Casting roles, casting votes: Lessons from Sesame Street on media representation, racial biases, and voting

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Abstract

Evidence on the media's potential to reduce prejudice is limited. *Sesame Street*'s positive representation of minority characters and working women was distinctive in the media landscape of 1969. Using age and technological variation in broadcast reception, we show that *Sesame Street* increased political engagement and reduced prejudice decades later. Exposed cohorts in high coverage counties are 4.2 ppts more likely to vote, have lower measures of racial biases, and report more votes for minority and women candidates to the U.S. House by 8.1 and 5.8 ppts respectively. On ballots featuring white men, turnout gains are split between parties. JEL Codes: J15, J16, L82, D72.

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1 Introduction

Can child mass media reduce prejudices and racial biases in adulthood? Can it increase people's willingness to vote for diverse representatives? Women and minority groups in the U.S. have often been underrepresented in media roles or depicted through negative stereotypes. Concern has been raised in the popular press and scientific community about the long-run impact of these patterns. Consequently, much research has focused on mass media's detrimental effects, leaving a gap in our understanding of its potential to *reduce* prejudice in the long-run, particularly when exposure occurs in childhood. Media instruments, such as television, can present great opportunity, with the potential to impact large populations, increasing their exposure to, knowledge of, and respect for diverse groups. This is particularly true for young children, as their understanding of race and gender is still developing. For many young kids, *Sesame Street*, which took the nation by storm when it started airing in 1969, was a window into a very different America than what they saw in their daily life and on other television programs.

In this paper, we break new ground by showing that child media can cause a long-run reduction in prejudice and increase voters' willingness to vote for diverse candidates–a consequential outcome. We study the effects of exposure to *Sesame Street*, a children's educational television show that portrayed an inclusive, egalitarian and diverse America. We examine impacts on voting, a critical context where preferences for political candidates can be influenced by candidate demographics. We follow the difference-in-differences strategy used by Kearney and Levine (2019) in their study of the show's educational effects. This approach compares cohort differences between counties with high and low broadcast coverage, as cohorts who were in their preschool years in 1969, when the show started airing, watched substantially more *Sesame Street* than slightly older cohorts. We apply this approach to our novel and socially important questions, examining new data compiled on candidate demographics, a large election survey, and an online survey of racial biases. We find that exposure to *Sesame Street* impacted elections in two distinct ways. First, *Sesame Street* coverage increased political knowledge and engagement, leading to higher voter turnout both when these cohorts were young voters and later in life. We estimate that turnout rates of exposed cohorts are 4.2 percentage points higher in high-coverage counties. Second, *Sesame Street* shaped voter preferences in adulthood. Younger cohorts in high coverage counties report more votes for minority and women candidates for the U.S. House of Representatives by 8.1 and 5.8 percentage points respectively. As a result, they report more votes for Democrats who are more likely to be women and minorities. In elections featuring two white men, turnout gains are split between parties. We find little evidence that the show changed policy views or political identities in a way that favored one party over another. Like Kearney and Levine (2019), we also find no impacts on long-run educational attainment or income. Instead, we find that *Sesame Street* coverage reduced measures of white racial bias against Blacks, and increased awareness of gender biases, suggesting that a long-run reduction in biases explains the differences in candidate choice. These findings provide needed causal evidence on the impacts of race and gender representation in child media, a topic of concern to parents and policy makers but one for which empirical evidence is scarce.

It is hard to overstate the cultural impact *Sesame Street* had in the United States. First airing in November 1969, the show rapidly became extremely popular. It is estimated that in the early 1970s over half of kids between the ages of 2 and 5 watched the show in the previous week if they had the technological capability to do so (Kearney and Levine (2019)). For many kids, the show became an hour-long daily routine.[1] The show was strikingly different from contemporaneous children's programming.[2] In addition to its novel focus on educational content, *Sesame Street* portrayed a thriving, civically engaged, and racially diverse urban community modeled off of New York's African-American neighborhoods. The show modeled cooperative behavior, community engagement, and respect for diversity–traits often viewed as foundational to civic participation later in life. Its human cast was exceptionally diverse from the outset, and all human characters,

[1]Early *Sesame Street* episodes lasted 60 minutes with 130 episodes per season. Re-runs were also common.

[2]*Mr. Rodgers' neighborhood*, began in 1968. Early cast members were European-American, except for Officer Clemmons, an African-American police officer. He appeared in two episodes in August 1968 and in 15% of episodes between 1969 and 1972. *Captain Kangaroo* was a popular children's show from 1955 to 1984 that aired on CBS, which had different broadcasting patterns. Mr. Baxter, its first regular Black character, made limited appearances starting in 1968.

including women, held jobs.[3] This choice of cast and setting was deliberate. The show was designed to educate young kids, particularly from lower income groups (Cooney (1967)). Input was sought from experts and practitioners in education, child development, psychology and the arts, including Dr. Chester Pierce, an expert on the psychiatric consequences of racism, and the effects of television's portrayals of minorities (Harrington (2019)). Dr. Pierce played an important role in defining the affective skills that the show sought to encourage in its viewers such as improving children's self-image and racial tolerance (Long (1973), Fisch et al. (1999), Lesser (1974)). Thus, *Sesame Street* portrayed an urban integrated community in a positive light, with Black actors cast as role models and figures of authority, working married women, friendly and egalitarian interactions between cast members of different races and between adults, children, and Muppets.[4] For many kids, *Sesame Street* was their first glimpse at a diverse America, allowing them to form strong bonds with fictional characters whose race did not always match their own.[5] Diverse, egalitarian media portrayals are a choice available to parents and children in today's fragmented media landscape. In the communal television culture of 1969, *Sesame Street* was a radical change that introduced diverse, egalitarian children's media to an environment with no prior exposure. As such, the introduction of *Sesame Street* provides an ideal setting to study the extensive-margin effects of exposure to diversity in children's media.

Recognition that biases and prejudices carry important social and economic costs motivates an extensive literature in child development, psychology, and economics, aiming to understand and mitigate prejudice (Lang and Spitzer (2020), Rutland and Killen (2015),

[3]Human characters in season 1 were nurse Susan (African-American); history teacher Gordon (African-American); music teacher Bob (European-American); and shopkeeper Mr. Hooper (European-American). Law student David (African-American); and librarian Linda (deaf European-American) joined in season 2. Season 3 added Fix-It shop owner Luis (Mexican-American); and librarian Maria (Puerto Rican).

[4]These groundbreaking choices were controversial. In May of 1970, the Mississippi State Commission for Educational Television voted to ban the airing of *Sesame Street* on the state's educational television station. Public backlash against this decision led to the show's rapid reinstatement (Greene (2019)).

[5]When long time cast member Emilio Delgado passed, memorial Twitter posts often expressed that he was their first exposure to Hispanic culture. Loretta Long who played Susan, recalled of a 1970 cast trip to Mississippi that *"Little white kids would reach out to kiss me or 'Gordon,' the other Black character, and you could see their mothers were uneasy. But they'd loosen up, because how can you hate someone who makes your child so happy?"*

Paluck et al. (2021), Bertrand and Duflo (2017)). Much of this work is guided by contact theory, the idea that prejudices can be reduced by interacting and cooperating with equal status and common goals (Allport et al. (1954)). Field and natural-experiments have generally found that interpersonal contact in schools or through sporting activities reduces in-group bias in the short and medium-run (Paluck et al. (2019), Corno et al. (2022), Boisjoly et al. (2006), Carrell et al. (2019), Rao (2019), Mousa (2020), Lowe (2021)). Only a few recent papers have examined the long-run effects of interpersonal childhood contact finding impacts on political preferences and neighborhood choice (Billings et al. (2021), Brown et al. (2021), Merlino et al. (2022)). In this paper we show that positive contact via children's media can also induce long-run effects on voting and racial biases.

Media exposure can affect a variety of social attitudes (DellaVigna and La Ferrara (2015)). Whether media contact with celebrity role-models or out-group characters can impact prejudices has been less studied, especially in the long-run (Paluck et al. (2021)). A few papers show that famous minority role models, like President Obama or soccer star Mohamed Salah, reduced short-run indicators of prejudice in adults (Plant et al. (2009), Marble et al. (2021)). Though a large literature has studied role-model impacts on the aspirations and education of underrepresented groups (Dee (2005), Bettinger and Long (2005), Eble and Hu (2020)), evidence on how minority role models impact majority biases is limited (Kearney and Levine (2020), Bertrand and Duflo (2017)). A growing literature is exploring the media's role in worsening racial and ethnic tensions (Wang (2021), Ang (2023), Adena et al. (2015), Yanagizawa-Drott (2014)). Few papers explore media's potential to improve these outcomes. Paluck (2009) and Blouin and Mukand (2019) show that radio soap operas in Rwanda reduced inter-group prejudice; Armand et al. (nd) shows that U.S. radio broadcasts in the 1940s of *The Adventures of Superman* increased racial tolerance; and Jensen and Oster (2009) shows the important effects TV had on gender attitudes in India. This paper contributes to this space by showing that representation in children's television reduced biases and impacted voting in the very long-run.

Though not previously documented, there are reasons to believe *Sesame Street* could have these impacts. Biases are thought to be more malleable in early childhood as responsiveness to race and gender is still evolving and can be shaped by children's environment.[6] Indeed, evidence suggests that shows like *Sesame Street* can reduce race and gender prejudices in the short-run, hinting at the possibility of long-run effects (Cole et al. (2003), Mares and Pan (2013), Vittrup and Holden (2011)).[7] Because children may be especially sensitive to media content, representations of race and gender in child media are a contentious topic for policy makers and parents. Yet despite frequent discussions in the popular press on how minority and gender representations in children's media impacts kids, this paper is the first to causally identify long-run impacts. Existing economic research on children's media is limited, and has generally focused on education and human capital outcomes. In their examination of *Sesame Street*'s impacts on academic and labor market outcomes, Kearney and Levine (2019) find that the show generated important improvements in students' grade-for-age during their school years, though long-term effects on educational attainment and wages were small and inconclusive.[8] Other work on child media's human capital impacts has found a range of effects due to differences in content and substitutions in time use.[9] In this paper we show that children's media has long-run effects on outcomes other than human capital by impacting electoral participation through modeling of civic behaviors and bias formation through representations of minorities and gender roles. Our findings complement the work by Adukia et al. (2023) who document how minorities and females are underrepresented in prize winning children's books and show that representation in the children's books consumed in local communities correlates

[6]Behavioral differences towards own- and other-race faces begin to develop in infancy. Most children correctly identify race by ages 2 to 3. By ages 3 to 5, explicit in-group favoritism in white children is common, peaking at ages 6 or 7 and declining thereafter, likely as children respond to social norms against racial bias. The development of explicit prejudices in majority-group children correlates with environments where contact with the minority group is low (Raabe and Beelmann (2011), Hughes et al. (2023)).

[7]Cole et al. (2003) look at the impact of two *Sesame Street* inspired shows designed by the Children's Television Workshop (CTW) for broadcast in Israel and Palestine. They find improved out-group attitudes for Israeli-Jewish preschoolers after 4 months of exposure. In an international meta-analysis of *Sesame Street* using CTW generated data, Mares and Pan (2013) find positive short-run impacts on attitudes towards social differences. Vittrup and Holden (2011) find positive effects on children's out-group attitudes shortly after having children watch videos that featured positive relationships between a racially diverse cast. Two of the five videos used were excerpts from *Sesame Street*.

[8]Early studies in psychology and education as well as impact evaluations commissioned by producers and governments generally conclude that *Sesame Street* was effective at teaching its targeted curriculum and had positive effects on prosocial behaviors (Fisch et al. (1999), Fisch and Truglio (2014), Murphy (1991)).

[9]These include positive (Gentzkow and Shapiro (2008), Riley (2019)) and negative (Hernæs et al. (2019), Durante et al. (2019)) impacts.

with local politics. Adukia et al. (2023) substantially increased our understanding of the media context in which children develop. The natural experiment we study, examining the launch of one of the most widely watched children's television shows, allows us to consider the implications of the patterns they document as we can identify the causal effects of exposure to representation on prejudice and voting in later life.

Finally, these findings also contribute to the literature examining the media's impacts on voter preferences and turnout (Campante et al. (2022)). Turnout effects likely depend on whether a media source increases or reduces exposure to political information (DellaVigna and Kaplan (2007), Oberholzer-Gee and Waldfogel (2009), Gentzkow (2006), Falck et al. (2014), Ellingsen and Hernæs (2018)). Existing work has focused on adult news media with the exception of Durante et al. (2019) who find that light entertainment TV in Italy in the 1980s increased populist voting, especially for those exposed in childhood. They hypothesize that exposure reduced political knowledge. In contrast, we document for the first time an increase in political knowledge and voter turnout as a result of exposure to children's media that modeled civic participation and engagement. Our contrasting findings highlight the importance of considering media content, even in the analysis of children's media.

The paper is organized as follows. Section 2 presents the data. Section 3 details our empirical strategy. Section 4 documents how *Sesame Street* coverage increased electoral participation. Section 5 examines how *Sesame Street* coverage shaped respondents' preferences over candidates. Section 6 examines which candidate attributes explain changes in voter behaviors. Section 7 shows impacts on racial biases. Section 8 concludes.

2 Data

Reported voting behavior and ballot composition: This paper uses post-election survey responses from the Cooperative Congressional Election Study (CCES) in election years 2006-2020.[10] The CCES surveys respondents on policy views and self-reported voting

[10]Survey questions vary from year to year. Kuriwaki (2021) harmonized key variables and generated harmonized weights for the cumulative 2006-2020 dataset that are used in all estimations.

behavior. It is widely used in economics and political science as a source of individual data on political preferences (Arteaga and Barone (2025), Cantoni and Pons (2022), Djourelova (2023), Ansolabehere and Kuriwaki (2022)). Though policy views and candidate choice are necessarily self-reported given the secrecy of the ballot box, the CCES data does validate voter registration and turnout.[11] Our main sample covers 57,221 responses from non-immigrant citizens born between 1959 and 1968, who faced 2,957 distinct *major party ballots* for their U.S. House Representative.[12] We define *major party ballots* as those where the two front-runners are a Democrat and a Republican, both of whom receive over 5% of their district's vote.[13] These ballots constitute 87% of ballots for the U.S. House between 2006 and 2020. We collected data on the race and gender of the two front-running candidates.[14] Of the 5,914 candidacies in our data, 522 (8.8%) are election runs by Black candidates, 320 (5.4%) are by non-Black Hispanics, 4,891 (82.7%) are by non-Hispanic whites, and 181 (3.1%) are by candidates of other races and ethnicities. 1,288 candidacies (21.8%) are election runs by women. 689 major party ballots feature a non-Hispanic white candidate running against a minority candidate and 1,034 feature a woman running against a man. Table A2 of the appendix provides a full picture of ballot composition by major party candidate demographics, and reports summary statistics for election results by ballot composition. In addition to the CCES data, we also examine data from the Current Population Survey (CPS) voting and registration supplement as a second source of data on self-reported turnout and registration.

[11]Registration and voting are validated by matching the CCES data through Catalist LLC., a political data vendor. Their matching procedure is validated in Ansolabehere and Hersh (2012). They find that Catalist correctly identifies 94% of voters, 96% of non-voters, and a respondent's race in 99.9% of cases.

[12]Responses from Washington DC and those missing information on *Sesame Street* coverage or ballot demographics are omitted. In election years, respondents are contacted both prior to, and shortly after, election day. The main sample covers individuals who complete the post-election survey. Exposure to *Sesame Street* is not associated with post-election survey completion (appendix Table A1).

[13]Election statistics are published by the clerk of the U.S. House of Representatives and are available from MIT Election Data and Science Lab (2021).

[14]Candidates were categorized into four mutually exclusive racio-ethnic categories (Black, non-Black Hispanic, other, non-Hispanic white) and two gender categories (man, woman). Demographic information is observed for both candidates on over 99% of ballots. Information for candidacies between 2006 and 2014 is from Fraga and Hassell (2021); data for the 2018 and 2020 elections is from OpenSecrets; and data for women candidates was made available by the Center for American Women and Politics (CAWP). Data on 2,359 additional candidacies was compiled by research assistants.

Race and gender biases: Data on race and gender biases comes from Harvard's *Project Implicit*, an online platform that makes implicit association tests (IATs) publicly available. Anyone can complete a survey and take an IAT online.[15] IATs are psychological tests that have been used in psychology and economics to measure respondents' implicit associations towards individual characteristics (Glover et al. (2017), Carlana (2019), Corno et al. (2022), Lowes et al. (2015)).[16] Our data covers the 350,080 race IAT and 84,479 gender-career IAT scores of U.S. resident citizens born between 1959 and 1968.[17]

***Sesame Street* coverage:** The predicted share of households in a county that could watch *Sesame Street* at home in 1969 was estimated in Kearney and Levine (2019) by using the 1968–1969 edition of the trade publication, *TV Factbook*. This lists all broadcasting television stations, their technical and geographical specifications (UHF or VHF signal, signal power, location, and height of the tower) and, for commercial stations only, the coverage rates for surrounding counties.[18] Commercial stations were used to estimate the relationship between a station's technical specifications and county coverage rates. The relationship was then applied to the non-commercial stations broadcasting *Sesame Street* to generate simulated coverage rates. Counties were then assigned the coverage rate of the best simulated signal received from surrounding towers. Figure 1a maps the Kearney and

[15] *Project Implicit* data is not representative and comparisons to a representative sample are not available (Ratliff and Smith (2021)). Selection into the IAT test is discussed in section 3.2.

[16] When completing the race IAT, respondents are sequentially presented with images of Black and white individuals, and words that have positive or negative connotations. For the gender-career IAT, respondents are presented with gendered names, and words associated with career or family. Respondents complete a series of rapid sorting exercises grouping together the images, or names, with words. The IAT is meant to measure the strength of a respondent's association between individual characteristics and word connotations, as sorting is easier when associated items are sorted together. IAT measures have been the subject of scrutiny in the psychology literature. There is general agreement that these measures are relevant and predictive, particularly for aggregated group level measures of socially sensitive topics (Bertrand and Duflo (2017), Hehman et al. (2019), Ratliff and Smith (2021)).

[17] Respondents that report having taken three or more IATs are dropped from the sample. In survey years where respondents report their age rather than their year of birth, birth years are coded as $survey\ year - age$ for surveys taken July through December, or $survey\ year - (age + 1)$ for surveys taken January through June.

