignment of individuals to cities: because I do not observe the city in which a respondent lived as a teenager, my measure of exposure contains some error. I expect this error to be larger for individuals living outside their state of birth. In column (4), I restrict the sample to individuals aged under 60. This restriction is intended to remove any effect of selective mortality associated with higher education in the older samples. The results are quite similar when I impose this

restriction. Finally, in column (5), I include playoff games in my calculation of the number of Thursday night NBA games. The restriction to regular season games was imposed both because playoffs introduce variation in the total number of NBA games experienced in a city, and because of potential concerns that cities with exceptionally good basketball teams might be different in terms of unobservable propensities for education. As shown in the table, however, this restriction makes very little difference to the results.

6 Magnitudes

As discussed in the Data section, a key limitation of the Nielsen ratings data is that it is not demographic specific. While I can observe the average response to an NBA game among viewers in a particular city, I do not observe the first-stage response for members of my sample: young viewers who are black or white. This error in the first stage does not affect the statistical significance of my results (which is driven by the underlying relationship between Thursday NBA games and changes in educational attainment), but it does bias my estimates. To see this, recall the Wald equation for the IV estimates:

$$\hat{\beta}_{IV} = \frac{\hat{\beta}_{RF}}{\hat{\beta}_{FS}}$$

where $\hat{\beta}_{RF}$ is the coefficient from a reduced form regression of educational attainment on Thursday NBA games, and $\hat{\beta}_{FS}$ is the coefficient from the first stage. The reduced form estimates, along with the assumption of the exogeneity of Thursday games, are sufficient to identify whether Cosby had a significant impact on educational attainment. Understanding the magnitude of the effect, however, requires a precise estimate of the first stage relationship. In particular, if the NBA was more popular with young and/or black viewers, then the first stage response $\hat{\beta}_{FS}$ may be larger than what I estimate for a typical viewer. This would imply that the "per-hour" effect of Cosby estimated in my main regressions is overstated.

To correct for this source of error, I use cross-city variation in demographics to examine

how the first stage varies by race and age. For each city, I use 1980 Census data to construct the proportion of the city's population that is young (under 20), black, and young and black. I then interact these terms with the NBA games measure in the first stage regression. The results are shown in Table 12. In the first column, I add only the age interaction (along with the variable "young" capturing the proportion of the city's population that is under 20.) Cities with younger populations had a significantly larger first stage response to Thursday NBA games. Taken to the limit, the predicted first stage response in a city with only young people would be -0.461, approximately 20 times as large as the overall first stage I estimate in Table 5.

The second column of Table 12 reports the results from a regression in which Thursday NBA games are interacted with the proportion of a city's population that is black. There is no evidence that the first stage response was stronger in blacker cities; the coefficient on the interaction is positive and insignificant. This remains true if I add interactions of the games variable with "young" and "black and young."

There are two important messages to take away from Table 12. The first is that my main IV estimates appear to substantially under-estimate the true first stage response for young people, and therefore over-estimate the "per-hour" impact of *The Cosby Show*. This is reassuring, as the estimated first stage response is quite large: based on the results in column (7) of Table 6, each hour of *The Cosby Show* increased years of education by around one-tenth of a year. Reducing this response by a factor of 20 leads to a much more reasonable estimated magnitude of around 0.006 years per hour. Multiplying this by average Cosby exposure of around 10.3 hours leads to a total estimated increase of just under one-tenth of a year attributable to *The Cosby Show*. For college attainment, the same process leads to a total estimated Cosby effect of around 1 percentage point.

The second important message from Table 12 is that there appear to be no racial differences in the first stage response. The second point is important for my interpretation of the results as reflecting role-model effects. If young white people did not respond to the NBA

instrument, the lack of a relationship between NBA games and changes in educational attainment could not be interpreted as a true non-response. It appears, however, that young white people probably did respond to NBA games by reducing Cosby viewership. This suggests that there was a true difference in the response to *The Cosby Show* across racial groups.

7 Discrimination

In this section, I explore an alternative channel for my results: reduced discrimination against black workers. If *The Cosby Show* improved racial attitudes, it may have increased the return to education for young black people.