[18] Particularly important to coverage was the broadcast signal type: UHF versus VHF. VHF signals travel further and are less impacted by physical obstacles. Televisions at the time were generally equipped to receive VHF signals. Demand for expanded channel offerings and the removal of legal barriers by the Federal Communications Commission led to a growth in UHF broadcasting channels, with newer television sets equipped to receive both UHF and VHF signals. Uptake of this new technology was costly though. 95 percent of households had TVs, but a UHF signal could only be received in 54 percent of these. Details on the construction of predicted coverage rates are available in Kearney and Levine (2019) and its appendix.

Levine (2019) coverage estimates. Figure 1b shows the coverage distribution for CCES survey respondents' counties of residence. Note that as simulated coverage rates are estimated using the average relationship between broadcast technology factors and signal receipt, they do not depend on the quality of receiving television sets in the county which likely correlates with household wealth. Though national ratings are not available for the early years of *Sesame Street*, these estimated coverage rates strongly predict the Nielsen ratings that are available for 28 metropolitan areas (Haydon (1973)). Viewership in the past week increases by 0.59 percentage points in the 2 to 5 year old demographic for each percentage point increase in predicted coverage. These measures predict a national coverage rate of 65%, consistent with the 69.4% that was estimated for *Sesame Street* in 1970.

3 Empirical strategy

To identify the effects of early childhood exposure to *Sesame Street* coverage on our outcomes, we exploit two dimensions of exposure variation: differences in broadcast coverage rates between counties and variation in cohort exposure. As described above, the technological specifications of broadcasting towers generated significant geographic variation in coverage rates, illustrated in Figure 1a. This geographic variation is combined with cohort variation in exposure to identify *Sesame Street* specific effects. Cohorts born between 1959 and 1963 would have been attending elementary school when the show started airing in 1969. We compare their outcomes to slightly younger individuals in the same county, born between 1964 and 1968, who would have been 5 and younger in 1969. Preschool was uncommon in this period with only 9% of 3 year olds and 19% of 4 year olds attending in the 1970 census. For 5 year olds, 57% attended kindergarten of which 88% were enrolled in half day programs (Kearney and Levine (2019)). Not only were preschool aged children the show's targeted demographic, they generally had time to watch *Sesame Street*, which often aired during the school-day. Viewership data from 28 metro areas confirms that preschool-aged children were the show's primary audience: 71% of the show's viewers were 2 to 5 year olds, 19% were between 6 and 11, and 9.7% were older than 12. The discontinuous cohort variation generated by the start of first grade is critical to identifying *Sesame Street*'s effects. People residing in areas with better *Sesame Street* coverage will also have had more exposure to other public broadcasting content, both during their childhood as well as in their adult years. The effects of this exposure will be controlled for by geographic fixed effects unless some cohorts were more impacted than others. For adult programming, there is no a priori reason to expect that the 1963 and 1964 cohorts were differentially exposed, or discontinuously affected, by shows such as the popular PBS current events program *Frontline*. Similarly, sharp exposure discontinuities are unlikely for programming aimed at older school-aged children, such as *The Electric Company*, a *Sesame Street*-style educational show with similar messaging, casting and content that began airing in 1971. By that time, the 1963 and 1964 cohorts would have been approximately 8 and 7 years old. These adjacent school-aged kids have similar TV watching habits; with any divergence possibly reflecting habits or preferences formed through *Sesame Street*. Indeed, the younger cohorts may have developed a greater taste for such programming due to earlier exposure to *Sesame Street*. In that case, any effects of *The Electric Company* can be interpreted as part of the broader *Sesame Street* effects we estimate.

We identify likely treated respondents as those from cohorts that were under the age of 6 when the show first aired *and* who live in high broadcast coverage counties. Likely untreated respondents are those who were 6 or older in 1969 as well as those who were younger but who live in counties with a poor broadcast signal. Our empirical strategy follows Kearney and Levine (2019). The main empirical model used in estimations using the CCES data on voting related outcomes is

$$Y_{icjdy} = \beta_0 + \beta_1(preschool69_i * SSCov_j) + \beta_2 X_i + \gamma_{scy} + \delta_{jdy} + \epsilon_i. \quad (1)$$

Y is a voting outcome for individual i, in cohort c, residing in county j, and voting in congressional district d in year y. The indicator $preschool69_i$ is set to 1 if i was under age 6 when *Sesame Street* first aired. This is interacted with $SSCov_j$, the predicted level of *Sesame Street* coverage in county j. Our preferred specification includes X_i, controls for an individual's gender and race. For the CCES data, γ_{scy} is a (*state* $\times$ *cohort* $\times$ *year*) fixed effect which captures electoral behaviors and preferences in a particular election year that impact all respondents in a state from the same birth cohort as well as the impact of any time-varying state level policy shocks or events that may have impacted particular cohorts within a state over their lifetime. δ_{jdy} is a (*county* $\times$ *congressional district* $\times$ *year*) fixed effect which captures the electoral behaviors and preferences of respondents that are constant across cohorts residing in the same county and voting in the same congressional district election. β_1 is the coefficient of interest. It identifies the difference in the voting patterns on a congressional district ballot between the preschool age cohorts in high *Sesame Street* coverage counties, as compared to slightly older cohorts in their county voting on the same ballot, while controlling for the voting patterns of same age cohorts in their state who were surveyed in the same year. This specification differs slightly from that used in Kearney and Levine (2019). The fixed effects are augmented to control for congressional districts and survey years, to capture all aspects of a congressional election that affect all respondents in the congressional district (third party candidates, idiosyncratic shocks, up-ballot elections, etc.). For non-voting outcomes, controlling for voting patterns in a particular election is not necessary, and not always possible as congressional districts are not observed in other datasets.[19] For these outcomes we estimate β_1^{KL} using the same fixed effects as Kearney and Levine (2019): a (*state* $\times$ *cohort*) fixed effect, γ_{sc}^{KL}, and a *county* fixed effect, δ_j^{KL}. Alongside these results, we also show estimates that allow us to observe underlying cohort differences in outcomes. We estimate

$$Y_{icjdy} = \mu_0 + \mu_1 preschool69_i + \mu_2 X_i + \phi_{sy} + \delta_{jdy} + \epsilon_i, \tag{2}$$

a simpler specification that omits the cohort fixed effect, such that $\hat{\mu}$ captures cohort trends. To observe treatment effects, we compare the $\hat{\mu}$ cohort differences observed in high and low coverage areas.

Finally, it should be noted that these estimates aggregate cohort differences across multiple cohorts. These estimates could be affected by trends in outcomes that differ between high and low coverage areas, a valid concern given the rapid social and political

[19]For turnout and registration outcomes in the CPS data, we estimate β_1^{CPS} using δ_{jy}^{CPS}, a $(county \times year)$ fixed effect as congressional districts are not observed in the CPS voting data.

changes happening during this period (civil rights movement, women's rights movement, school desegregation, etc.). We generate several additional estimates to ensure that such factors are not confounding our results. First, we estimate specifications that include interactions between cohort indicators and county characteristics to control for the differential impacts local conditions may have had on different cohorts. County characteristics include whether the county was part of a metropolitan ares, the county's 1970 Black population share, low income share, Democratic vote share, a measure of school segregation, and the 1948 vote share for Strom Thurmond, a segregationist presidential candidate. We also estimate specifications that control for respondents' education, and income. Second, we estimate

$$Y_{icjdy} = \lambda_0 + \sum_{c=59, c\neq 63}^{68} \lambda_{1c}(Cohort_{ic} * SSCov_j) + \lambda_2 X_i + \gamma_{scy} + \delta_{jdy} + \epsilon_i, \quad (3)$$

where $Cohort_{ic}$ is an indicator set to one if the respondent was born in cohorts c. Figures reporting the cohort estimates of λ_{1c} allow us to check for (i) differential pre-treatment trends between high and low coverage areas, and (ii) a discontinuous jump in outcomes starting with the first treated cohorts. These estimates also allow us to observe how treatment effects change across the treated cohorts though it is not a prior clear what we would expect.[20]

A measure of coverage, not viewership. Our treatment measures a county's exposure to *Sesame Street* broadcast coverage, not actual viewership. Because our ratings data is very limited, we cannot estimate the impact of actual viewership. Rather, our estimates are "intent-to-treat" estimates, identifying the policy relevant effect of making the show more accessible in a county rather than the impact of watching the show on an individual.[21] As

[20]First, it is not theoretically obvious, nor is there clear empirical evidence to suggest a dosage response: a single year of exposure at age 5 may generate the maximum effects. Second, children's programming was transformed by *Sesame Street*'s success, influencing new educational programs on other channels like *Schoolhouse Rock!* (1973-ABC) or *Fat Albert and the Cosby Kids* (1972-CBS). If such shows generated treatment effects in low *Sesame Street* coverage counties, the inter-county differentials we are estimating will shrink over time. Finally, coverage rates are calculated using data from 1968–1969. As broadcasting and television set technology was evolving over this period, coverages estimates are less accurate for the youngest cohorts.

[21]Micro-data on viewership, and even national county level ratings data is unavailable, limiting our ability to evaluate selection into viewership. A very limited examination of heterogeneity in ratings using the 28 metro areas with viewership data is reported with heterogeneity results in appendix section A3.

seen in Figure 1b, the theoretical comparison of counties with 0% and 100% coverage is out of sample. We will interpret the effects of a 30 percentage point increase in a county's predicted coverage rate, roughly the gap between the 25th percentile (48.5%) and the 75th percentile (81%) of the predicted coverage distribution.[22] This is comparable to moving from a typical county with weak coverage, like Los Angeles county which had a predicted coverage rate of 49%, to a typical high coverage county, like Sacramento county, with an estimated coverage rate of 79%.

Unobserved time substitutions. Treatment in our context includes both increased exposure to *Sesame Street* coverage, and a time substitution away from other activities such as viewership of other TV programs, or entirely different uses of children's time. While we frame our interpretation as the impacts of *Sesame Street* coverage, our estimates could also be picking up the effects of reduced exposure to other television programming in which minorities and women were not represented in a respectful or egalitarian manner.

Self-reported outcomes. Because of the secrecy of the ballot box, many of our outcomes are self-reported. Exposure to *Sesame Street*'s prosocial messaging could (ambiguously) affect the propensity to misrepresent oneself by either increasing the desire to supply a socially desirable response or decreasing the propensity to lie to a surveyor. To check for this, we compare self-reported turnout records to the validated turnout record available in the CCES data. Voter turnout is known to be prone to prosocial misrepresentation: in the CCES data, 85% of respondents self-report voting, but turnout is validated for only 63% – much more in line with official numbers. Recent work has found that this gap is due to overreporting turnout to surveyors (Enamorado and Imai (2019)). We estimate $\hat{\beta}_1$ on an indicator for inconsistent validated and self-reported responses. We find no evidence that *Sesame Street* impacted prosocial misrepresentation (Table 1, panel a, row 3). There is no evidence that younger cohorts in high *Sesame Street* coverage counties differentially misrepresent their behavior to surveyors.

[22] This interpretation follows that used in Kearney and Levine (2019).

3.1 Migration: Selection and attenuation bias

Ideally we would observe respondents' counties of residence in childhood to assign them to $SScov_j^{child}$ to estimate β_1^{child}. As these are unobserved in our data, β_1 estimates β_1^{adult} as it is estimated using $SScov_j^{adult}$, the 1969 coverage in respondents' county of residence when surveyed between 2006-2020. Substantial out-of-county migration could generate attrition bias. The possibility that exposure affected out-of-county migration, and destination choice, is also a concern. We explore these issues below.

We find no evidence that younger cohorts residing in counties that had high *Sesame Street* coverage differ in their probability of being life-long residents of a city as compared to older cohorts in the same county. Problematic selection bias could arise if exposure to *Sesame Street* coverage impacted respondents' inter-county mobility between 1969 and their survey date. Though we do not observe county of residence in 1969, in many waves CCES respondents report how long they have lived in their current *city*. We identify city residents who have lived in their city since they were five or younger and estimate both $\hat{\beta}_1^{KLadult}$ and the simplified specification on this indicator. In both cases, we fail to reject the null of no effect (Table A4, column 1, panel a). We also estimate $\hat{\beta}_1^{KLadult}$ on indicators for being non-Hispanic white (column 4) and female (column 5). We find no evidence that younger cohorts residing in counties that had high *Sesame Street* coverage differ in their demographics from older cohorts in the same county.

We find no evidence that younger cohorts residing in counties that had high *Sesame Street* coverage differ in their probability of facing diverse congressional ballots as compared to older cohorts in the same county.[23] We estimate both $\hat{\beta}_1^{KLadult}$ and the simplified specification on the indicators for facing minority-white and woman-man ballots. For both specifications and both indicators we fail to reject the null of no difference between the treated and untreated cohorts (Table A4, columns 2 and 3, panel a). All respondents residing in counties with high *Sesame Street* coverage in 1969 are more likely to face such ballots (see the positive coefficients on $SScov_j^{adult}$ in the simplified specification). This is

[23] County lines and congressional districts are often incongruous.

not surprising as these counties tend to be more urban (Figure 1a) and less white (note the negative coefficient on $SScov_j^{adult}$ in column 4, Table A4). However, as compared to slightly older cohorts in their county, younger cohorts in high coverage counties are no more likely to reside in parts of the county with diverse congressional ballots.

We find no evidence that *Sesame Street* exposure impacted the probability of residing in one's childhood state or county. To estimate the effects of $SScov_j^{child}$ on migration, we use restricted-use data from the Panel Survey of Income Dynamics (PSID). We identify 1,204 original panel members born between 1959 and 1968 whose county of residence is observed in both 1969 and 2019.[24] Using this sample, we estimate $\hat{\beta}_1^{KLchild}$ (Table A4, panel b), on indicators for residing in 2019 in the same state (column 2) and county (column 3) as in 1969. In both cases, we fail to reject the null of no effect. There is no evidence that younger cohorts from high coverage counties differ in their likelihood of residing in their childhood county or state as compared to slightly older cohorts from their county.

We find no evidence that *Sesame Street* exposure impacted the probability of residing in a county that had high *Sesame Street* coverage as an adult. Even if the show did not impact the propensity to move, it may have impacted destination choice. Problematic selection would arise if younger and older cohorts from high coverage counties reside as adults in counties that differ in their 1969 coverage rates. To check this, we estimate $\hat{\beta}^{KLchild}$ on $SSCov_j^{adult}$. We fail to reject the null of no such difference (Table A4, column 4, panel b). Critically, we can rule out that a 30 percentage point increase in the coverage rate impacted the 1969 *Sesame Street* coverage rate in adult counties of residence outside of a [-6.4, 5] percentage point range with a 95% confidence level.

Coverage rates between destination and sending counties are correlated for county-to-county migrants, counteracting attenuation bias. Only 47.3% of our PSID sample

[24] The original household sample included 4,312 members from these cohorts. We estimate $\hat{\beta}^{KLchild}$ on an indicator for attrition between the original PSID sample and 2019 (Table A4, column 1, panel b). We find no evidence that exposure to *Sesame Street* coverage impacted attrition in the PSID.

reside in their childhood county in 2019, raising the issue of attenuation bias.[25] Nevertheless, the correlation between the $SSCov_j^{adult}$ and $SSCov_j^{child}$ in the PSID is high at 0.547 (p-value$<$0.001) (Table A5, column 1, row 1). Attenuation bias is reduced as even among inter-county migrants, these coverage rates are correlated (column 2). 30% of inter-county migration occurs between neighboring counties with highly correlated coverage rates (0.789 in column 3). We also observe a positive correlation of 0.101 (p-value=0.06) for long distance movers (column 4). These patterns in the PSID are consistent with census data.[26]

We observe similar patterns of treatment effects on outcomes observed in the 1980's. Using the CPS voter and registration supplement, we are able to examine voter turnout for elections throughout the 1980's and 1990's. We observe similar treatment effects on this outcome in the 1980's, when our sample cohorts are in their twenties and have accumulated fewer decades of migration history.

Overall, we find no evidence that the show impacted inter-county migration, or that exposed cohorts differentially select into high or low coverage adult counties of residence. Nevertheless, the small size of the PSID sample prevents us from completely ruling out some impact on migration and destination choice. Accordingly, we interpret our estimates of β_1 most accurately as the difference between younger and older cohorts residing in counties that had high *Sesame Street* coverage in 1969. The balance of evidence suggests observed differences are due to exposure to the show and that, to the extent that migration is affecting our estimates, it is primarily attenuating them. Table A6 reports estimates of our main effects for CCES respondents who have lived in the same city since before age 6. Consistent with attenuation bias being the main impact of migration, we generally find larger effects for this sub-sample, though the small sample size gives imprecise estimates.

[25] 9% of the CCES sample are city never-movers though this survey question will not capture respondents who moved between cities in a county, and those who moved, but then returned, to their childhood city.

[26] Using census estimates of 2016-2020 county-to-county moves for each county pair in our main sample, we regress the destination county's coverage on the sending county's coverage, weighting the regression by the pair's total county-to-county movers. Results (Table A5, row 2) show very similar correlation estimates.

3.2 Selection into the *Project implicit* data

A particular concern with the analysis of *Project Implicit* IAT scores is selection into the sample. IAT tests are publicly available online and anyone can take a test. Exposure to *Sesame Street* coverage may have affected selection into this sample, biasing estimates.[27] To check for selection, we calculate $ShareSS_j = \frac{N_{j,1964 \leq Cohort_i \leq 1968}}{N_j}$, the share of treated cohort observations in each county j. We regress $ShareSS_j$ on $SSCov_j$ to check if a larger share of IAT test takers are from treated cohorts in counties that had higher levels of *Sesame Street* coverage. Results are reported in appendix Table A7. Column 1 presents estimates using all the race IAT test takers, column 2 using only non-Hispanic white race IAT test takers, and column 3 shows estimates for the gender-career IAT test. For the race IAT, both estimates in columns 1 and 2 are small and not statistically significant (p-value$\in [0.31, 0.84]$). There is no evidence that exposure to *Sesame Street* coverage changed selection into the race IAT data. In contrast, *Sesame Street* coverage increased the likelihood of taking the (less widely used) gender-career IAT test.[28] Given this selection, section 7 focuses on the race IAT data. Nevertheless, it should be noted that this impact on selection into taking the gender-career IAT test suggests that the show may have increased awareness of gender and career biases.