Direct evidence on racial attitudes is available from the General Social Surveys. Unfortunately, the number of respondents in each sample year is too low to permit a geographic analysis relating Cosby Show exposure directly to racial attitudes. The evidence in Table 13 suggests that the country as a whole did not see a marked change in racial attitudes during *The Cosby Show*, relative to earlier trends. The table shows time trends for two items: an indicator for whether the respondent thought the government was spending too little to improve the conditions of blacks, and an indicator for whether the respondent was against inter-racial marriage. Responses to both items showed a strong improvement in attitudes towards blacks prior to 1984, with the pace of change leveling off thereafter. Of course, the declining pace of improvement was probably related to the unusually rapid pace of growth in the 1970's. However, the table does suggest that any change in racial attitudes induced by *The Cosby Show* was, if present, offset by other factors.

An alternative measure of racial discrimination is the black-white wage gap. If the impact of *The Cosby Show* was driven by changes in racial attitudes, this should be reflected in a declining wage gap between observably similar black and white workers. In Table 14, I examine whether there are any differential changes in the black-white wage gap associated with Thursday NBA games over the period 1980 to 1990. Using the 1980 and 1990 Census

data, I construct a measure of mean log wages among 25-34 year olds for each city by race by year cell. I then run a regression of mean log wages on indicators for "black" and "1990", "Thursday NBA games", and all two- and three-way interactions of these variables. To control for educational attainment, I run this regression separately for individuals with either a high school diploma only, or a bachelor's degree. The coefficient of interest is on the the three way interaction between black, 1990, and games. This coefficient tells us whether a higher number of Thursday NBA games was associated with a smaller reduction in racial wage gaps between 1980 and 1990.

Table 14 shows the results of this regression. I see no evidence that there was any differential change in wage gaps related to Thursday night NBA games. The coefficients on the three-way interactions are insignificant and close to zero for both high school and college graduates.

The combined evidence on attitudes and wage gaps suggests that the influence of *The Cosby Show* on young black men and women did not work through reduced discrimination. Instead, I interpret my main results as reflecting role model effects.

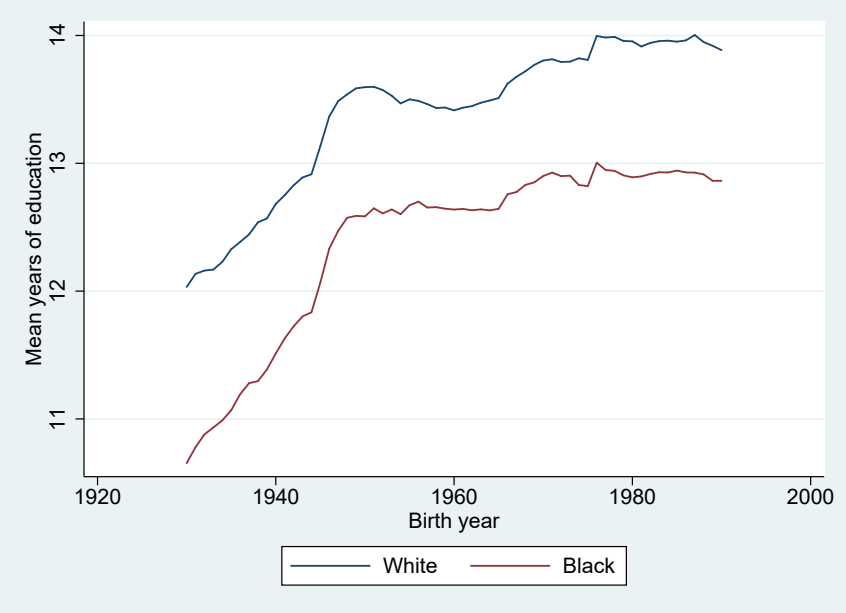
8 Conclusion

The results in this paper suggest that *The Cosby Show* had a significant influence over the educational decisions of young black men and women in the 1980's. The show did not have a significant effect on the white youth, although there is some evidence that educational attainment increased by a smaller amount for this group. The results in the white sample suggest that the increase in educational attainment was not driven by pure information effects. Given that the results do not appear to be driven by changes in white attitudes or reduced discrimination, I conclude that the results are driven by role model effects. To my knowledge, this is the first evidence causally relating media role models to the black-white education gap.

The results also suggest a potentially cheap and effective policy tool for addressing the declining education levels of young black people: the increased representation of educated minorities in the popular media. Black children watch an average of around 5 hours of television per day, slightly more than their white counterparts (Nielsen Media Inc. 2014). My results suggest that changing the television portrayals of African Americans has the potential to significantly influence these children's perceptions of the value of education.

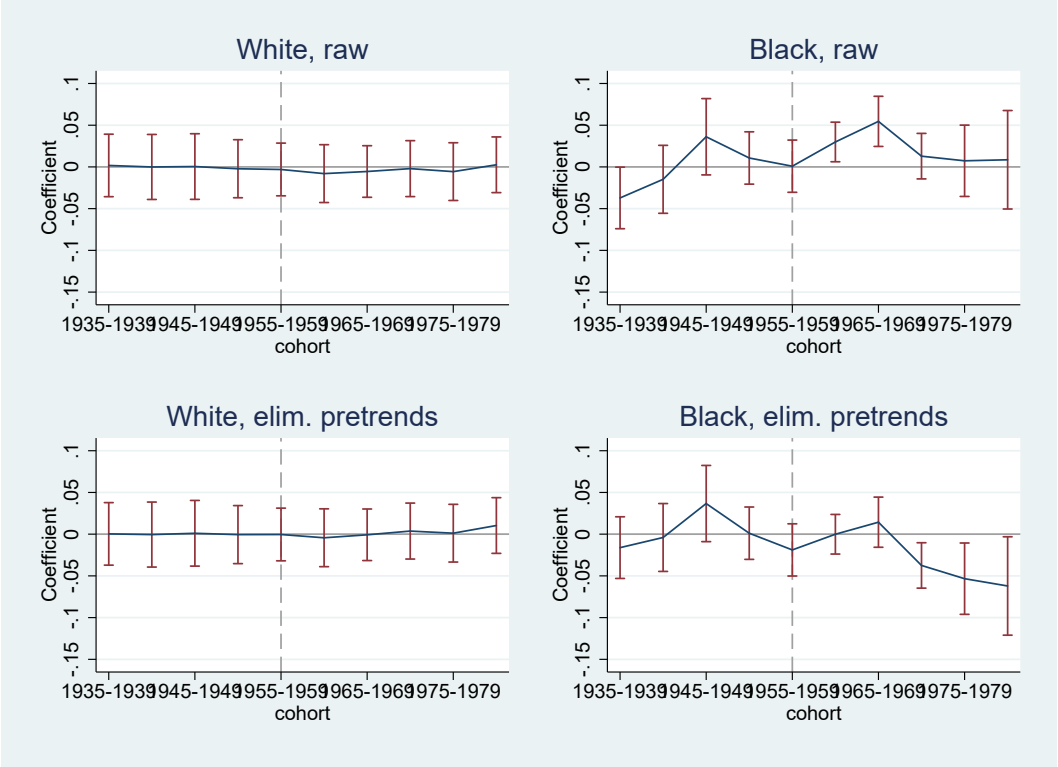
9 Figures

Figure 1: Educational attainment by cohort and race



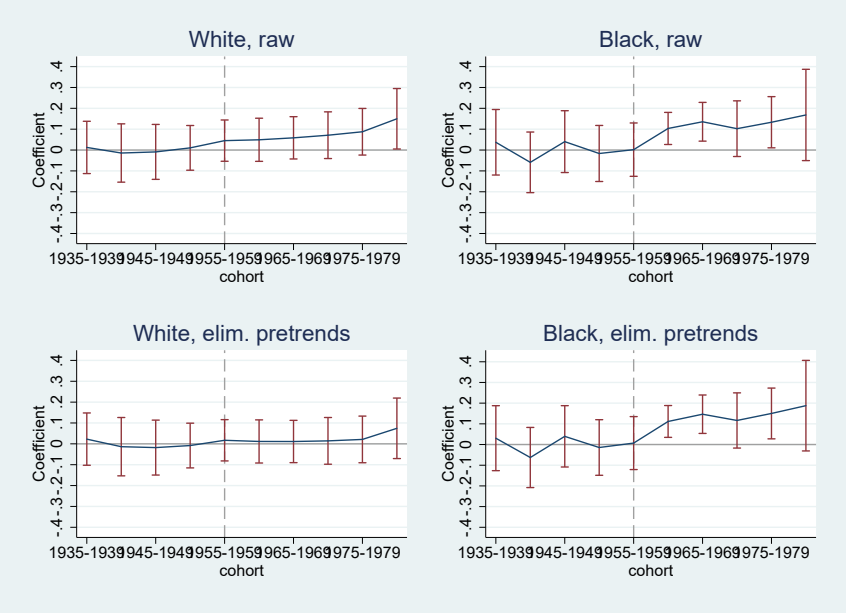
This figure shows the average years of education for individuals born in the indicated cohort (horizontal axis), separately by race.