4 *Sesame Street* coverage increased electoral participation

In this section, we document our first finding: that younger cohorts in high *Sesame Street* coverage counties are more likely to turnout to vote and have an active voter registration, likely due to an observed increase in political interest and knowledge.

Younger cohorts in high *Sesame Street* coverage counties vote more. The $\hat{\beta}_1$ estimate of 0.139 (p-value $<$ 0.001) on verified general election turnout (Table 1, panel a, column 2, row 1) implies that the probability of younger cohort members turning out to vote increases by 4.2 percentage points, a 6.6% increase, if they reside in a county where the coverage rate

[27] If more biased individuals are less likely to take IAT tests, selection will bias estimates towards zero.

[28] The positive and statistically significant estimate of 0.034 implies that a 30 percentage point increase in the *Sesame Street* coverage rate increased the share of test takers from treated cohorts in a county by 1.0 ppts.

was 30 percentage points higher. This result is highly statistically significant and robust to alternative specifications (column 3), including those with cohort specific controls for county characteristics (appendix Table A3, column 1).[29] Figure 2a plots the cohort level $\hat{\lambda}_{1c}$ estimates, and Figure 2c plots cohort differences in verified turnout rates for counties with above and below median predicted coverage rates. In both figures we see no clear evidence of coverage correlated differential trends until the 1964 cohort – which has a differentially high validated turnout rate in high coverage areas– a pattern that persists for younger cohorts. To more clearly understand the underlying differences, Figure 2e plots the $\hat{\mu}$'s for turnout difference between treated and untreated cohorts estimated separately by quintile of predicted *Sesame Street* coverage. As visible in Figure 2c, younger cohorts in low coverage areas have lower levels of voter turnout, possibly due to age as these estimates do not include a $cohort \times year$ fixed effect. This cohort difference shrinks as the coverage quintile increases.

As discussed in section 3, Table 1, panel a, row 2 and Figure 2a show that the estimated $\hat{\beta}_1$ and $\hat{\lambda}_{1c}$'s are similar when using the indicator for self-reported election turnout. There is no evidence that younger cohorts in high *Sesame Street* coverage counties differentially misrepresent their behavior to surveyors. With this in mind, we can examine self-reported turnout behavior as reported in the CPS voting and registration supplement to (i) validate our findings in the CCES data for the same election years, and (ii) establish the continuity of this effect over time.[30] We begin by replicating our CCES analysis for the 2006-2020 elections using the CPS data. The estimated coefficient of 0.108 (p-value = 0.051) on self-reported turnout in the CPS (column 8) is also positive and statistically significant. We cannot reject equality (p-value= 0.77) with the CCES point estimate of 0.092 (p-value = 0.06) estimated on a comparable sample.[31] Next we use the earlier years of CPS voting data to

[29]Dropping data from 2006 and 2008, years when the quality of turnout validation was poorer (Grimmer et al. (2018)) give a similar estimate of 0.117 (p-value = 0.018). Figure A1 plots the coefficients from 1000 permutation tests that randomly assign cohorts (panel a) or counties (panel e). Placebo coefficients are normally distributed around 0 and none are as large in magnitude.

[30]Self-reported turnout is in the Performance of American Elections and the American National Election Studies but these smaller datasets contain too few observations from the 1959–1968 birth cohorts.

[31]The CPS voting data is coded to be comparable to our CCES sample: we drop non-citizens, naturalized Americans, and responses from proxy respondents. The CPS does not report congressional districts and county codes are only available in the later years for some CPS counties. In column 6 we limit the CCES data to counties identified in the CPS and estimate β_1^{CPS} using (county × year) and (state × cohort × year) fixed

establish that the treated cohorts have always had differentially higher turnout rates. The oldest treated cohort was first eligible to vote in the 1982 election. We estimate treatment effects on self-reported turnout using all elections between 1982 and 2002, as well as separately for the 1982-1992 and 1994-2002 periods. Results are reported in Table 2, Panel A. Due to data limitations, these early CPS estimates rely on a slightly different measure of exposure—metro-area rather than county-level coverage—making direct comparisons difficult.[32] Nevertheless, the estimated coefficients show an effect of similar magnitude. Exposure to *Sesame Street* increased these cohorts voter turnout in their first decade of electoral engagement, an effect that persisted throughout the decades.

To better understand why *Sesame Street* coverage impacts turnout, in appendix Table A8 we examine respondents' reasons for not voting.[33] Younger cohorts in high *Sesame Street* coverage counties are less likely to report not voting because they dislike the candidates and because they are not registered. These effects are robust across specifications, explaining about half the effect on reported non-turnout. Other estimates are less clear, but taken jointly, suggest an increased interest in participating in elections, increased knowledge about the voting process, and an increased willingness to incur non-pecuniary costs to vote.

Younger cohorts in high *Sesame Street* coverage counties register to vote more. The $\hat{\beta}_1$ estimate of 0.091 (p-value = 0.017) on having a verified active registration record (Table 1, panel b, column 2, row 1) implies that for a 30 percentage point increase in the coverage rate, the probability of having a verified active registration record is 2.7 percentage points higher, a 3.7% increase. This effect is statistically significant, and robust to alternative specifications (column 3). Figure 2b plots the cohort level $\hat{\lambda}_{1c}$ estimates, confirming a discontinuity in validated registrations between younger and older cohorts. Figure 2f plots the $\hat{\mu}$'s for registration differences between treated and untreated cohorts estimated separately by quintile of predicted *Sesame Street* coverage. In the lowest coverage areas,

effects.

[32]County identifiers are not available in the early years of the CPS data. We use the subset of observations residing in the 225 identified metro areas. We estimate metro area's predicted coverage rates as the population weighted average coverage rate across composing counties. 22 MSA's are dropped as they are composed of split counties.

[33]This question is only administered to self-reported non-voters and thus does not shed light on the reasons for non-turnout for respondents who misreport their voting behavior.

younger cohorts have lower levels of voter registration, a gap that disappears in the highest coverage quintiles. This pattern is also observable in Figure 2d which plots cohort differences in verified registration for counties with above and below median predicted coverage rates.

While still present, the estimate of $\hat{\beta}_1$ on self-reported voter registration is smaller in magnitude with little visual evidence of a discontinuity between treated and untreated cohorts. This is consistent with what we observe in the CPS data where we also find a small positive statistically insignificant effect on self-reported registration (column 8). We cannot reject equality between the CPS and CCES estimates (p-value$=0.74$) when estimated with the same specification and counties. These small differences in registration are not entirely surprising. Absent moves, voter registration is a persistent state. Once registered, most people stay registered such that registration rates are quite high by later-life creating ceiling effects. Consistent with this, we actually observe a large treatment effect on registration when we look at early-life elections when respondents are first becoming eligible to register (Table 2, panel b, column 4). For our 1982 thru 1992 election sample, the 0.170 (p-value $= < 0.001$) coefficient implies that a 30% increase in the *Sesame Street* coverage rate increased self-reported registration for treated cohorts by 8.5%.

Finally, it is interesting to note in Table 1 that a substantial share of CCES respondents misreport their registration status. 94% of respondents self-report being registered to vote while only 74% have a verified active registration. The estimated coefficient of -0.066 (p-value$=0.07$) on an indicator of inconsistency (Table 1, panel b, row 3) implies that younger cohorts in high coverage counties are less likely to misreport their registration status. Is this misreporting driven by misinformation or misrepresentation? Turnout results showed no evidence of an impact on prosocial misrepresentation. Furthermore, evidence discussed below shows that younger cohorts in high coverage counties are more politically informed suggesting that *Sesame Street* coverage increased respondents' knowledge of their voter registration status.

Heterogeneity in this increase in electoral participation is examined in the appendix. Table A9 evaluates differences by respondent demographics. We cannot reject the null of no heterogeneity in estimated effects. There is no evidence that these turnout effects are driven by a specific demographic group. Furthermore, appendix Table A10 shows that turnout increases hold for different ballot compositions, regardless of candidate demographics, and are also observed during midterm elections which are not affected by the demographic composition of the presidential ballot ($\hat{\beta}_1 \in [0.121, 0.178]$ across the subsamples). Consequently, this effect on electoral participation is distinct from the effect on preferences over candidate demographics that we document in section 5. How did *Sesame Street* increase electoral participation? We hypothesize that this may be related to exposure to the neighborhood civic and community engagement portrayed in *Sesame Street*. After exploring several potential mechanisms, we find that treated cohorts exhibit higher levels of political interest and knowledge, which is consistent with this hypothesis.

We find no evidence of a difference in educational attainment and income. Treatment effects on years of education and family income (in $ 1,000) are reported in appendix Table A11. Like Kearney and Levine (2019) we cannot reject the null of no effect on these outcomes. In both the CCES and IAT data, the education coefficients are positive but not statistically significant (0.167 (p-value= 0.49) and 0.034 (p-value= 0.56) respectively), as is the coefficient on family income in the CCES (at 3.5 (p-value= 0.36)). Adding controls for educational attainment and family income to our main specifications (appendix Table A3) does not meaningfully alter our estimates. Increased educational attainment and family income are not the main mechanisms behind our results.

Younger cohorts in higher *Sesame Street* coverage counties have greater political knowledge and interest. Estimates in Table 3, panel a, show they are better at recognizing the names of their senators and representatives, with an estimate of 0.179 (p-value = 0.012) out of 3 names. They report more interest in politics with a $\hat{\beta}_1$ of 0.155 (p-value = 0.048) on a 4 point scale and are less likely to report accessing no news media in the past 24 hours with an estimate of -0.029 (p-value= 0.15), though this last result is not statistically significant.

Younger cohorts in higher *Sesame Street* coverage counties are more likely to report

moderate political identities, but do not differ in strong political identification or active political engagement. With respect to political identification, a more complex picture emerges in Table 3, panel b. Younger cohorts in high coverage counties are more likely to report identifying with a political ideology and major political party. Political ideology is measured in both the IAT and CCES data. We estimate treatment effects of 0.037 (p-value < 0.001) and 0.024 (p-value$=0.53$) respectively. Though the CCES estimate on having a political ideology is not statistically significant, we do observe increased identification with major political parties in the CCES data with an estimate of 0.055 (p-value$=0.08$). All of these effects are driven by respondents who express weak preferences towards a political party or identity.[34] These indicators of political affiliation are a common form of political engagement. The majority of respondents indicate that they identify with a political party and ideology. We do not observe a treatment effect on reporting a strong party identification, or rarer more involved forms of political engagement, examined in panel c, associated with individuals highly engaged in the political process. We see no effects on voting in primary elections, donating money to political campaigns, putting up political signs, attending political meetings or working for a candidate or campaign. For all of these indicators, we fail to reject the null of no treatment effect (p-values $\in [0.19, 0.77]$).[35] Consistent with this, Table A13 shows that estimates of β_1 on electoral participation are larger for marginally engaged respondents, as proxied by the fact that their primary election turnout is not verified.

How do these findings on political participation compare to the existing literature? Much causal work on voter turnout focuses on interventions conducted shortly before elections. The *persuasion rate*, as described by DellaVigna and Gentzkow (2010), measures the share of individuals whose behavior was altered by an intervention. DellaVigna and Gentzkow (2010) summarize evidence from get-out-the-vote field experiments and report persuasion rates of roughly 4.5–20% for interventions such as door-to-door canvassing and phone calls. Our 4.2 ppt effect of a 30 ppt increase in coverage implies a persuasion

[34] These respondents describe their preferences as "leaning towards", "not strong preference for", "slightly" or "moderate" as opposed to "strong" or "very".

[35] We also find no evidence of a difference in measures of non-political civic engagement as proxied by blood donation, union membership, and having been part of the military, reported in appendix Table A12.

rate of 38% (see Appendix A1 for calculations). Unsurprisingly, this is larger than most persuasion rates found for short-run turnout interventions. *Sesame Street* was designed to produce lasting attitudinal change through sustained engagement and repeated messaging that shaped norms and skill acquisition. Our findings are therefore more comparable to estimates of education's impacts on turnout. As most of these papers examine turnout in the first years of voting, we compare them to the 16.5% persuasion rate implied by our 1982-1992 estimates.[36] Sondheimer and Green (2010) examine three early childhood education interventions and find long-run validated turnout effects of +2 to +8.8 ppts, implying persuasion rates of 6.41% to 13.19%. Cohodes and Feigenbaum (2021) report a +6 ppt increase in turnout from improvements in high school educational quality, implying a persuasion rate of 9.1%, while Kaplan et al. (2025) estimate that an additional year of schooling increases turnout by 3 ppts, a 10.4% persuasion rate. Regarding mechanisms, our results suggest that the impacts of *Sesame Street* coverage on registration and turnout stem from an increase in the political engagement and knowledge of marginal voters. This is consistent with meta-analyses that highlight party identification, political interest, and political knowledge as consistently linked to voter turnout (Smets and Van Ham (2013)). Previous work has also identified increases in political interest, knowledge and participation following the introduction of public television in Norway in the 1960s (Sørensen (2019)).

5 *Sesame Street* coverage shaped who receives votes

In this section, we examine how *Sesame Street* coverage impacted voter preferences over candidates to the U.S. House of Representatives. In particular, we examine whether exposure to the show's working women and the egalitarian interactions among the racially integrated cast increased reported voting for minority and women candidates.

[36] The persuasion rate for the 1982–1992 estimates (16.5%) is substantially lower than the persuasion rate for the 2006–2020 estimates (38%). This difference is driven primarily by variation in the baseline turnout rate which is considerably lower, at 43.3%, when individuals first become eligible to vote at age 18 than the 64.3% turnout observed when they are in their 40s and 50s.

5.1 Younger cohorts in high *Sesame Street* coverage counties report more votes for minority candidates

We restrict the sample to the 13,120 respondents facing one of the 689 ballots where a minority candidate ran against a non-Hispanic white candidate.[37] We build four mutually exclusive indicators defined for all respondents: reporting a vote for the minority candidate, the white candidate, a third party candidate or not voting. Estimates of β_1 using each indicator as an outcome are reported in column 2 of Table 4, panel a. Younger cohorts in high *Sesame Street* coverage counties are substantially more likely to report voting for a minority candidate compared to older cohorts in these counties. The 0.271 (p-value $<$ 0.001) estimate of $\hat{\beta}_1$ (Table 4, panel a, row 1) implies that for a 30 percentage point increase in the coverage rate, the probability a voter reports a vote for a minority candidate is 8.1 percentage points higher, a 20% increase. This effect is highly statistically significant, and robust to alternative specifications (column 3), including those with cohort specific controls for county characteristics (appendix Table A3, column 2).[38] We also observe reductions in reported nonvoting, as indicated by the negative and statistically significant coefficient reported in the fourth row. In addition, younger cohorts report lower rates of voting for white candidates, as shown by the -0.144 (p-value= 0.09) estimate (Table 4, panel A, row 2). That reported voting for white candidates is lower for younger cohorts, despite their higher turnout, signals that this effect on preferences over candidates is distinct from the impacts on electoral participation discussed in section 4.

Figure 3a plots the $\hat{\lambda}_{1c}$ cohort estimates for the minority candidate, white candidate, and non-voting indicators, and Figure 3c plots cohort differences in reporting a minority vote for counties with above and below median predicted coverage rates. In both figures we see no clear evidence of coverage correlated differential trends until the 1964 cohort – which reports a differentially high vote share for minority candidates in high coverage areas– a pattern that persists for younger cohorts. Figure 3e plots the $\hat{\mu}$'s for differences in

[37] Appendix Table A4 shows that *Sesame Street* coverage rates in this subset of counties was 9.03 percentage points higher on average. However, we find no evidence that younger cohorts differ in their likelihood of facing this type of ballot, mitigating concerns of a treatment effect on selection into this subsample.

[38] Figure A1 plots the coefficients from 1000 permutation tests that randomly assign cohorts (panel b) or counties (panel f). Placebo coefficients are normally distributed around 0 and none are as large in magnitude.

reporting a minority vote between treated and untreated cohorts estimated separately by quintile of predicted *Sesame Street* coverage. In the lowest coverage areas, younger cohorts are less likely to report voting for a minority candidate, a pattern that shrinks and even reverses as the coverage quintile increases.

Similar impacts are found when the sample is limited to respondents who report voting for a major party, and validated voters (appendix Table A14). Table A6 also shows a larger effect of 0.848 (p-value < 0.001) for respondents who have lived in their city since before the age of 6, as compared to 0.216 (p-value $= 0.039$) for those who moved after age 6, a difference that is statistically significant (p-value $= 0.004$). This is consistent with migration generating attrition bias but should be interpreted with caution given the small subsample. Potential impacts on election outcomes are discussed in appendix A2.

When estimated separately, we find no evidence of a differential effect (p-value$= 0.79$) on reported voting for minority candidates for white and minority respondents (Table 4, columns 4 and 5), though the small sample size generates imprecise estimates for the minority sample. For white respondents, the effect is driven by reduced voting for white candidates while the turnout effect plays a larger role for minority respondents.

5.2 Younger cohorts in high *Sesame Street* coverage counties report more votes for women candidates

We restrict the sample to the 1,034 ballots where a woman ran against a man.[39] As above, we construct four mutually exclusive indicators representing reported votes. Estimates of β_1 on each indicator are reported in Table 4, panel b, column 2. Younger cohorts in high *Sesame Street* coverage counties report more votes for women candidates. The 0.192 (p-value < 0.001) estimate of $\hat{\beta}_1$ (Table 4, panel b, row 1) implies that a 30 percentage point increase in the coverage rate increased the probability of reporting a vote for a woman candidate by 5.8 percentage points, a 15% increase. This effect is highly statistically significant, and robust to alternate specifications (column 3), including those with cohort

[39]These respondents live in counties with higher 1969 *Sesame Street* coverage rates, by 11.3 percentage points on average. However, we find no evidence that younger cohorts differ in their likelihood of facing this type of ballot, mitigating concerns of a treatment effect on selection into this subsample (appendix Table A4).

specific controls for county characteristics (appendix Table A3, column 3).[40] We also observe reduced reported non-voting, as indicated by the negative coefficient of comparable magnitude (panel b, row 4). Exposure to *Sesame Street* coverage had little effect on reported voting for men (panel b, row 2). Reported voting for third party candidates is lower for treated respondents but few respondents report third party votes. Figure 3b plots the $\hat{\lambda}_{1c}$ cohort estimates for the woman candidate, man candidate, and non-voting indicators, and Figure 3d plots cohort differences in reporting a vote for a woman for counties with above and below median predicted coverage rates. In both figures we see no clear evidence of coverage correlated differential trends until the 1964 cohort – which reports a differentially high vote share for women candidates in high coverage areas– a pattern that persists for younger cohorts. Figure 3f plots the $\hat{\mu}$'s for differences in reporting a vote for a woman between treated and untreated cohorts estimated separately by quintile of predicted *Sesame Street* coverage. In the lowest coverage areas, younger cohorts are slightly less likely to report voting for women candidates, a pattern that reverses as the coverage quintile increases. Appendix A2 discusses possible implications for election outcomes. When estimated separately, we find no evidence of a differential effect (p-value= 0.79) for female and male respondents (columns 4 and 5). Suggestively the turnout effect is more important for male voters while female voters switch from men to women candidates.