Figure 2: OLS results, Years of education



This figure shows coefficients and confidence bands from a regression of years of education on Cosby exposure, separately by 5-year cohort. The regressions include region fixed effects, birth year fixed effects and region by birth year fixed effects.

Figure 3: IV results, Years of education



This figure shows coefficients and confidence bands from a regression of years of education on Cosby exposure, instrumented with Thursday night NBA games, separately by 5-year cohort. The regressions include region fixed effects, birth year fixed effects and region by birth year fixed effects.

10 Tables

Table 1: Summary statistics: Cosby exposure and NBA games

	Cosby exposure	Thursday NBA games
	Summary statistics:	
Mean	10.3	220.0
S.D.	2.2	34.4
Min	3.6	163.0
Max	16.8	297.0
	Regression on city covariates:	
% black	0.115***	-0.643
	(0.027)	(0.420)
% Hispanic	-0.021	0.419
	(0.027)	(0.420)
% immigrants	-0.112*	0.051
	(0.062)	(0.973)
% age under 20	0.231**	-1.690
	(0.099)	(1.547)
% age 21 to 40	-0.170**	-0.247
	(0.080)	(1.247)
% age 41 to 60	0.100	-3.922
	(0.164)	(2.557)
Log population	-0.019	-2.172
	(0.196)	(3.047)
Region, Midwest	-0.351	-51.694***
	(0.634)	(9.872)
Region, South	-0.834	-42.761***
	(0.537)	(8.364)
Region, West	-0.673	-18.868**
	(0.602)	(9.384)
N	122	122
R^2	0.459	0.456

The first panel of this table shows summary statistics for Cosby Show exposure (constructed from ratings, and measured in hours) and the number of regular season Thursday NBA games that occurred for each city over the period 1984-1992. The second panel shows a regression of these measures on city-level characteristics as measured in the 1980 Census.

Table 2: Balancing tests: Cosby exposure, Thursday NBA games, and the educational attainment of older cohorts

	Dependent variable: years of education					
	Panel 1: Cosby Exposure					
	Whites					
Cosby exposure	-0.083***	-0.029*	-0.047***	-0.032**	-0.023	-0.019
	(0.015)	(0.015)	(0.015)	(0.013)	(0.019)	(0.014)
N	122	122	122	122	122	122
R^2	0.203	0.566	0.409	0.728	0.648	0.864
	Blacks					
Cosby exposure	-0.105***	-0.066**	-0.072***	-0.080***	-0.042**	-0.050***
	(0.020)	(0.027)	(0.021)	(0.026)	(0.016)	(0.017)
N	122	122	122	122	122	122
R^2	0.174	0.272	0.387	0.414	0.862	0.881
	Panel 2: NBA Games					
	Whites					
Games	0.005***	0.002***	0.003**	0.003***	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
N	122	122	122	122	122	122
R^2	0.201	0.583	0.394	0.743	0.644	0.861
	Blacks					
Games	0.006***	0.005***	0.005***	0.005**	-0.000	-0.000
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
N	122	122	122	122	122	122
R^2	0.123	0.292	0.368	0.398	0.849	0.867
Covariates		X		X		X
Division fixed effects			X	X		
State fixed effects					X	X

This table shows the results from a regression of years of education among cohorts born from 1935-1959 (who were too old to be affected by Cosby) on Cosby exposure in Panel 1 and on Thursday NBA games in Panel 2. Covariates include all variables shown in Table 1 except for the region indicators; these are replaced, where indicated, by Census division or state fixed effects.

Table 3: Pre-trends in Cosby exposure and the instrument

	Dependent variable: years of education							
	Panel 1: Exposure							
	Whites				*Blacks*			
Exposure	-2.644**	1.667	0.533	1.179	-8.003***	-2.084	-4.479**	-1.768
	(1.211)	(1.646)	(0.890)	(0.852)	(2.286)	(2.653)	(2.233)	(2.726)
Exposure x trend	0.001**	-0.001	-0.000	-0.001	0.004***	0.001	0.002**	0.001
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
N	3,075	3,075	3,075	3,075	2,851	2,851	2,851	2,851
R^2	0.466	0.695	0.612	0.801	0.242	0.332	0.335	0.406
	Panel 1: Games							
	Whites				*Blacks*			
Games	0.513***	0.409***	0.098	0.065	0.052	0.049	-0.070	-0.202
	(0.065)	(0.076)	(0.085)	(0.077)	(0.171)	(0.169)	(0.205)	(0.187)
Games x trend	-0.000***	-0.000***	-0.000	-0.000	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	3,075	3,075	3,075	3,075	2,262	2,262	2,262	2,262
R^2	0.497	0.714	0.613	0.812	0.211	0.332	0.322	0.402
Birth year FE	X	X	X	X	X	X	X	X
Covariates		X		X		X		X
Covariates x birth year FE		X		X		X		X
Division FE			X	X			X	X
Division x birth year FE			X	X			X	X

This table shows the results from a regression of years of education, measured at the city by birth year level, on Cosby exposure and the interaction of Cosby exposure with birth year, using cohorts born from 1935-1959. Regressions include the indicated set of fixed effects/covariates, and standard errors are clustered at the city level.

Table 4: OLS results

	Dependent variable: years of education							
	Whites				*Blacks*			
Exposure	-0.079***	0.000	-0.043**	0.000	-0.091***	0.000	-0.066***	0.000
	(0.016)	(0.016)	(0.018)	(0.018)	(0.013)	(0.013)	(0.013)	(0.013)
Exposure x Post	0.023**	-0.014	-0.003	0.002	0.054***	-0.040**	0.023**	-0.027**
	(0.011)	(0.011)	(0.010)	(0.010)	(0.012)	(0.012)	(0.011)	(0.011)
N	6,079	6,079	6,079	6,079	5,738	5,738	5,738	5,738
R^2	0.505	0.347	0.593	0.657	0.209	0.046	0.292	0.451
Birth year FE	X	X	X	X	X	X	X	X
Division FE			X	X			X	X
Division by birth year FE			X	X			X	X
Elim. pretrends		X		X		X		X

This table shows the results from a regression of years of education, measured at the city by birth year level, on Cosby exposure and the interaction of Cosby exposure with "Post", an indicator for being born after 1960. Regressions include the indicated set of fixed effects, and standard errors are clustered at the city by post level. Where indicated, pre-trends are eliminated by first running a regression of years of education on exposure and an exposure-specific trend in the 1935-1959 sample, and using the residuals as the dependent variable in the main regressions.

Table 5: First stage results

	White sample				Black sample			
	Dependent variable:							
	Exposure	Exp. x Post	Exposure	Exp. x Post	Exposure	Exp. x Post	Exposure	Exp. x Post
Games	-0.021***	-0.000	-0.026***	0.000	-0.020***	0.000	-0.025**	0.000
	(0.005)	(.)	(0.009)	(0.000)	(0.006)	(.)	(0.010)	(0.000)
Games	0.000*	-0.021***	0.001	-0.025***	-0.001	-0.021***	-0.000	-0.025***
x Post	(0.000)	(0.005)	(0.001)	(0.009)	(0.001)	(0.006)	(0.001)	(0.009)
Birth year FE	X	X	X	X	X	X	X	X
Region FE			X	X			X	X
Birth year x Region FE			X	X			X	X
SW F-statistic	15.3***	17.0***	16.3***	8.5***	12.0***	15.0***	15.0***	8.2***

This table shows the results of the first-stage regressions of Cosby exposure and Cosby exposure by post (where "post" indicates cohorts born after 1960) on Thursday NBA games and Thursday NBA games by post. Regressions include the indicated set of fixed effects, and standard errors are clustered at the city level. The reported F-statistic is the Sanderson-Windmeijer multivariate F-test.