Note that this result is not as clear-cut as the effect on voting for minority candidates. When the sample is limited to validated voters reporting a major party vote (appendix Table A14), we cannot reject the null of no effect, though the direction of the coefficient suggests a small positive effect on women candidates' vote share. We also find no evidence of a larger effect for respondents who have lived in their city since before the age of 6 (appendix Table A6) though the small sample complicates this comparison.

[40] Figure A1 plots the coefficients from 1000 permutation tests that randomly assign cohorts (panel c) or counties (panel g). Placebo coefficients are normally distributed around 0 and none are as large in magnitude.

5.3 Younger cohorts in high *Sesame Street* coverage counties report more votes for Democratic candidates.

As above, we build four mutually exclusive indicators for reported votes: reporting a Democratic vote, a Republican vote, a third party vote or not voting. β_1 estimates for each are reported in column 2 of Table 4, panel c. Younger cohorts in high *Sesame Street* coverage counties report more Democratic votes. The 0.100 (p-value $= 0.007$) estimate of $\hat{\beta}_1$ (panel c, row 1) implies that a 30 percentage point increase in the coverage rate increased the probability of reporting a Democratic vote by 3.0 percentage points, a 7.5% increase. The $\hat{\beta}_1^{KL}$ estimate (column 3) is of a slightly smaller magnitude, but still marginally significant (p-value=0.07). Most of the Democratic gains come from reduced reported non-voting, as indicated by the negative and significant coefficients in the fourth row. Estimates on reported Republican votes are positive but smaller in magnitude. Reported third party votes are lower for treated respondents but few report third party votes. When the sample is limited to validated voters voting for major parties we cannot reject the null of no effect, though the coefficient suggests a small positive effect favoring Democrats (Table A14).

It is worth stressing that the impact of *Sesame Street* coverage on reported votes is not driven by a treatment effect on the propensity to misrepresent one's vote. Exposure to *Sesame Street*'s prosocial messaging could have impacted the misrepresentation of voting behavior, a concern as we only observe reported voting, not actual votes. The data shows no evidence of this. First, in section 4 we compared reported and verified turnout data and found no treatment effect on the propensity to misrepresent turnout behavior. Second, we compare estimates for validated and non-validated voters who report voting for a major party candidate (Table A14, columns 3 and 4). Non-validated voters who report a major party vote are respondents for whom the issue of prosocial misrepresentation may be particularly pronounced. In all three panels, we fail to reject the null that treatment effects are equal across these two types of respondents (p-value$\in [0.65, 0.77]$), rejecting the idea that respondents prone to misrepresentation are driving our results.

6 Disentangling correlated candidate characteristics

Section 5 shows that exposure to *Sesame Street* coverage increased reported voting for minority, women and Democratic candidates. Which candidate attributes voters are responding to is unclear. Many candidate attributes correlate with one another. Democratic candidates are more demographically diverse. 69.3% of women candidacies and 67.8% of minority candidacies are Democratic candidacies. Minority candidacies are also more likely to be women at 33% versus 19.4% of white candidacies. Below we consider which of these observable candidate characteristics voters are responding to. We show that changes in reported votes are due to candidate demographics rather than candidate party.

Note that we cannot completely rule out that being a minority or woman candidate correlates with unobservable candidate characteristics that may be generating these effects. In this section we explore mechanisms, ruling out potential confounds. In section 7 we provide evidence that younger cohorts in high *Sesame Street* coverage counties have reduced measures of biases, the most likely explanation for the observed impacts on reported voting.

6.1 Candidates from underrepresented backgrounds drive the increase in reported voting for Democrats

Younger cohorts in high coverage counties report more Democratic votes. Is this due to different political views or a response to correlated candidate characteristics?

Table 5 estimates the effects on reported vote by party for sub-samples of ballots with different demographic compositions. Column 1 limits the sample to ballots where both candidates are white men. The turnout effect is still apparent with the -0.093 (p-value= 0.10) estimate on not voting, but the turnout gains are split between Democrat and Republican candidates, favoring Republicans. The $\hat{\beta}_1$ on reporting a Democratic vote, conditional on voting for a major party (column 2), is negative and statistically insignificant at -0.011 (p-value= 0.88). On ballots featuring two white men, we cannot reject that voting for both parties is comparably affected by the turnout effect of *Sesame Street* coverage. In contrast, we see strong treatment effects favoring Democrats on ballots where the Democratic candidate is a woman (column 3), at 0.183 (p-value = 0.002); and where the Democratic candidate is a minority (column 5), at 0.235 (p-value = 0.009). Overall, the increase in reported votes for Democrats is driven by women and minority Democratic candidacies.

6.2 More voting for minority candidates regardless of party or gender

Younger cohorts in high *Sesame Street* coverage counties report more votes for minority candidates than slightly older cohorts in their counties. Table 6, panel a, evaluates whether candidate characteristics correlated with minority identity, namely political party and gender, can explain this.[41] We estimate β_1 separately for ballots where the minority candidate is the Democrat (column 1) and the Republican (column 2). In both sub-samples, $\hat{\beta}_1$ is positive and significant. Respondents report more votes for minority candidates regardless of their party, with suggestively larger effects for Republican minority candidates (p-value= 0.16). We also estimate β_1 separately for ballots where the minority candidate is a woman (column 3) and a man (column 4). In both sub-samples, $\hat{\beta}_1$ is positive and significant. The increase in reported voting for minority candidates holds for both minority men and women candidates, and is possibly larger for women minority candidates (p-value= 0.14).

6.3 Voting effects for women candidates hold for Democratic women only, with larger effects for minority women

Younger cohorts in high *Sesame Street* coverage counties report more votes for women candidates. Table 6, panel b, evaluates if this is a response to characteristics correlated with candidates' gender, namely political party and minority identity.[42] We separately estimate β_1 on ballots where the woman is running as a Democrat (column 1) and Republican (column 2). Treated respondents are no more likely to report voting for Republican women than for the Democratic men they run against, though estimates are imprecise as there are

[41]Minority candidates tend to be Democrats. Of the 689 ballots where a minority candidate runs against a white candidate, the minority candidate runs as a Democrat on 76% . Minority candidates are also more likely to be women. 33% of minority candidacies are woman candidacies versus 19.4% of white candidacies.

[42]Women candidates tend to be Democrats. Of the 1,034 woman-man ballots, the woman runs as a Democrat on 74%. Women candidates are also more likely to be minorities. 26.2% of woman candidacies are minority candidacies as opposed to 20.5% of candidacies by men.

only 268 such ballots. In contrast, the $\hat{\beta}_1$ for Democratic women is large and statistically significant. The increase in reported voting for women seems to be driven by Democratic women, though we cannot reject equality between the two coefficients. We also estimate β_1 separately on ballots where the woman candidate is white (column 3) or a minority (column 4). In both sub-samples, $\hat{\beta}_1$ is positive and statistically significant. Increased reported voting for women candidates holds for both white and minority women, though the magnitude of the estimate is larger for minority women, a difference that is statistically significant (p-value= 0.06). Overall, results in panels a and b suggest that reported voting for minority women candidates is doubly affected in high *Sesame Street* coverage counties.

6.4 No clear impacts on preferences for policies or incumbents

Younger cohorts in high *Sesame Street* coverage counties do not differ in their policy preferences in ways consistent with party platforms. Exposure to *Sesame Street* coverage could shape policy preferences towards those more likely to be espoused by minority and female candidates. To check this, we examine views on specific policies. Appendix Table A15 shows estimates on policy indices.[43] Overall, we cannot reject that younger cohorts in high *Sesame Street* coverage counties support environmental policies and abortion rights at the same rate as older cohorts in their counties. Treatment effects are observed signaling increased support for same-sex marriage, an effect consistent with increased tolerance of diversity. In contrast, we observe reduced support for less restrictive immigration policies. The CCES and IAT surveys also include opinion questions on US race relations and policies. Estimated effects on these measures are generally small, not statistically significant, and inconsistent across data sources and question topics.[44] Overall, impacts on policy views are mixed or null and do not clearly align with any particular political platform.

Younger cohorts in high *Sesame Street* coverage counties identify more with political

[43] Administered questions vary across surveys. Selected questions used to construct indices were administered in multiple survey years. Disaggregated estimates are reported in Table A16 and A17.

[44] For instance, while we detect a statistically significant positive effect on support for affirmative action policies in the IAT survey, the coefficient for the same topic in the CCES survey is negative. We also detect a statistically significant reduction in agreement with statements expressing belief in structural racial barriers for the small sample of Black respondents in the CCES, but not for the full sample of all respondents.

ideologies and parties, without clearly favoring one end of the political spectrum. Table 7, panel a, examines treatment effects on respondents' reported political ideology in both the CCES and IAT data. Younger respondents in high coverage counties are more likely to report identifying with a political ideology as opposed to being moderate or neutral (columns 2 and 5). However, the shift is split between liberal and conservative. When we restrict the sample to individuals expressing a political ideology (columns 3 and 6), we cannot reject the null of no effect on identifying with a liberal ideology (p-value$\in[0.13, 0.63]$). A similar pattern is observed for party identification in the CCES data. Results in panel b show reduced identification as independents but conditional on having a party identity, we cannot reject the null of no effect on identifying with the Democratic party (p-value=0.21).

Younger cohorts in high *Sesame Street* coverage counties do not differ in their voting for incumbents. As younger cohorts in high *Sesame Street* coverage counties demonstrate greater political knowledge, they may differ in their propensity to vote for incumbent candidates. However, Table A18 shows no evidence of an impact on reported voting for incumbents. Turnout effects are split. Respondents report more votes for both incumbent and non-incumbent candidates. When we restrict the sample to major party voters, we cannot reject the null of no effect on reported voting for the incumbent (p-value$=0.87$).

Overall, the balance of evidence suggests that the changes in reported voting occur in response to candidate demographics. This conclusion is further corroborated by analysis of the *Project Implicit* data below confirming impacts on measures of racial biases.

7 Younger cohorts in high *Sesame Street* coverage counties have reduced measures of racial biases

In this section we show that *Sesame Street*'s portrayal of positive minority role models in an integrated cohesive community reduced long-run racial biases, thereby increasing the propensity to vote for diverse candidates. Given the evidence of selection into the gender-career IAT discussed in section 3.2, we focus our analysis on the race IAT. Note however that the selection pattern suggests that exposure to *Sesame Street* coverage increased interest in issues surrounding gender-career biases, though we cannot make conclusive statements on how it impacted gender-career IAT scores, reported in panel b of Table 8.[45]

Younger cohorts in high *Sesame Street* coverage counties have lower long-run measures of white racial bias towards Blacks. The -0.067 sd (p-value = 0.007) estimate on standardized race IAT scores signals a negative and statistically significant effect on implicit biases for non-Hispanic white test takers (Table 8, column 2, row 1).[46] This estimate implies that a 30 percentage point increase in the coverage rate reduces IAT scores by -0.02 standard deviations.[47] This effect is highly statistically significant, and robust to alternate specifications (columns 3 and 4), including those with cohort specific controls for county characteristics (appendix Table A3, column 4). Figure 4a plots the $\hat{\lambda}_{1c}^{KL}$ cohort estimates on the IAT scores of white test takers, and Figure 4b plots cohort differences for counties with above and below median predicted coverage rates. Though cohort level estimates are not precises, these figures show no clear evidence of coverage correlated differential trends until the 1964 cohort – which scores differentially lower on the IAT test in high coverage areas– a pattern that persists for younger cohorts. Figure 4c plots the $\hat{\mu}$'s for differences in IAT scores between treated and untreated cohorts estimated separately by quintile of predicted *Sesame Street* coverage. Younger cohorts have lower IAT scores – even in low coverage counties– but this gap grows substantially larger in the higher coverage quintile counties.

In addition to measures of implicit biases, the IAT survey asks about explicit preferences

[45]If less biased test takers select into taking the test, estimates will be biased towards zero. Given the selection patterns detected, estimates of $\hat{\beta}_1^{KL}$ on the gender-career IAT scores are, unsurprisingly, small and not statistically significant (Table 8, panel b).

[46]Figure A1 plots the coefficients from 1000 permutation tests that randomly assign cohorts (panel d) or counties (panel h). Placebo coefficients are normally distributed around 0 and none are as large in magnitude.

[47]For reference, Corno et al. (2022) find a -0.63 standard deviation effect on the race IAT of white freshman college students in South Africa measured at the end of their freshman year after being randomly allocated to a shared dorm room with a Black student. Like Corno et al. (2022), we also find an opposite sign effect for Blacks (row 2) though it is imprecisely estimated given the small sample. This suggests that, consistent with contact theory, exposure to the equitable Black-white interactions on *Sesame Street* may have reduced Black prejudice against whites.

and warmth felt towards European and African Americans. Overall, non-Hispanic white test takers rarely express explicit in-group preferences. When asked whether they prefer European Americans over African Americans, 60% respond that they like the two groups equally. Similarly, when asked separately about the warmth they feel towards European and African Americans on a 10 point scale, 66% report the same value for both groups (appendix Figures A2a and A2b).[48] It is thus unsurprising that $\hat{\beta}_1^{KL}$ estimates on white test takers' explicit preferences are small and not statistically significant (p-value $\in [0.17, 0.51]$) when we use the full sample (Table 9, rows 1 and 3). However, when we limit the sample to test takers who do not report the same value for both groups, in rows 2 and 4, effect magnitudes are larger, and a small effect signaling reduced preferential in-group warmth becomes marginally significant (p-value$= 0.09$).[49] Reporting no preferences across racial groups is less common for African Americans. 36% report liking European and African Americans equally and assign these groups the same level of warmth on the 10 point scale (Figures A2a and A2b). Effects on African Americans are less muted, displaying a similar reduction in in-group preferences for exposed cohorts. Overall, these patterns suggest a muted reduction in explicit in-group preferences that is more pronounced for respondents willing to express variation in warmth towards individuals based on their race.

Non-incumbent minority candidates gain most from their electorate's exposure to *Sesame Street* coverage. If racial biases drive the change in voting patterns, larger effects may be expected for less known minority candidates. For known candidates, voters may base decisions on observed behaviors, potentially reducing the influence of racial biases. Consistent with this hypothesis, non-incumbent minority candidates gain most from electorate exposure to *Sesame Street*. Table 10 presents heterogeneity by minority candidate incumbency status. For non-incumbent minority candidates, the impact of *Sesame Street* is large at 0.313 (p-value$= 0.23$) when they face a non-incumbent (column

[48]Expressing racial preferences, particularly for white Americans, is stigmatized in contemporary U.S. culture. As such, test takers reporting no preferences likely include truthful responses as well as socially-desirable misrepresentations that reduce the signal received from measures of explicit racial preferences.

[49]The -0.192 estimate implies that a 30 ppt coverage rate increase reduces the gap in expressed warmth by -0.06 points, a 0.017 sd decrease, for white test takers who don't give the same warmth value for both groups.

1) and 0.363 (p-value = 0.003) when facing an incumbent (column 3).[50] For incumbent minority candidates (column 2), the estimate is substantially smaller at 0.070 (p-value= 0.62) and not statistically significant. The difference between these two groups approaches statistical significance (p-value= 0.11) when comparing columns 2 and 3.

Given these findings, it is worth considering if the impacts of *Sesame Street* differed based on the social and racial context in which it aired. A priori it is not clear whether media "contact" with minority role models would have a larger effect in areas where opportunities for other forms of contact were high or low. Where contact opportunities were few, or where pre-existing racial stereotypes were strong, the show may have filled a vacuum allowing for a large effect. Alternatively, effects may have been larger in areas where preschool age exposure to the show could be reinforced through other interactions. This question is of academic interest as it would shed light on contact theory mechanisms. Unfortunately, the lack of national county level viewership data complicates this type of heterogeneity analysis. Indeed, county characteristics could affect both viewership probability, as parents might be less likely to allow their children to watch the program, and how the show impacted its viewers. Without viewership data, disentangling these mechanisms is impossible. Nevertheless, heterogeneity in effects by county characteristics are presented in appendix A3. Overall, we find no evidence of significant heterogeneity across a number of variables that characterize the social and racial environment of counties.

8 Conclusion

Can child mass media shape prejudices and racial biases in adulthood? Can it increase people's willingness to vote for diverse representatives? The importance of media representation is increasingly recognized and discussed in the popular press. How gender and race are approached in child media is also the subject of political debate. Yet despite this flood of attention, there is little causal evidence on how positive non-stereotyped representation affects social and economic outcomes.

[50] Column 1 estimates are imprecise due to the small number of white-minority ballots with no incumbents.

This paper helps inform these debates. We examine the long-run effects of exposure to *Sesame Street* at its 1969 launch, when it introduced diverse, egalitarian children's programming into a media environment with little prior exposure to such content. This setting allows us to estimate extensive-margin effects of exposure to diversity in media among preschool-aged children–a population and context likely to be especially responsive given both age, the absence of diversity in prior children's media, and the limited children's programming at the time. We show that preschool-age exposure to *Sesame Street* coverage, and its portrayal of a civically engaged, inclusive, egalitarian and diverse America, had long-run effects on adult voting and racial biases. Decades later, exposed cohorts are more interested and engaged in the political process, registering and turning out to vote at higher rates than slightly older cohorts in the same county. When voting, exposed cohorts report more votes for minority and women candidates. While voting increased slightly more for Democrats–whose candidates are more diverse–turnout gains are split between both parties on ballots where both candidates are white men. This is consistent with the observed increase in moderate political engagement we see across the political spectrum. Overall, the evidence suggests that these changes in voting behavior result from reduced biases against diverse candidates, consistent with the reduced racial bias measures of white IAT test takers we observe for these exposed cohorts.