Table 6: IV results for years of education

	Dependent variable: years of education							
	Whites				*Blacks*			
Exposure	-0.244***	0.000	-0.128**	0.000	-0.190***	-0.000	-0.143***	0.000
	(0.066)	(0.066)	(0.060)	(0.060)	(0.060)	(0.060)	(0.051)	(0.051)
Exposure x post	0.228***	-0.074	0.071*	0.024	0.160***	0.085	0.126**	0.140**
	(0.062)	(0.062)	(0.038)	(0.038)	(0.057)	(0.057)	(0.058)	(0.058)
N	6,079	6,079	6,079	6,079	5,738	5,738	5,738	5,738
Birth year FE	X	X	X	X	X	X	X	X
Region FE			X	X			X	X
Region by birth year FE			X	X			X	X
Elim. pretrends		X		X		X		X

This table shows the results from an IV regression of years of education (measured at the city by birth year level) on Cosby exposure and exposure interacted with "post", an indicator for being born after 1960. Exposure and exposure by post are instrumented with Thursday NBA games and Thursday NBA games interacted with the post indicator. Regressions include the indicated set of fixed effects, and standard errors are clustered at the city level. Where indicated, pre-trends are eliminated by first running an IV regression of years of education on exposure and an exposure-specific trend in the 1935-1959 sample, and using the residuals as the dependent variable in the main regressions.

Table 7: IV results across the educational distribution

	Dependent variable:				
	Years of education	No high school	High school	Some college	College
			Whites		
Exposure	-0.128**	0.017***	0.004	-0.007*	-0.014*
	(0.060)	(0.006)	(0.007)	(0.004)	(0.008)
Exposure by post	0.071*	-0.011**	0.002	0.002	0.006
	(0.038)	(0.005)	(0.005)	(0.003)	(0.006)
N	6,079	6,079	6,079	6,079	6,079
			Blacks		
Exposure	-0.143***	0.014**	0.012	-0.007	-0.019**
	(0.051)	(0.006)	(0.009)	(0.007)	(0.009)
Exposure by post	0.126**	-0.007	-0.020*	0.008	0.018*
	(0.058)	(0.007)	(0.012)	(0.008)	(0.010)
N	5,738	5,738	5,738	5,738	5,738
Birth year FE	X	X	X	X	X
Region FE	X	X	X	X	X
Region by birth year FE	X	X	X	X	X

This table shows the results from an IV regression of the indicated dependent variables (measured at the city by birth year level) on Cosby exposure and exposure interacted with "post", an indicator for being born after 1960. Exposure and exposure by post are instrumented with Thursday NBA games and Thursday NBA games interacted with the post indicator. Regressions include the indicated set of fixed effects, and standard errors are clustered at the city level.

Table 8: Placebo tests, first stage

	Dependent variable: Cosby exposure					
Games	-0.021***	-0.026***	-0.033***	-0.032***	-0.019***	-0.025***
	(0.006)	(0.008)	(0.008)	(0.010)	(0.005)	(0.007)
Games, pre/post			0.012**	0.008		
			(0.005)	(0.008)		
Games, conflicting					-0.027***	-0.022*
					(0.010)	(0.012)
Region FE		X		X		X
N	122	122	122	122	122	122
R^2	0.110	0.286	0.140	0.292	0.167	0.308

This table shows the first stage results (run at the city level), using different measures of Thursday NBA games. The main variable, "games" is a city-level count of all Thursday night NBA games occurring during the period 1984-1992. The variable "Games, pre/post" is a city-level count of all Thursday NBA games occurring during the periods 1976-1983 and 1992-2000 (excluding the 1998/1999 season, which was shorter because of strike.) The variable "games, conflicting" is a count of Thursday night NBA games occurring during the period 1984-1992 that I estimate to have started between 7-9 pm. Some Thursday night games do not meet this restriction because they are played in different time zones.

Table 9: Placebo test: pre- and post-Cosby era NBA games

	Dependent variable: years of education			
	Whites		*Blacks*	
Games	0.003**	0.003*	0.004**	0.003*
	(0.002)	(0.002)	(0.001)	(0.002)
Games x Post	-0.002***	-0.001	-0.003***	-0.003
	(0.001)	(0.001)	(0.001)	(0.002)
Games, pre/post		0.000		0.001
		(0.001)		(0.001)
Games, pre/post		-0.001		-0.001
x Post		(0.001)		(0.001)
Birth year FE	X	X	X	X
Region FE	X	X	X	X
Birth year x Region FE	X	X	X	X
N	6,079	6,079	5,738	5,738
R^2	0.581	0.581	0.280	0.280

The first and third columns of this table shows reduced form regressions of years of education (measured at the city by birth year level) on Thursday NBA games and Thursday NBA games interacted with "post", an indicator for being born after 1960. The second and fourth columns show the same regression, but also include a count of Thursday NBA games occurring from 1976-1983 and 1992-2000, and the same variable interacted with "post". Regressions include the indicated set of fixed effects, and standard errors are clustered at the city level.