Our results suggest multiple new directions for future research on children's media, which remains limited. First, our extensive-margin findings are estimated using the historical introduction of *Sesame Street*. Further work should examine how these effects might manifest today, a very different media environment where on screen diversity is not novel and child media markets have become much more fragmented. Second, our understanding of the long-run effects of children's media on outcomes beyond education is scarce. This paper shows that child media can have long-run impacts on consequential adult decisions, suggesting that other long-run impacts could be important as well. This paper also demonstrates that exposure to underrepresented role models has important effects on majority group members, an underexamined relationship. Finally, we show that mass media can reduce prejudices and biases, revealing an important yet understudied behavioral lever for efforts in prejudice reduction.

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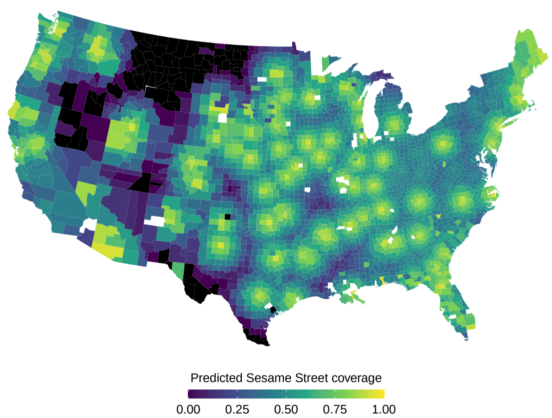
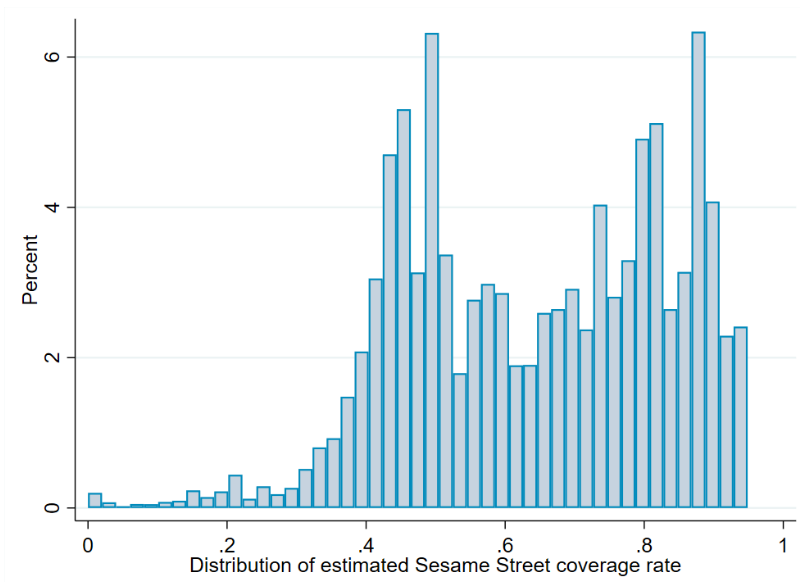
9 Figures



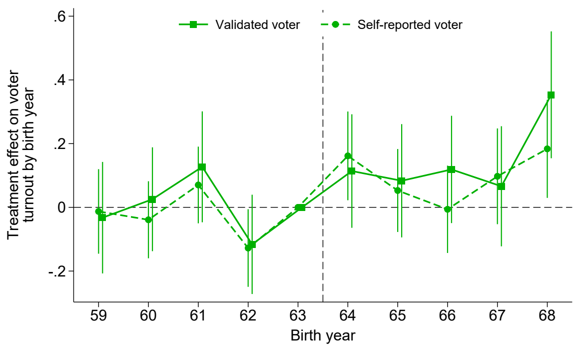
(a) Estimated 1969 *Sesame Street* coverage rates



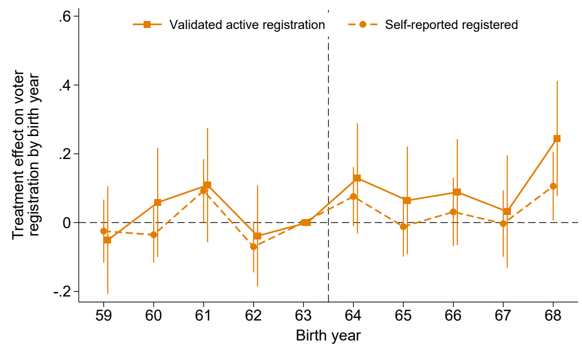
(b) Estimated coverage rates in the CCES

Figure 1. ***Sesame Street*** **coverage**

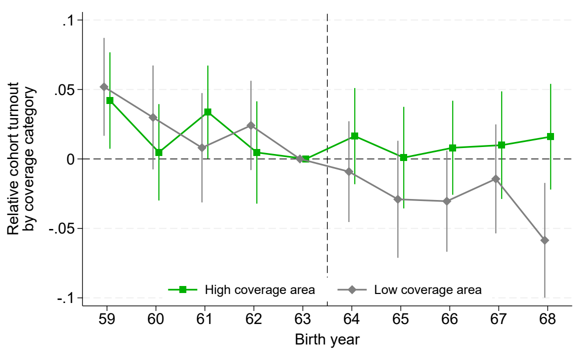
Notes: Estimated coverage is simulated using the average relationship between the broadcast technology of signal towers and signal receipt. In Figure a, the color gradient represents the predicted share of households that could watch *Sesame Street* in that county in 1969 as calculated in Kearney and Levine (2019). Figure b plots the distribution of estimated *Sesame Street* coverage rates for our sample of CCES respondents.



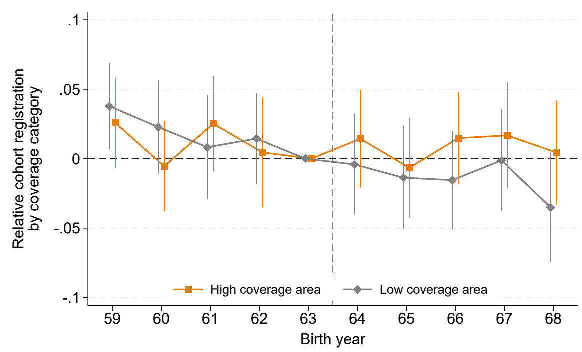
(a) Self-reported and validated turnout



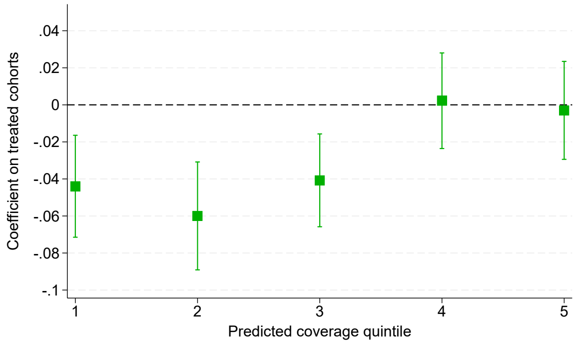
(b) Self-reported and validated registration



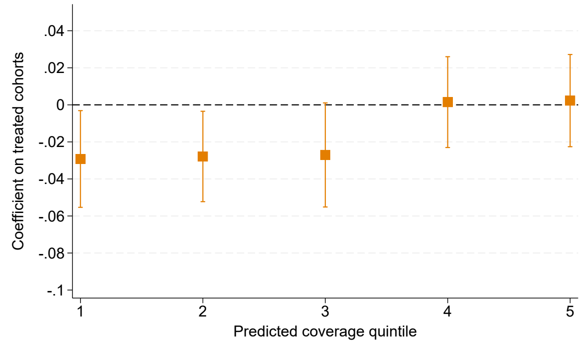
(c) Validated turnout for high and low coverage counties



(d) Validated registration for high and low coverage counties



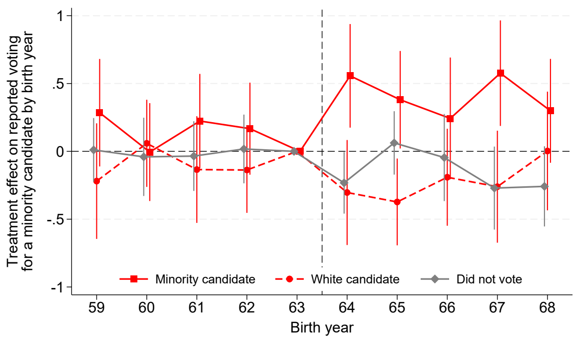
(e) Validated turnout by coverage quintile



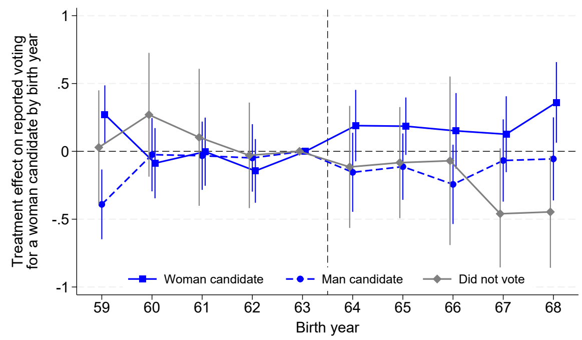
(f) Validated registration by coverage quintile

Figure 2. Voter turnout and registration is higher for younger cohorts in high *Sesame Street* coverage counties

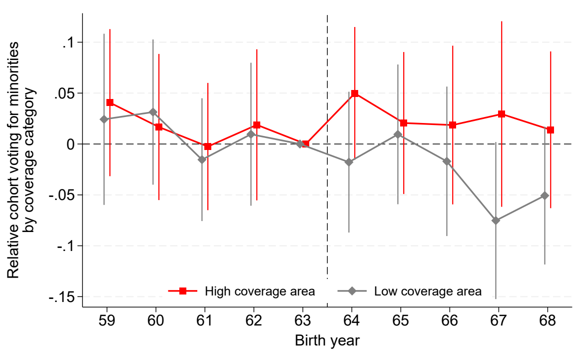
Notes: The top Figures plot the $\hat{\lambda}_{1c}$ estimates on $(Cohort_{ic} * SSCov_j)$ as specified in equation 3. Controls for race and gender and (*state*×*cohort*×*year*) and (*county*×*congressional district*×*year*) fixed effects are included. The 1963 cohort is omitted. Figures c-f plot the $\hat{\mu}$ estimates on $preschool69_i$ as specified in equation 2 with controls for race and gender and (*state*×*year*) and (*county*×*congressional district*×*year*) fixed effects. Figures c-d estimate cohort $\hat{\mu}$'s separately for areas with above and below median coverage. Figures e-f estimate $\hat{\mu}$ separately by quintile of *Sesame Street* coverage. All outcome variables are indicators. Estimates include survey weights. 95% confidence intervals are depicted using standard errors clustered at the county level.



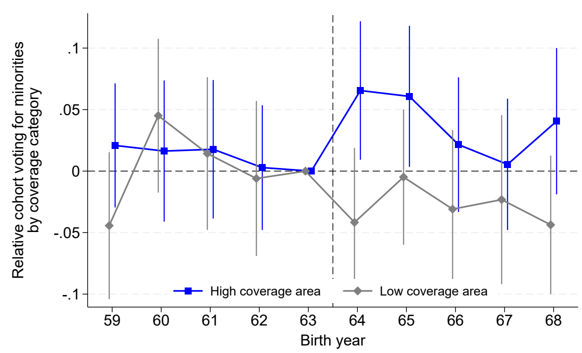
(a) Reported voting on minority-white ballots



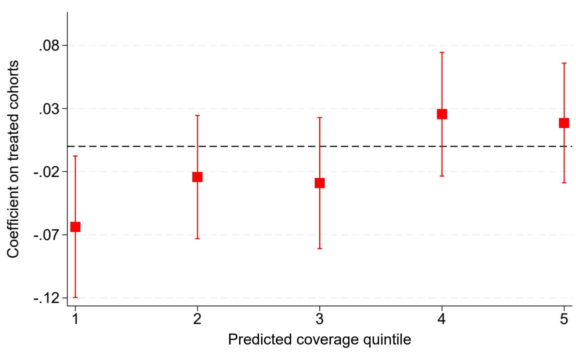
(b) Reported voting on woman-man ballots



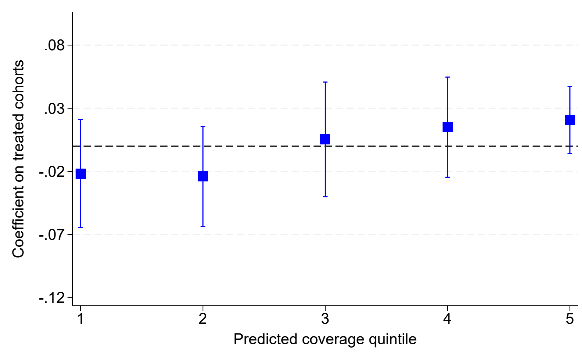
(c) Minority candidate votes–high and low coverage counties



(d) Woman candidate votes–high and low coverage counties



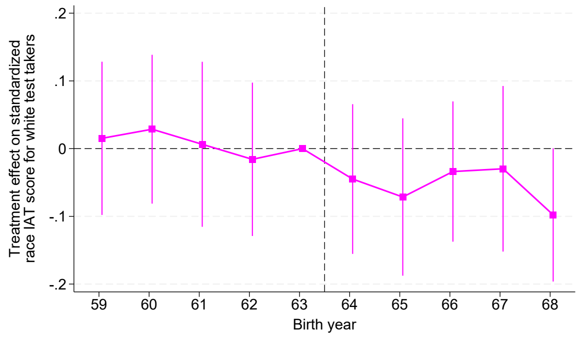
(e) Minority candidate votes by coverage quintile



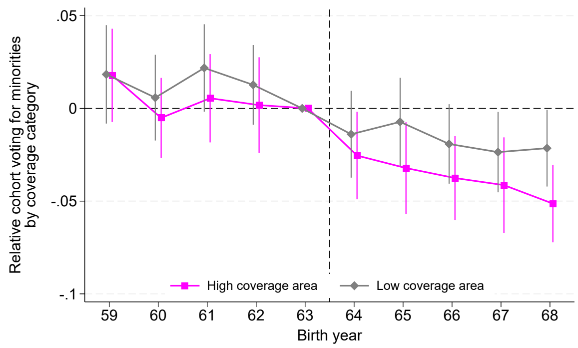
(f) Woman candidate votes by coverage quintile

Figure 3. Reported voting for minority and women candidates is higher for younger cohorts in high *Sesame Street* coverage counties

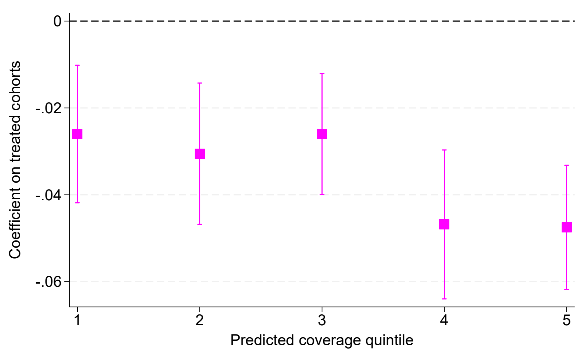
Notes: The top Figures plot the $\hat{\lambda}_{1c}$ estimates on $(Cohort_{ic} * SSCov_j)$ as specified in equation 3. Controls for race and gender and (*state*×*cohort*×*year*) and (*county*×*congressional district*×*year*) fixed effects are included. The 1963 cohort is omitted. In each figure, coefficients for three indicators are plotted: reported voting for the minority (Figure a) or woman (Figure b) candidate; reported voting for the white (Figure a) or man (Figure b) candidate; reporting not voting. Estimates on reported voting for third party candidates are omitted for clarity. Figures c-f plot the $\hat{\mu}$ estimates on $preschool69_i$ as specified in equation 2 with controls for race and gender and (*state*×*year*) and (*county*×*congressional district*×*year*) fixed effects. Figures c-d estimate cohort $\hat{\mu}$'s separately for areas with above and below median coverage. Figures e-f estimate $\hat{\mu}$ separately by quintile of *Sesame Street* coverage. The sample is limited to respondents who face minority-white ballots (Figures a-c-e) or woman-man ballots (Figures b-d-f). All outcome variables are indicators. Estimates include survey weights. 95% confidence intervals are depicted using standard errors clustered at the county level.



(a) **White IAT scores**



(b) **White IAT scores for high and low coverage counties**



(c) **White IAT scores by coverage quintile**

Figure 4. **Measures of implicit bias against Blacks are lower for younger white test takers in high *Sesame Street* coverage counties**

Notes: Larger positive values indicate stronger Black-bad and white-good associations. The top figure plots the $\hat{\lambda}_{1c}^{KL}$ estimate as specified in equation 3, with controls for gender and *state*×*cohort* and *county* fixed effects. Figures b and c plot the $\hat{\mu}$ estimates on $preschool69_i$ as specified in equation 2 controlling for gender and county fixed effects. Figure b estimates cohort $\hat{\mu}$'s separately for areas with above and below median coverage. Figure c estimates $\hat{\mu}$ separately by quintile of *Sesame Street* coverage. 95% confidence intervals are depicted using standard errors clustered at the county level.