Table 10: Effect of conflicting vs. non-conflicting NBA games

	Dependent variable: years of education			
	Whites		*Blacks*	
Games	0.005***	0.003**	0.004***	0.003**
	(0.001)	(0.002)	(0.001)	(0.001)
Games x Post	-0.005***	-0.002***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
Games, conflicting	0.003**	0.001	0.002	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Games, conflicting	-0.001	0.002	-0.003**	-0.000
x Post	(0.001)	(0.001)	(0.001)	(0.001)
Birth year FE	X	X	X	X
Region FE		X		X
Region x birth year FE		X		X
N	6,079	6,079	5,738	5,738
R^2	0.493	0.583	0.190	0.280

This table shows reduced form regressions of years of education (measured at the city by birth year level) on Thursday NBA games and Thursday NBA games interacted with "post", an indicator for being born after 1960. The variable "Games" includes a count of all Thursday NBA games occurring over the period 1984-1992; the variable "Games, conflicting" includes the number of these games that I estimate to have started between 7-9 pm. Regressions include the indicated set of fixed effects, and standard errors are clustered at the city level.

Table 11: Robustness to alternative specifications

	Baseline	Add covariates	Incl. movers	Age restriction	Playoff games
	(1)	(2)	(3)	(4)	(5)
	Dependent variable: years of education				
Exposure x post	0.126**	0.126	0.049	0.136**	0.133**
	(0.058)	(0.077)	(0.038)	(0.065)	(0.060)
	Dependent variable: exposure				
Games	-0.025**	-0.017**	-0.026**	-0.024**	-0.023**
	(0.010)	(0.007)	(0.009)	(0.010)	(0.012)
N	5,738	5,738	6,003	5,682	5,738

The first column of this table shows the main first stage and IV results for the black sample, using years of education as the dependent variable. In the remaining six columns, I show the same results using alternative sample constructions. In column (2), I add the city-level covariates shown in Table 1, as well as interactions of these variables with birth year fixed effects. In column (3), I no longer restrict the sample to individuals living in their state of birth. In column (4), I eliminate individuals over the age of 60 to control for selective mortality. In column (5), my games measure includes playoff games. All regressions include division, birth year, and division by birth year fixed effects; standard errors are clustered at the city level.

Table 12: First stage by city demographics

	Dependent variable: Cosby exposure		
Games	0.206***	-0.023**	0.113
	(0.073)	(0.010)	(0.085)
Games x Young	-0.668***		-0.404
	(0.208)		(0.251)
Games x Black		0.029	-1.265
		(0.057)	(0.936)
Games x Black and Young			2.902
			(2.120)
N	122	122	122
R^2	0.379	0.407	0.488
Division FE	X	X	X
Implied coefficient, white	-0.461***	-0.023**	-0.291*
Implied coefficient, black	-0.461***	0.006	1.345

This table shows the first stage regressions, run at the city level, when the Thursday NBA games variable is interacted with city demographics, measured in the 1980 Census. The variable "young" indicates the proportion of a city's population that was under 20 in 1980. The regression includes Census division fixed effects, as well as the proportion of the population that is black, young and/or black and young where appropriate.

Table 13: Time trends in racial attitudes

	Dependent variable:	
	Government spending too little to help blacks	Against inter-racial marriage
	(Proportion agreeing: time trend)	
1972-1984	0.007***	-0.024***
1985-1992	0.004***	-0.015***
1992-2002	-0.001**	-0.007***

This table shows the time trends in two measures of racial attitudes from the 1972-2002 General Social Surveys. $*$ denotes statistical significance at the 10% level, $**$ denotes statistical significance at the 5% level and $***$ denotes statistical significance at the 1% level.