10 Tables

Table 1: Voter turnout and registration is higher for younger cohorts in high *Sesame Street* coverage counties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full CCES			**CCES with CPS counties only**			**CPS**	
Dependent indicator variable	**Dependent mean**	$\hat{\beta}_1$	$\hat{\beta}_1^{KL}$	**Dependent mean**	$\hat{\beta}_1$	$\hat{\beta}_1^{CPS}$	**Dependent mean**	$\hat{\beta}_1^{CPS}$
Panel a: *Sesame Street* increased self-reported and validated voter turnout								
Verified general election turnout	0.634	0.139*** (0.040) [51,723]	0.109*** (0.040) [56,850]	0.643	0.074 (0.059) [28,788]	0.096* (0.058) [29,230]		
Self-reported election turnout	0.851	0.120*** (0.032) [51,723]	0.118*** (0.033) [56,850]	0.862	0.087* (0.047) [28,788]	0.092* (0.048) [29,230]	0.716	0.108* (0.055) [23,016]
Inconsistent self-reported voting status with validation	0.229	-0.034 (0.034) [51,723]	-0.018 (0.034) [56,850]	0.230	0.013 (0.052) [28,788]	-0.005 (0.050) [29,230]		
Panel b: *Sesame Street* increased voter registration and knowledge of registration status								
Verified active voter registration	0.744	0.091** (0.038) [47,604]	0.081** (0.038) [52,030]	0.747	0.081 (0.054) [26,454]	0.093* (0.054) [26,832]		
Self-reported voter registration	0.944	0.044* (0.024) [47,353]	0.049* (0.028) [51,777]	0.948	-0.001 (0.038) [26,338]	0.005 (0.038) [26,720]	0.865	0.020 (0.043) [22,939]
Inconsistent self-reported registration status with validation	0.210	-0.066* (0.037) [47,353]	-0.062* (0.033) [51,777]	0.209	-0.060 (0.059) [26,338]	-0.078 (0.059) [26,720]		
Controls: Gender and race		Yes	Yes		Yes	Yes		Yes
FE: County		.	Yes		.	.		.
FE: State x cohort		.	Yes		.	.		.
FE: County x cong. district x year		Yes	No		Yes	No		NA
FE: State x cohort x year		Yes	No		Yes	Yes		Yes
FE: County x year		.	No		.	Yes		Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. $\hat{\beta}_1^{KL}$ estimates are estimated using the same specification as Kearney and Levine (2019), with $(county)$ and $(state \times cohort)$ fixed effects. $\hat{\beta}_1^{CPS}$ estimates control for $(county \times year)$ and $(state \times cohort \times year)$ as congressional districts are not available in the CPS voting data. Columns 1-3 use the full CCES sample. As county of residence is only observable for some counties in the CPS data, columns 4-5 estimate effects in the CCES using only respondents living in counties observed in the CPS. CPS voting data is cleaned and coded to be comparable to our CCES sample: non-citizens and naturalized Americans are dropped, as well as responses reported by proxy respondents. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table 2: Voter turnout and registration was always higher for younger cohorts in high *Sesame Street* coverage areas

	(1)	(2)	(3)	(4)	(5)	(6)
	1982-2002 CPS		**1982-1992 CPS**		**1994-2002 CPS**	
Dependent indicator variable	**Dependent mean**	$\hat{\beta}_1^{CPS}$	**Dependent mean**	$\hat{\beta}_1^{CPS}$	**Dependent mean**	$\hat{\beta}_1^{CPS}$
Panel a: *Sesame Street* increased self-reported voter turnout since treated cohorts were first eligible to vote						
Self-reported election turnout	0.479	0.089** (0.035) [53,383]	0.433	0.094** (0.043) [26,414]	0.525	0.085 (0.053) [26,969]
Panel b: *Sesame Street* increased voter registration, starting when treated cohorts were first eligible to register						
Self-reported voter registration	0.669	0.108*** (0.030) [53,014]	0.600	0.170*** (0.049) [26,156]	0.735	0.060 (0.041) [26,858]
Controls: Gender and race		Yes		Yes		Yes
FE: State x cohort x year		Yes		Yes		Yes
FE: MSA x year		Yes		Yes		Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1^{CPS}$ is estimated using a modified version of equation 1 which controls for $(MSA \times year)$ and $(state \times cohort \times year)$ fixed effects as congressional districts are not available in the CPS voting data. County identifiers are also not available in the early years of the CPS data. We use the subset of observations residing in one of 225 identified metro areas. We estimate metro area's predicted coverage rates as the population weighted average coverage rate across composing counties. 22 MSA's are dropped as they are composed of split counties. CPS voting data is cleaned and coded to be comparable to our CCES sample: non-citizens and naturalized Americans are dropped, as well as responses reported by proxy respondents. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the MSA level, with the following significance indicators: * $p<0.1$, ** $p<0.05$ and *** $p<0.01$.

Table 3: Younger cohorts in high *Sesame Street* coverage counties are more politically informed with increased rates of moderate political identities

	(1)	(2)	(3)	(4)	(5)
	CCES			Race IAT	
Dependent variable	**Mean**	$\hat{\beta}_1$	$\hat{\beta}_1^{KL}$	**Mean**	$\hat{\beta}_1^{KL}$
Panel a: Political knowledge					
Recognized names of elected representatives (out of 3)	2.6	0.179** (0.071)	0.139** (0.070)		
		[50,836]	[55,939]		
Interest in government and public affairs (scale 1-4)	3.4	0.155** (0.079)	0.165** (0.078)		
		[46,835]	[51,248]		
Accessed no news media in the past 24 hours	0.056	-0.029 (0.020)	-0.018 (0.021)		
		[41,890]	[45,616]		
Panel b: Political identification					
Has a political ideology	0.615	0.024 (0.039)	0.015 (0.038)	0.804	0.037*** (0.009)
..... Weakly identifies with a political ideology	0.396	0.017 (0.041)	0.034 (0.038)	0.599	0.027** (0.012)
..... Strongly identifies with a political ideology	0.220	0.007 (0.032)	-0.018 (0.028)	0.205	0.010 (0.010)
N		[51,575]	[56,707]		[319,563]
Identifies with a major political party	0.839	0.055* (0.031)	0.069** (0.033)		
..... Weakly identifies with a major political party	0.418	0.073* (0.043)	0.091** (0.040)		
..... Strongly identifies with a major political party	0.421	-0.018 (0.040)	-0.022 (0.039)		
N		[51,527]	[56,659]		
Panel c: Involved political engagement					
Verified congressional primary turnout	0.347	0.026 (0.042)	0.014 (0.041)		
		[47,604]	[52,030]		
Donated money to a political campaign or organization	0.241	0.011 (0.038)	0.016 (0.033)		
		[43,637]	[47,405]		
Put up a political sign	0.198	0.033 (0.038)	0.045 (0.035)		
		[43,637]	[47,405]		
Attended a political meeting	0.134	-0.041 (0.031)	-0.006 (0.028)		
		[43,637]	[47,405]		
Worked for a candidate or campaign	0.061	-0.020 (0.021)	-0.008 (0.018)		
		[43,637]	[47,405]		
Reports having run for office	0.028	0.012 (0.013)	0.014 (0.013)		
		[43,491]	[47,267]		
Controls: Gender and race		Yes	Yes		Yes
FE: County		.	Yes		Yes
FE: State x cohort		.	Yes		Yes
FE: County x cong. district x year		Yes	No		No
FE: State x cohort x year		Yes	No		No

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. $\hat{\beta}_1^{KL}$ estimates use $(county)$ and $(state \times cohort)$ fixed effects. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing outcome and control variables are omitted. Most dependent variables are indicator variables. Self-report of interest in and public affairs is an index variable ranging from 1 (Hardly at all) to 4 (Most of the time). Recognized names of elected representatives takes on values from 0 to 3 indicating whether the respondent recognizes the name of their current U.S. House Representative, and both U.S. Senators. All estimates using CCES data employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table 4: Younger cohorts in high *Sesame Street* coverage counties report more votes for minority, women, and Democratic candidates

	(1) Share	(2) $\hat{\beta}_1$	(3) $\hat{\beta}_1^{KL}$	(4)	*p-value of difference*	(5)
Panel a: Reported voting on minority-white ballots				Respondent is		
				White		**Minority**
Minority	0.409	0.271*** (0.082)	0.195*** (0.074)	0.312*** (0.095)	—0.790—	0.383 (0.260)
White	0.398	-0.144* (0.085)	-0.083 (0.070)	-0.266*** (0.092)	—0.289—	0.022 (0.268)
Third party	0.028	0.004 (0.021)	-0.006 (0.017)	-0.009 (0.028)	—0.918—	-0.016 (0.063)
Not voting	0.165	-0.132* (0.069)	-0.106* (0.063)	-0.037 (0.077)	—0.050—	-0.389** (0.169)
N		[11,811]	[12,819]	[8,055]		[2,469]
Panel b: Reported voting on woman-man ballots						
				Female		**Male**
Woman	0.396	0.192*** (0.058)	0.192*** (0.050)	0.214** (0.106)	—0.794—	0.170 (0.129)
Man	0.417	-0.031 (0.062)	-0.004 (0.059)	-0.164* (0.094)	—0.127—	0.055 (0.109)
Third party	0.025	-0.041** (0.017)	-0.056*** (0.020)	-0.032 (0.021)	—0.874—	-0.039 (0.037)
Not voting	0.162	-0.121*** (0.046)	-0.132*** (0.047)	-0.017 (0.070)	—0.164—	-0.186* (0.100)
N		[19,034]	[20,602]	[8,952]		[7,565]
Panel c: Reported voting by candidate party						
Democrat	0.398	0.100*** (0.037)	0.065* (0.036)			
Republican	0.407	0.033 (0.037)	0.059* (0.036)			
Third party	0.027	-0.021* (0.011)	-0.021* (0.011)			
Not voting	0.168	-0.113*** (0.033)	-0.103*** (0.034)			
N		[51,723]	[56,850]			
Controls: Gender and race		Yes	Yes	Yes		Yes
FE: County		.	Yes	.		.
FE: State x cohort		.	Yes	.		.
FE: County x cong. dist. x year		Yes	No	Yes		Yes
FE: State x cohort x year		Yes	No	Yes		Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. $\hat{\beta}_1^{KL}$ estimates use $(county)$ and $(state \times cohort)$ fixed effects. Unless otherwise specified, estimates of $\hat{\beta}_1$ are reported as indicated by the listed fixed effects. The sample is limited to respondents voting in U.S. House elections that feature a Democratic candidate and a Republican candidate (all panels); one of whom is a minority and the other is white (panel a); or one of whom is a man and the other is a woman (panel b). Each observation in the sample has one of the mutually exclusive voting behaviors listed set to 1 and all others set to 0. The share of respondents in each category is reported in column 1. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Values between columns 4-5 give the p-value on the interaction term of the equivalent fully interacted specification testing for heterogeneity between the two groups. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * $p<0.1$, ** $p<0.05$ and *** $p<0.01$.

Table 5: No evidence that younger cohorts in high *Sesame Street* coverage counties differ in their party preferences on ballots featuring two white men

	(1)	(2)	(3)	(4)	(5)	(6)
	Both candidates are white men		**Democrat is a woman**		**Democrat is a minority**	
	All	**Major party voters**	**All**	**Major party voters**	**All**	**Major party voters**
Democrat	0.014 (0.059)	-0.011 (0.073)	0.183*** (0.060)	0.156** (0.068)	0.235*** (0.089)	0.217** (0.094)
Republican	0.087 (0.064)		-0.026 (0.062)		-0.099 (0.090)	
Third party	-0.008 (0.018)		-0.022 (0.016)		-0.004 (0.022)	
Not voting	-0.093* (0.056)		-0.135*** (0.049)		-0.132* (0.075)	
N	[22,143]	[17,071]	[16,782]	[13,307]	[8,946]	[6,916]
Controls: Gender and race	Yes	Yes	Yes	Yes	Yes	Yes
FE: County x cong. dist. x year	Yes	Yes	Yes	Yes	Yes	Yes
FE: State x cohort x year	Yes	Yes	Yes	Yes	Yes	Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents and are estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. The sample is limited to respondents voting in U.S. House elections that feature a Democratic candidate and a Republican candidate. Each observation in the sample has one of the mutually exclusive voting behaviors listed set to 1 and all others set to 0. Estimates in even columns limit the sample to respondents who report voting for a major party candidate. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * $p<0.1$, ** $p<0.05$ and *** $p<0.01$.

Table 6: Younger cohorts in high *Sesame Street* coverage counties report more votes for minority candidates of both parties, and women Democrats

	(1)	*p-value of difference*	(2)	(3)	*p-value of difference*	(4)
Panel a: Reported voting when minority candidates are						
	Minority candidate is			**Minority candidate is**		
	Democrat		**Republican**	**Woman**		**Man**
Minority	0.235*** (0.089)	—0.161—	0.569** (0.232)	0.514*** (0.191)	—0.138—	0.190* (0.106)
White	-0.099 (0.090)	—0.364—	-0.317 (0.232)	-0.138 (0.179)	—0.916—	-0.160 (0.114)
Third party	-0.004 (0.022)	—0.614—	-0.028 (0.043)	-0.072* (0.037)	—0.007—	0.047* (0.024)
Not voting	-0.132* (0.075)	—0.598—	-0.225 (0.168)	-0.304*** (0.109)	—0.100—	-0.077 (0.086)
N	[8,946]		[2,655]	[4,282]		[7,285]
Panel b: Reported voting when women candidates are						
	Woman candidate is			**Woman candidate is**		
	Democrat		**Republican**	**White**		**Minority**
Woman	0.228*** (0.065)	—0.221—	0.036 (0.149)	0.138** (0.066)	—0.056—	0.467*** (0.162)
Man	-0.042 (0.072)	—0.580—	0.054 (0.167)	0.012 (0.072)	—0.247—	-0.189 (0.162)
Third party	-0.021 (0.017)	—0.369—	-0.066 (0.049)	-0.033* (0.018)	—0.590—	-0.060 (0.049)
Not voting	-0.164*** (0.052)	—0.233—	-0.025 (0.110)	-0.117** (0.055)	—0.428—	-0.217* (0.117)
N	[14,169]		[4,473]	[14,340]		[4,371]
Controls: Gender and race	Yes		Yes	Yes		Yes
FE: County x cong. dist. x year	Yes		Yes	Yes		Yes
FE: State x cohort x year	Yes		Yes	Yes		Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents and are estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. The sample is limited to respondents voting in U.S. House elections that feature a minority and white candidate (panel a); or a man and woman candidate (panel b). Each observation in the sample has one of the mutually exclusive voting behaviors listed set to 1 and all others set to 0. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Values between columns 1-2 and 3-4 give the p-value on the interaction term of the equivalent fully interacted specification testing for heterogeneity between the two groups. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table 7: Younger cohorts in high *Sesame Street* coverage counties report more political and party identification

	(1)	(2)	(3)	(4)	(5)	(6)
	Full CESS			**Full IAT**		
	Share	$\hat{\beta}_1$	**CCES if lib. or cons. dem. or rep.**	**Share**	$\hat{\beta}_1^{KL}$	**IAT if lib. or cons.**
Panel a: Impacts on political ideology						
Liberal	0.239	0.012 (0.033)	0.084 (0.056)	0.516	0.025** (0.012)	-0.006 (0.013)
Moderate/Neutral	0.324	-0.030 (0.039)		0.196	-0.037*** (0.009)	
Conservative	0.376	0.012 (0.040)		0.288	0.012 (0.011)	
Not sure	0.061	0.006 (0.024)				
N		[51,575]	[29,549]		[319,563]	[256,923]
Panel b: Impacts on party identity						
Democrat	0.429	0.071* (0.038)	0.057 (0.046)			
Independent	0.139	-0.058** (0.028)				
Republican	0.410	-0.016 (0.044)				
Not Sure	0.022	0.003 (0.013)				
N		[51,527]	[42,430]			
Controls: Gender and race		Yes	Yes		Yes	Yes
FE: County		.	.		Yes	Yes
FE: State x cohort		.	.		Yes	Yes
FE: County x cong. district x year		Yes	Yes		.	.
FE: State x cohort x year		Yes	Yes		.	.

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. $\hat{\beta}_1^{KL}$ estimates use $(county)$ and $(state \times cohort)$ fixed effects. Each observation in the sample has one of the mutually exclusive political identities listed set to 1 and all others set to 0. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Estimates using the CCES data employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table 8: Younger white test takers in high *Sesame Street* coverage counties have lower measures of implicit bias against Blacks

	(1)	(2)	(3)	(4)
Dependent variable and sample	**Mean score**	$\hat{\beta}_1^{KL}$		
Panel a: Impacts on the race IAT				
Race IAT score of white test takers	0.145	-0.067***	-0.067***	-0.051*
		(0.025)	(0.025)	(0.028)
		[261,189]	[261,189]	[252,098]
Race IAT score of Black test takers	-0.800	0.065	0.052	0.097
		(0.079)	(0.078)	(0.098)
		[43,397]	[43,397]	[38,601]
Race IAT score of Hispanic/other/unreported test takers	-0.063	0.041	0.043	-0.104
		(0.065)	(0.064)	(0.074)
		[44,356]	[44,356]	[38,288]
Panel b: Impacts on the gender-career IAT				
Gender-career IAT score of all test takers	0.107	0.012	0.016	0.046
		(0.047)	(0.047)	(0.060)
		[83,687]	[83,687]	[76,329]
Gender-career IAT score of female test takers	0.210	-0.023	-0.012	0.022
		(0.059)	(0.059)	(0.077)
		[54,826]	[54,826]	[47,638]
Gender-career IAT score of male test takers	-0.089	0.091	0.092	-0.023
		(0.096)	(0.096)	(0.142)
		[28,280]	[28,280]	[23,152]
Controls: Gender and race		Yes	Yes	Yes
FE: County		Yes	Yes	.
FE: State x cohort		Yes	Yes	.
FE: County x year		No	No	Yes
FE: State x cohort x year		No	No	Yes
Controls: Education level		No	Yes	Yes

Note: Each coefficient is the result of a separate regression using the indicated controls and fixed effects. In panel a, standardized race IAT scores are the dependent variable. Larger positive values indicate stronger Black-bad and white-good associations. In panel b, standardized gender-career IAT scores are the dependent variable. Larger positive values indicate stronger man-career and woman-family associations. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * $p<0.1$, ** $p<0.05$ and *** $p<0.01$.

Table 9: Younger cohorts in high *Sesame Street* coverage counties exhibit muted differences in explicit racial preferences reported on the race IAT survey

	(1)	(2)	(3)	(4)
	White and Black test takers		White test takers	Black test takers
Dependent variable and sub sample	**Mean**	$\hat{\beta}_1^{KL}$	$\hat{\beta}_1^{KL}$	$\hat{\beta}_1^{KL}$
Reported preference for in-group over out-group Americans (1 to 7) (1-Strongly prefers out-group, 4-likes equally, 7-strongly prefers in-group)	4.6	-0.028 (0.024)	-0.016 (0.025)	-0.175* (0.105)
		[288,344]	[247,539]	[40,376]
..........Reported preference for non-equal respondents	5.3	-0.045 (0.039)	-0.031 (0.042)	-0.130 (0.136)
		[123,781]	[97,944]	[25,443]
Difference in warmth towards in-group and out-group Americans (-10 to 10) (-10-Strongly prefers out-group, 0-likes equally, 10-strongly prefers in-group)	0.584	-0.061 (0.040)	-0.056 (0.041)	-0.090 (0.178)
		[297,737]	[255,188]	[42,109]
..........Difference in warmth for non-zero respondents	1.5	-0.231** (0.095)	-0.192* (0.112)	-0.378* (0.200)
N		[112,997]	[85,991]	[26,614]
Controls: Gender and race		Yes	Yes	Yes
FE: County		Yes	Yes	Yes
FE: State x cohort		Yes	Yes	Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1^{KL}$ estimates use $(county)$ and $(state \times cohort)$ fixed effects. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Difference in warmth is the difference in the warmth reported for the respondent's in-group and respondent's out-group on 10 point scales. The distribution of these indicators in plotted in appendix Figure A2. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table 10: Non-incumbent minority candidates gain more from electorate exposure to *Sesame Street* coverage

	(1)	*p-value of difference*	(2)	*p-value of difference*	(3)
	Ballots with no incumbents		**Minority candidate is incumbent**		**Minority candidate is non-incumbent**
Minority	0.313 (0.258)	—0.400—	0.070 (0.140)	—0.111—	0.363*** (0.120)
White	-0.318 (0.233)	—0.216—	0.015 (0.141)	—0.597—	-0.085 (0.125)
Third party	0.126* (0.073)	—0.119—	0.005 (0.030)	—0.051—	-0.077** (0.030)
Not voting	-0.121 (0.185)	—0.886—	-0.090 (0.126)	—0.507—	-0.200* (0.109)
N	[1,844]		[4,270]		[5,114]
Controls: Gender and race	Yes		Yes		Yes
FE: County x cong. district x year	Yes		Yes		Yes
FE: State x cohort x year	Yes		Yes		Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 with controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. The sample is limited to respondents voting in U.S. house white-minority ballots. Each observation in the sample has one of the mutually exclusive voting behaviors listed set to 1 and all others set to 0. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Values between columns 1-2 and 2-3 give the p-value on the interaction term of the equivalent fully interacted specification testing for heterogeneity between the two groups. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Online Appendix

Table A1: Younger cohorts in high *Sesame Street* coverage counties do not differ in their post-election survey response rates

	(1)	(2)	(3)
Dependent variable	**Dependent variable mean**	$\hat{\beta}_1$	$\hat{\beta}_1^{KL}$
Took the post-election survey	0.888	-0.004 (0.024) [59,251]	-0.011 (0.024) [64,335]
Controls: Gender and race		No	No
FE: County		.	Yes
FE: State x cohort		.	Yes
FE: County x cong. district x year		Yes	No
FE: State x cohort x year		Yes	No

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. $\hat{\beta}_1^{KL}$ estimates use $(county)$ and $(state \times cohort)$ fixed effects. Educational attainment (in years) and reported family income are continuous variables built from binned response options (6 and 12 bins respectively). Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A2: Demographic composition of candidates on major party ballots

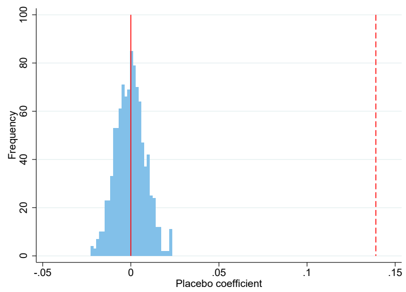
Race		Gender						Election outcomes				
								Democratic margin (ppts)			Share	
Democrat	Republican	Democrat	Republican	Respondents	Share	Ballots	Share	Mean	Median	Std. Dev.	Close (±10)	Very close (±5)
White	White	Man	Man	25,304	0.442	1,333	0.451	-6.10	-12.35	30.05	0.152	0.075
		Man	Woman	3,642	0.064	179	0.061	-0.83	1.34	28.47	0.218	0.117
		Woman	Man	11,101	0.194	516	0.175	-5.08	-11.18	27.21	0.211	0.105
		Woman	Woman	1,628	0.028	73	0.025	2.64	-0.48	27.47	0.260	0.151
		Total		41,675	0.728	2,101	0.711	-5.10	-10.97	29.21	0.176	0.089
Minority	White	Man	Man	5,552	0.097	303	0.102	9.88	10.09	36.71	0.125	0.063
		Man	Woman	611	0.011	31	0.010	18.69	20.68	34.24	0.129	0.000
		Woman	Man	3,224	0.056	166	0.056	11.82	9.57	38.48	0.084	0.036
		Woman	Woman	641	0.011	26	0.009	15.79	9.62	34.13	0.269	0.192
		Total		10,028	0.175	526	0.178	11.30	11.21	36.99	0.120	0.057
White	Minority	Man	Man	1,420	0.025	79	0.027	18.12	22.42	28.60	0.127	0.076
		Man	Woman	594	0.010	32	0.011	21.48	31.53	29.15	0.250	0.094
		Woman	Man	769	0.013	39	0.013	14.06	22.66	32.64	0.128	0.103
		Woman	Woman	309	0.005	13	0.004	19.68	19.05	18.00	0.154	0.000
		Total		3,092	0.054	163	0.055	17.93	22.42	28.93	0.153	0.080
Minority	Minority	Man	Man	1,084	0.019	81	0.027	36.39	42.40	31.33	0.111	0.074
		Man	Woman	407	0.007	26	0.009	39.79	38.28	23.39	0.115	0.115
		Woman	Man	661	0.012	45	0.015	39.64	45.38	28.70	0.089	0.067
		Woman	Woman	274	0.005	15	0.005	38.60	44.06	21.65	0.000	0.000
		Total		2,426	0.042	167	0.056	37.99	42.83	28.56	0.096	0.072
All		Man	Man	33,360	0.583	1,796	0.607	-0.42	-6.07	33.08	0.144	0.073
		Man	Woman	5,254	0.092	268	0.091	8.03	8.17	31.75	0.201	0.101
		Woman	Man	15,755	0.275	766	0.259	2.19	-6.34	32.61	0.172	0.087
		Woman	Woman	2,852	0.050	127	0.043	11.32	12.39	29.81	0.220	0.126
		Total		57,221	1.000	2,957	1.000	1.52	-3.45	32.85	0.160	0.082

Note: This table presents the composition of the 2006-2020 U.S. House ballots in our sample, organized by the demographics of their major party candidates. *Major party* ballots in our sample are defined as ballots where the two front-runners are a Democrat and a Republican, both of whom receive over 5% of their district's vote, and where candidate demographics and *Sesame Street* coverage is observed. Mean election outcomes for ballots of each type are also reported.

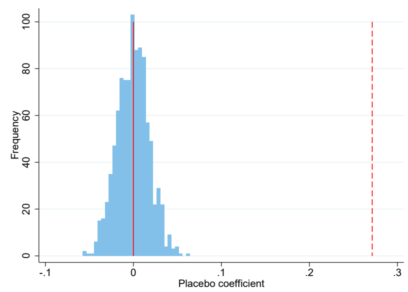
Table A3: Main results are robust to alternative specifications

	(1) Turnout	(2) Reports vote for a minority candidate	(3) Reports vote for a woman candidate	(4) White race IAT score
Main results	0.139***	0.271***	0.192***	-0.067***
	(0.040)	(0.082)	(0.058)	(0.025)
	[51,723]	[11,811]	[19,034]	[261,189]
Panel a: Main specification with added controls				
Main specifications with the addition of controls for ...				
Respondent's education	0.131***	0.279***	0.181***	-0.067***
	(0.040)	(0.080)	(0.057)	(0.025)
	[51,713]	[11,809]	[19,031]	[261,189]
Respondent's family income	0.148***	0.278***	0.185***	.
	(0.041)	(0.082)	(0.064)	
	[46,596]	[10,574]	[17,151]	
Metro area × Cohort indicators	0.140***	0.316***	0.225***	-0.067**
	(0.045)	(0.083)	(0.065)	(0.026)
	[51,723]	[11,811]	[19,034]	[261,189]
County Black population share in 1970 × Cohort indicators	0.148***	0.262***	0.207***	-0.067***
	(0.040)	(0.084)	(0.062)	(0.025)
	[51,721]	[11,811]	[19,034]	[260,003]
County low income share in 1970 × Cohort indicators	0.114***	0.255***	0.177***	-0.067***
	(0.042)	(0.083)	(0.060)	(0.025)
	[51,721]	[11,811]	[19,034]	[260,003]
County Thurmond vote share in 1948 × Cohort indicators	0.138***	0.263***	0.194***	-0.070***
	(0.040)	(0.082)	(0.058)	(0.025)
	[51,239]	[11,628]	[18,846]	[257,687]
County Democratic vote share in 1968 × Cohort indicators	0.153	0.279***	0.201***	-0.068***
	(0.000)	(0.082)	(0.060)	(0.025)
	[51,669]	[11,811]	[19,005]	[259,916]
School segregation × Cohort indicators	0.144	0.260***	0.192***	-0.073***
	(0.000)	(0.084)	(0.060)	(0.026)
	[48,908]	[11,501]	[17,800]	[240,842]
Controls: Gender and race	Yes	Yes	Yes	Gender
FE: County x cong. district x year	Yes	Yes	Yes	No
FE: State x cohort x year	Yes	Yes	Yes	No
FE: County	.	.	.	Yes
FE: State x cohort	.	.	.	Yes
Panel b: Coefficients from a simple difference-in-differences specification with no fixed effects				
$preschool69_i \times SSCov_j$	0.103***	0.130**	0.125***	-0.057***
	(0.034)	(0.065)	(0.046)	(0.019)
$SSCov_j$	-0.049*	0.029	0.037	-0.070**
	(0.028)	(0.051)	(0.040)	(0.032)
$preschool69_i$	-0.087***	-0.085*	-0.076**	0.004
	(0.022)	(0.043)	(0.031)	(0.014)
N	[57,191]	[13,113]	[21,005]	[260,080]
Controls: Gender and race	Yes	Yes	Yes	Gender

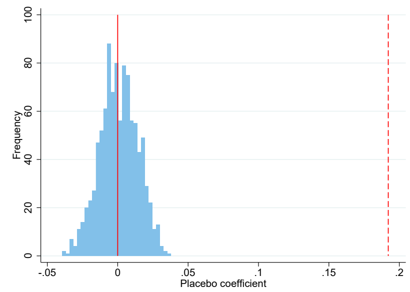
Note: In panel a, each coefficient is the result of a separate regression. In columns 1, 2, and 3, CCES data is used to estimate $\hat{\beta}_1$ from equation 1 with the addition of the indicated controls in each row. These specifications also control for respondents' race and gender, *(county × congressional district × year)* and *(state × cohort × year)* fixed effects. Column 4 estimates $\hat{\beta}_1^{KL}$ on the race IAT data with the addition of the indicated controls in each row. This specification also controls for respondents' gender and *(county)* and *(state × cohort)* fixed effects. Panel b reports the coefficients from four simple difference-in-differences specifications regressing the main outcomes on $preschool69_i \times SSCov_j$; $SSCov_j$; $preschool69_i$; and controls for race and gender, but no fixed effects. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Survey weights are used in columns 1-3. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.



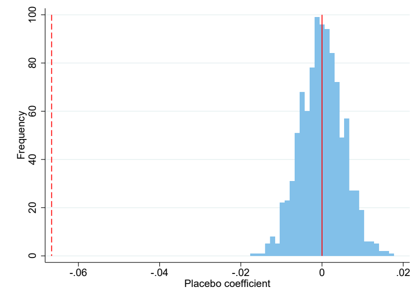
(a) Placebo cohort estimates on turnout



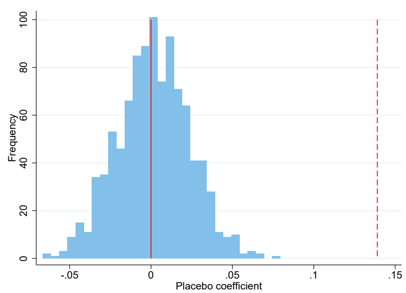
(b) Placebo cohort estimates on reported voting for minorities



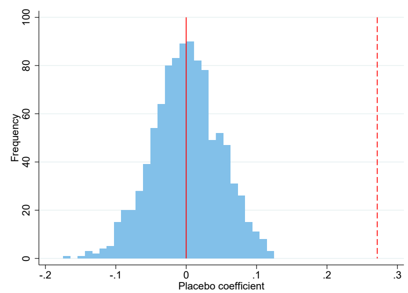
(c) Placebo cohort estimates on reported voting for women



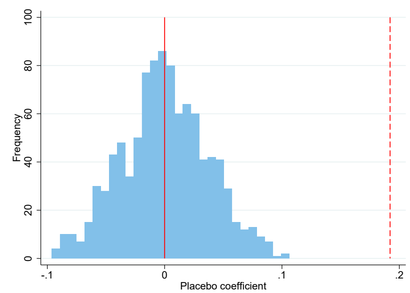
(d) Placebo cohort estimates on white IAT scores



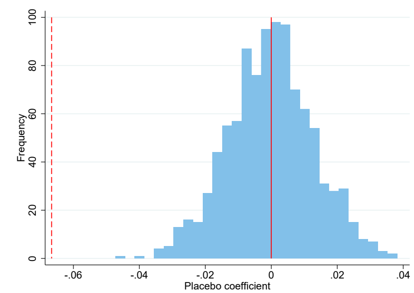
(e) Placebo coverage estimates on turnout



(f) Placebo coverage estimates on reported for minorities



(g) Placebo coverage estimates on reported voting for women



(h) Placebo coverage estimates on white IAT scores

Figure A1. Plots of 1000 placebo estimates randomizing cohort or predicted county coverage rates on turnout, reported voting for minority and women candidates, and white IAT scores

Notes: The figures plot the distribution of 1000 placebo estimates where either the observation was randomly assigned to treated or control cohorts top row) or the observation was randomly assigned a predicted *Sesame Street* coverage level from the predicted coverage distribution (bottom row). The dashed lines highlight the observed effects corresponding to the true cohort and predicted coverage rates.

Table A4: No evidence that exposure to *Sesame Street* coverage impacted moving between states, counties, cities, or towards counties with more or less *Sesame Street* coverage in 1969

	(1)	(2)	(3)	(4)	(5)
Panel a: Tests for selection in the CCES data					
	Lived in current city since under age 6	**Faces a minority-white ballot**	**Faces a woman-man ballot**	**White**	**Female**
Estimates of $\hat{\beta}_1^{KLadult}$					
$preschool69_i \times SSCov_j^{adult}$	0.046 (0.031)	0.007 (0.024)	0.011 (0.032)	-0.038 (0.037)	0.056 (0.039)
N	[34,909]	[56,850]	[56,850]	[56,850]	[56,850]
Controls: Gender and race	Yes	Yes	Yes	No	No
FE: Adult county	Yes	Yes	Yes	Yes	Yes
FE: Adult state x cohort	Yes	Yes	Yes	Yes	Yes
Coefficients from simplified specification					
$preschool69_i \times SSCov_j^{adult}$	0.037 (0.027)	0.003 (0.027)	-0.011 (0.032)	-0.021 (0.035)	0.032 (0.035)
$SSCov_j^{adult}$	0.005 (0.029)	0.090* (0.047)	0.113** (0.051)	-0.120*** (0.042)	-0.062** (0.025)
$preschool69_i$	-0.019 (0.018)	-0.007 (0.018)	0.002 (0.020)	-0.008 (0.023)	-0.020 (0.024)
N	[35,342]	[57,191]	[57,191]	[57,191]	[57,191]
Controls: Gender and race	Yes	Yes	Yes	No	No
FE: Adult county	No	No	No	No	No
FE: Adult state x cohort	No	No	No	No	No
Dependent variable mean	0.101	0.229	0.367	0.801	0.530
Panel b: Tests for selection in the PSID data					
	Attrits from PSID panel	**Lives in childhood state**	**Lives in childhood county**	**$SSCov_j^{adult}$ of adult county**	
Estimates of $\hat{\beta}_1^{KLchild}$					
$preschool69_i \times SSCov_j^{child}$	-0.024 (0.095)	0.073 (0.382)	-0.082 (0.293)	-0.024 (0.097)	
N	[4,190]	[1,052]	[1,052]	[1,044]	
Controls: Gender and race	Yes	Yes	Yes	Yes	
FE: Childhood county	Yes	Yes	Yes	Yes	
FE: Childhood state x cohort	Yes	Yes	Yes	Yes	
Dependent variable mean	0.721	0.746	0.473	0.630	

Note: Panel a uses CCES data to estimate differences between younger and older cohorts residing in high coverage counties on outcomes that could signal selection effects. $SScov_j^{adult}$ is the coverage rate in respondents' county of residence in the CCES when surveyed as adults. The top portion of panel a presents estimates of $\hat{\beta}_1^{KLadult}$ with $(adult\ county)$ and $(adult\ state \times cohort)$ fixed effects. Race and gender controls are used in columns 1-3. Each coefficient is the result of a separate regression. The bottom portion of panel a reports the coefficients from five simple difference-in-differences specifications regressing these outcomes on $preschool69_i \times SSCov_j^{adult}$; $SSCov_j^{adult}$; $preschool69_i$; and controls for race and gender in columns 1-3; with no fixed effects. Panel b tests for impacts of *Sesame Street* exposure on migration using data from the PSID where we observe both $SScov_j^{child}$ and $SScov_j^{adult}$. Each coefficient is the result of a separate regression estimating $\hat{\beta}_1^{KLchild}$ with $(child\ county)$ and $(child\ state \times cohort)$ fixed effects, and race and gender controls. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing outcome and control variables are omitted. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A5: Coverage rates of sending and destination counties are correlated for inter-county migrants

	(1)	(2)	(3)	(4)
	Destination county coverage rate			
	All observations	**All movers**	**Movers to neighboring counties**	**Movers to non-neighboring counties**
Sending county coverage rate (PSID)	0.547***	0.242***	0.789***	0.101*
	(0.040)	(0.050)	(0.036)	(0.054)
	[1,189]	[622]	[186]	[436]
Sending county coverage rate (Census)		0.347***	0.818***	0.124***
		(0.002)	(0.005)	(0.002)
		[236,880]	[13,964]	[222,916]