Table 14: Changes in racial wage gaps, 1980-1990

	Dependent variable: average log wages	
	High school only	College degree
Black	-0.148	-0.145
	(0.222)	(0.184)
1990	0.073	0.212
	(0.222)	(0.182)
Games	-0.000	-0.001
	(0.001)	(0.001)
Black x 1990	-0.030	-0.297
	(0.318)	(0.262)
Games x 1990	-0.000	-0.000
	(0.001)	(0.001)
Black x Games	0.000	0.000
	(0.001)	(0.001)
Black x Games x 1990	-0.001	0.001
	(0.001)	(0.001)
Division FE	X	X
N	475	473
R^2	0.305	0.115

This table shows the results from a regression of mean log wages for 25-34 year olds in the 1980 and 1990 Census samples (constructed at the city by race by year level) on indicators for "black" and "1990", Thursday NBA games and all two- and three-way interactions of these variables. To control for education, I restrict the sample to individuals with exactly a high school diploma or college degree, and run the regressions separately for each of these two groups. The coefficient of interest is on the variable "Black x Games x 1990", which indicates whether the racial wage gap changed differentially in cities with more Thursday NBA games. The regressions also include Census division fixed effects.

References

Autor, David, and Melanie Wasserman. 2013. "Wayward Sons: The Emerging Gender Gap in Labor Markets and Education." *Third Way*.

Clampet-Lundquist, Susan, Kathryn Edin, Jeffry R. Kling, and Greg J. Duncan. 2011. "Moving Teenagers Out of High-Risk Neighborhoods: How Girls Fare Better than Boys." *American Journal of Sociology*, 116: 1154–1189.

Dee, Thomas S. 2004. "Teachers, Race and Student Achievement In a Randomized Experiment." *The Review of Economics and Statistics*, 86(1): 195–210.

Dee, Thomas S. 2005. "A Teacher Like Me: Does Race, Ethnicity, or Gender Matter?" *American Economic Review*, 95(2): 158–165.

Dee, Thomas S. 2007. "Teachers and the Gender Gaps in Student Achievement." *Journal of Human Resources*, 42(3): 528–554.

Eliana La Ferrara, Alberto Chong, and Suzanne Duryea. 2012. "Soap Operas and Fertility: Evidence from Brazil." *American Economic Journal: Applied Economics*, 4: 1–31.

Evans, William N., Craig Garthwaite, and Timothy J. Moore. 2012. "The White/Black Educational Gap, Stalled Progress and the Consequences of the Emergence of Crack Cocaine Markets." *NBER Working Paper*, No. w18437.

Fairlie, Robert W., Florian Hoffman, and Philip Oreopoulos. 2014. "A Community College Instructor Like Me: Race and Ethnicity Interactions in the Classroom." *American Economic Review*, 104(8): 2567–2591.

Goldin, Claudia, and Lawrence F. Katz. 2008. *The Race Between Education and Technology*. Cambridge, MA:Harvard University Press.

Jensen, Robert, and Emily Oster. 2009. "The Power of TV: Cable Television and Women's Status in India." *Quarterly Journal of Economics*, 124: 1057–1094.

Jhally, Sut, and Justin Lewis. 1992. *Enlightened Racism: The Cosby Show, Audiences and the Myth of the American Dream.* Oxford, UK:Westview Press.

Kearney, Melissa S., and Phillip B. Levine. 2015*a*. "Early Childhood Education by MOOC: Lessons from Sesame Street." *NBER Working Papers*, No. 21229.

Kearney, Melissa S., and Phillip B. Levine. 2015*b*. "Media Influences on Social Outcomes: The Impact of MTV's 16 and Pregnant on Teen Childbearing." *American Economic Review*, 105(12): 3597–3632.

Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2007. "Experimental Analysis of Neighborhood Effects." *Econometrica*, 75: 83–119.

Martin, John. 1989. "Keeping 'Cosby' Honest Pyschiatrist Alvin F. Poussaint Has a Say in Every Script." *Providence Journal*.

Mattabane, Paula Whatley, and Bishetta D. Merrit. 2014. "Media Use, Gender and African American College Attendance: The Cosby Effect." *Howard Journal of Communications*, 25: 452–471.

Nielsen Media Inc. 2014. "An Era of Growth: The Cross Platform Report."

O'Connor, John J. 1985. "TV Reviews: Bill Cosby's Triumph." *New York Times*, C.30.

Rosenkoetter, Lawrence I. 2009. "The Television Situation Comedy and Children's Prosocial Behavior." *Journal of Applied Social Psychology*, 29: 979–993.

Ruggles, Steven, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. 2015. "Integrated Public Use Microdata Series: Version 6.0." Machine-Readable Database, University of Minnesota: Minneapolis, MN.

Shales, Tom. 1984. "NBC's 'Cosby', the Pick of the Fall Crop." *Washington Post*, C.1.

Television Bureau of Advertising. 2012. "TV Basics."