Note: For correlations using PSID data in row 1, each observation represents a respondent with observations used to estimate the correlation reported in brackets. PSID regressions are weighted by survey weights. For correlations using census data in row 2, each observation consists of migration flows observed between county pair combinations by the census between 2016 and 2020. Census regressions are weighted by the number of migrants between that sending and destination county. Numbers in brackets report the county pair observations used in each estimation. Standard errors are reported in parenthesis, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A6: Effects are generally larger for respondents who have resided in their city since before age 6

	(1)	(2)	*p-value of difference*	(3)
	$\hat{\beta}_1^{KL}$	**Respondent**		
Dependent indicator variable		**Lived in current city since under age 6**		**Moved to current city at age 6 or later**
Validated turnout	0.119**	0.234	—0.427—	0.102**
	(0.048)	(0.166)		(0.051)
	[34,909]	[3,101]		[31,308]
Active voter registration	0.113**	0.303*	—0.207—	0.089*
	(0.050)	(0.171)		(0.051)
	[30,372]	[2,602]		[27,285]
Reports voting for a minority candidate	0.263**	0.848***	—0.004—	0.216**
	(0.108)	(0.213)		(0.104)
	[7,489]	[574]		[6,674]
Reports voting for a female candidate	0.144**	-0.217	—0.216—	0.148**
	(0.069)	(0.316)		(0.074)
	[12,063]	[823]		[10,871]
Controls: Gender and race	Yes	Yes		Yes
FE: County	Yes	Yes		Yes
FE: State x cohort	Yes	Yes		Yes

Note: Each coefficient is the result of a separate regression. Given the small sub-samples, we estimate $\hat{\beta}_1^{KL}$ with $(county)$ and $(state \times cohort)$ fixed effects. All regressions control for the race and gender of respondents. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Column 1 uses all 35,372 observations where the duration of residence is observed. Column 2 limits the sample to respondents who report living in their city since before age 6. Column 3 limits the sample to respondents who report living in their city since age 6 or later. Values between columns 2-3 give the p-value on the interaction term of the equivalent fully interacted specification testing for heterogeneity between the two groups. All estimates employ survey weights. Standard errors are reported in parenthesis, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A7: No evidence that *Sesame Street* coverage impacted selection into taking the race IAT but it did impact selection into taking the gender-career IAT

	(1)	(2)	(3)
	Share of county's test takers in treated cohorts		
	Race IAT **All test takers**	**Race IAT** **White test takers**	**Gender-career IAT** **All test takers**
Predicted *Sesame Street* coverage rate ($SScov_j$	0.0080 (0.0078)	-0.0018 (0.0092)	0.0340** (0.0151)
Constant	0.55*** (0.01)	0.55*** (0.01)	0.54*** (0.01)
County observations	2,856	2,810	2,372
Total test takers	350,080	261,412	84,479

Note: Each observation represents a county. Coefficients measure the correlation between the the share of test takers in the county coming from treated cohorts with $SScov_j$, the county's predicted coverage rate, weighted by the total number of test takers in the 1959-1968 cohorts. Standard errors are reported in parentheses with the following significance indicators: * $p<0.1$, ** $p<0.05$ and *** $p<0.01$.

Table A8: Differences between younger and older cohort in high *Sesame Street* coverage counties in the reported reasons for not voting

	(1)	(2)	(3)
Dependent indicator variable	**Dependent indicator mean**	$\hat{\beta}_1$	$\hat{\beta}_1^{KL}$
All reasons	0.151	-0.128***	-0.128***
		(0.033)	(0.035)
		[47,592]	[52,017]
...Did not like the candidates	0.024	-0.025**	-0.028**
		(0.012)	(0.012)
		[47,592]	[52,017]
...I am not registered	0.024	-0.030*	-0.024
		(0.016)	(0.016)
		[47,592]	[52,017]
...I am not interested	0.016	-0.014	-0.011
		(0.012)	(0.012)
		[47,592]	[52,017]
...Sick or disabled	0.017	-0.009	0.006
		(0.013)	(0.014)
		[47,592]	[52,017]
...I did not feel that I knew enough about the choices	0.014	-0.001	-0.019
		(0.011)	(0.016)
		[47,592]	[52,017]
...All other reasons listed	0.057	-0.048**	-0.052**
		(0.020)	(0.022)
		[47,592]	[52,017]
Controls: Gender and race		Yes	Yes
FE: County		.	Yes
FE: State x cohort		.	Yes
FE: County x cong. district x year		Yes	No
FE: State x cohort x year		Yes	No

Note: Each coefficient is the result of a separate regression. Outcome variables are coded as 0 if the respondent reports voting, or not-voting for a different reason, and 1 if the respondent gives the listed reason for non-turnout. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. $\hat{\beta}_1^{KL}$ estimates use $(county)$ and $(state \times cohort)$ fixed effects. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Other reasons for non-turnout include bad weather, not knowing why, lack of identification, lack of knowledge about polling locations, forgetting to vote, fear of covid exposure, non-receipt of absentee ballots, being out of town, long lines at polling stations, dismissal at the polling station, lack of transportation, being too busy, and other reasons. Reason for non-turnout was not asked in the 2006 survey, and is only asked to respondents who report not voting. All estimates employ survey weights. Standard errors are reported in parenthesis, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A9: No clear heterogeneity in turnout and registration effects by respondent minority status and sex

	(1)	*p-value of difference*	(2)	(3)	*p-value of difference*	(4)
	Respondent is			**Respondent is**		
Dependent indicator variable	**White**		**Minority**	**Female**		**Male**
Panel a: Heterogeneity in turnout effects by respondent minority status and sex						
Verified general election turnout	0.132*** (0.044)	—0.602—	0.064 (0.130)	0.148** (0.061)	—0.912—	0.139** (0.055)
Self-reported election turnout	0.124*** (0.034)	—0.448—	0.043 (0.106)	0.136*** (0.048)	—0.399—	0.080* (0.045)
Inconsistent self-reported voting status with validation	-0.011 (0.036)	—0.507—	-0.091 (0.121)	-0.038 (0.052)	—0.842—	-0.052 (0.050)
N	[40,376]		[7,688]	[24,614]		[21,683]
Panel b: Heterogeneity in registration effects by respondent minority status and sex						
Verified active voter registration	0.074* (0.042)	—0.477—	-0.005 (0.108)	0.127** (0.058)	—0.577—	0.080 (0.060)
Self-reported voter registration	0.040 (0.025)	—0.528—	-0.019 (0.094)	0.053 (0.038)	—0.821—	0.043 (0.027)
Inconsistent self-reported registration status with validation	-0.048 (0.038)	—0.715—	-0.002 (0.125)	-0.080 (0.055)	—0.925—	-0.087 (0.056)
N	[37,330]		[7,049]	[22,911]		[19,918]
Controls: Gender and race	Gender only		Yes	Race only		Race only
FE: County x cong. dist. x year	Yes		Yes	Yes		Yes
FE: State x cohort x year	Yes		Yes	Yes		Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Values between columns 1-2 and 3-4 give the p-value on the interaction term of the equivalent fully interacted specification testing for heterogeneity between the two groups. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A10: No evidence that turnout effects differ by ballot composition

Dependent variable and ballot sub-sample	(1) Dependent indicator mean	(2) $\hat{\beta}_1$
Verified general election turnout	0.634	0.139*** (0.040) [51,723]
... turnout when ballot is white man vs. white man	0.618	0.178** (0.074) [22,143]
... turnout when ballot is white vs. white	0.632	0.163*** (0.050) [37,139]
... turnout when ballot is minority vs. white	0.640	0.126 (0.080) [11,811]
... turnout when ballot is man vs. man	0.620	0.158*** (0.056) [29,573]
... turnout when ballot is woman vs. man	0.649	0.121* (0.064) [19,034]
... turnout when ballot is midterm elections	0.565	0.171*** (0.054) [25,032]
Controls: Gender and race		Yes
FE: County x cong. district x year		Yes
FE: State x cohort x year		Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A11: No evidence that younger cohorts in high *Sesame Street* coverage counties differ in their educational attainment or family income

	(1)	(2)	(3)	(4)	(5)
	CCES			**Race IAT**	
Dependent variable	**Mean**	$\hat{\beta}_1$	$\hat{\beta}_1^{KL}$	**Mean**	$\hat{\beta}_1^{KL}$
Educational attainment (years)	14	0.167 (0.244) [51,713]	0.283 (0.264) [56,839]	16	0.034 (0.059) [327,498]
Reported family income (in $1,000)	69	3.482 (3.831) [46,596]	1.895 (3.154) [51,729]		.
Controls: Gender and race		Yes	Yes		Yes
FE: County		.	Yes		Yes
FE: State x cohort		.	Yes		Yes
FE: County x cong. district x year		Yes	No		No
FE: State x cohort x year		Yes	No		No

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. $\hat{\beta}_1^{KL}$ estimates are estimated using the same specification as Kearney and Levine (2019), with $(county)$ and $(state \times cohort)$ fixed effects. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Educational attainment (in years) and reported family income are continuous variables built from binned response options (6 and 12 bins respectively). Estimates using the CCES data employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A12: No evidence that younger cohorts in high *Sesame Street* coverage counties differ in reported indicators of civic engagement

Dependent variable	(1) **Dependent indicator mean**	(2) $\hat{\beta}_1$	(3) $\hat{\beta}_1^{KL}$
Donated blood	0.135	0.035	0.015
		(0.030)	(0.029)
		[43,637]	[47,405]
Was ever a union member	0.279	-0.021	-0.029
		(0.040)	(0.038)
		[47,646]	[52,114]
Was ever in the military	0.134	0.017	0.003
		(0.032)	(0.028)
		[51,723]	[56,850]
Controls: Gender and race		Yes	Yes
FE: County		.	Yes
FE: State x cohort		.	Yes
FE: County x cong. district x year		Yes	No
FE: State x cohort x year		Yes	No

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. $\hat{\beta}_1^{KL}$ estimates use $(county)$ and $(state \times cohort)$ fixed effects. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing outcome and control variables are omitted. Dependent variables are indicator variables. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * $p<0.1$, ** $p<0.05$ and *** $p<0.01$.

Table A13: Impacts on general election participation are larger for non-voters in primary elections

	(1)	*p-value of difference*	(2)
	Primary turnout is		
	Verified		**Not-verified**
Panel a: Heterogeneity in turnout effects by verified primary election turnout			
Verified general election turnout	0.051** (0.024)	—0.067—	0.162*** (0.056)
Self-reported election turnout	0.012 (0.017)	—$<$ 0.001—	0.183*** (0.048)
Inconsistent self-reported voting status with validation	-0.025 (0.021)	—0.369—	0.023 (0.049)
N	[13,937]		[29,421]
Panel b: Heterogeneity in registration effects by verified primary election turnout			
Verified active voter registration	0.015 (0.011)	—0.072—	0.116** (0.055)
Self-reported voter registration	0.003 (0.005)	—0.045—	0.074** (0.035)
Inconsistent self-reported registration status with validation	-0.017 (0.012)	—0.337—	-0.069 (0.053)
N	[13,937]		[29,421]
Controls: Gender and race	Yes		Yes
FE: County x cong. dist. x year	Yes		Yes
FE: State x cohort x year	Yes		Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Values between columns 1 and 2 give the p-value on the interaction term of the equivalent fully interacted specification testing for heterogeneity between the two groups. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A14: Among major party voters, no evidence of heterogeneity in increased reported voting for minority, women, and Democratic candidates by respondents' turnout validation

	(1)	(2)	(3)	*p-value of difference*	(4)
		All major party	Major party voters whose turnout is		
	Share	voters	Validated		Not-Validated
Panel a: Reported voting of major party voters on white-minority ballots					
Minority	0.507	0.277*** (0.092)	0.250** (0.119)	—0.652—	0.143 (0.220)
N		[9,200]	[6,467]		[1,310]
Panel b: Reported voting of major party voters on woman-man ballots					
Woman	0.488	0.154** (0.066)	0.106 (0.082)	—0.772—	0.170 (0.218)
N		[14,994]	[10,851]		[2,092]
Panel c: Reported voting of major party voters by candidate party					
Democrat	0.495	0.082* (0.043)	0.075 (0.051)	—0.710—	0.019 (0.149)
N		[40,659]	[29,141]		[6,972]
Controls: Gender and race		Yes	Yes		Yes
FE: County x cong. dist. x year		Yes	Yes		Yes
FE: State x cohort x year		Yes	Yes		Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. The sample is limited to respondents who report a major party vote for the U.S. House on a white-minority ballot (panel a), a man-woman ballot (panel b), or all ballots (panel c). Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Values between columns 3 and 4 give the p-value on the interaction term of the equivalent fully interacted specification testing for heterogeneity between the two groups. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

A1 Persuasion rate

We calculate the persuasion rate of *Sesame Street* exposure on validated turnout using the CCES sample from 2006–2020. Following the binary-outcome persuasion framework of DellaVigna and Gentzkow (2010), the persuasion rate is defined as

$$f = 100 \times \frac{y_T - y_C}{(e_T - e_C)(1 - y_0)},$$

where $y_T - y_C$ denotes the causal effect of exposure on the outcome of interest (validated voter turnout), $e_T - e_C$ is the difference in exposure rates between treated and comparison groups, and $1 - y_0$ represents the share of individuals who would not have adopted the behavior absent the message (i.e., the persuadable population).

In our setting, a 30 percentage point increase in predicted *Sesame Street* coverage raises validated turnout by approximately 4.2 percentage points, implying $(y_T - y_C) = 0.042$ and $(e_T - e_C) = 0.30$. Because the counterfactual turnout rate in the absence of exposure is not directly observed, we follow DellaVigna and Gentzkow in approximating y_0 with the control-group turnout rate, which is about 0.63. Substituting these values yields

$$f \approx 100 \times \frac{0.042}{0.30} \times \frac{1}{1 - 0.634} \approx 38.3.$$

Applying the same approach to self-reported turnout in the CPS sample from 1982–1992 yields a persuasion rate of approximately 16.5:

$$f \approx 100 \times \frac{0.028}{0.30} \times \frac{1}{1 - 0.433} \approx 16.5.$$

A2 Implications for election outcomes

It is interesting to consider how these effects may have impacted election outcomes. Our estimates are based on a small share of the electorate born between 1959 and 1968, making projections onto electoral outcomes challenging for several reasons. First, broadcast availability of the show changed substantially over subsequent decades. More critically, although cohorts born after 1968 watched and continue to watch *Sesame Street* in large numbers, their counterfactual activities and programming options are quite different from those of the early 1970s making the relative impact of *Sesame Street* in later decades unclear. One could also argue that *Sesame Street* may have had "general equilibrium effects" as other shows adapted to compete with *Sesame Street*'s success, yet estimating such effects is beyond the scope of this paper.

Because of these confounds, we estimate impacts on election outcomes very conservatively, assuming that only individuals born between 1964 and 1968 are treated. Using this assumption, and the full sample of all CCES respondents from all cohorts, we calculate a measure of electorate exposure for each election in each congressional district as

$$ElectorateExposure_{dy} = \frac{\sum_1^N SScov_j * \mathbb{1}(preschool69_i = 1)}{N_{dy}}.$$

An election outcome is considered impacted if $WinMargin_{dy} - ElectorateExposure_{dy} * \hat{\beta}_1 < 0$, where $WinMargin_{dy}$ is the difference in the received vote share of a winning minority or woman candidate and their major party opponent, and $\hat{\beta}_1$ is the coefficient in the relevant first row of either panel a or b of column 2, Table 4. Using this conservative approach we find that the show may have contributed to the electoral victories of 6 minority candidates (1.75% of minority wins) and 7 women candidates (1.57% of woman wins).

Table A15: ***Sesame Street*** **coverage and cohort differences in policy preferences**

	(1)	(2)	(3)	(4)
Dependent variable	**Mean**	**All**		
Panel a: Policy questions in the CCES survey			**Respondent is**	
Index of support for environmental policies (Scale from 0 to 1)	0.596	0.019 (0.025) [56,680]		
Supportive of allowing gays and lesbians to marry legally	0.558	0.122** (0.050) [34,849]		
			Male	***Female***
Index of support for abortion rights (Scale from 0 to 1)	0.689	0.009 (0.029) [56,630]	-0.010 (0.044) [26,334]	0.006 (0.043) [29,782]
			Non-hispanic	***Hispanic***
Index for support of less restrictive immigration policies (Scale from 0 to 1)	0.448	-0.078** (0.033) [38,609]	-0.078** (0.032) [36,713]	-0.028 (0.170) [1,614]
Panel b: Race relations and race policy questions in the CCES survey				
			White	***Black***
Belief in structural racism index Respondent agrees with statements indicating that Blacks face barriers to socio-economic advancement compared to whites (scale 1 - 5)	2.7	-0.083 (0.118) [47,378]	0.070 (0.114) [37,679]	-0.722** (0.310) [4,741]
Supports minority affirmative action programs in employment and college admissions (scale 1 - 4)	2.1	-0.070 (0.097) [24,726]	0.033 (0.105) [19,538]	-0.433 (0.515) [2,403]
Panel c: Race relations and race policy questions in the IAT survey				
			White	***Black***
Supports affirmative action index Indicates support on questions regarding affirmative action in employment and college admissions (scale 0-1)	0.229	0.064** (0.032) [23,022]	0.057 (0.038) [16,525]	0.037 (0.132) [3,312]
Justifies racial profiling index Indicates racial profiling can be justified in certain situations (scale 0-1)	0.091	0.036 (0.023) [23,041]	0.032 (0.027) [16,568]	0.077 (0.091) [3,257]
Controls: Gender and race		Yes	Yes	Yes
FE: County		Yes	Yes	Yes
FE: State x cohort		Yes	Yes	Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1^{KL}$ estimates use $(county)$ and $(state \times cohort)$ fixed effects. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Indices are built as the average response to questions pertaining to that topic that were administered in the respondent's survey year. Estimates on individual questions are reported in appendix Tables A16 and A17. Estimates using the CCES data employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A16: Question estimates of how younger cohorts in high *Sesame Street* coverage counties differ in policy views

		(1)	(2)	(3)	(4)
	Dependent variable	**Mean**	**All**	**Respondent is**	
Environmental policy					
A	In a trade-off between environmental protection versus jobs and the economy: priority is the environment (1) or the economy (5). (Scale from 1 to 5)	2.9	-0.140 (0.129) [22,604]		
B	Supports strengthening the Environmental Protection Agency enforcement of the Clean Air Act and Clean Water Act even if it costs U.S. jobs	0.524	0.026 (0.050) [33,435]		
C	Supports giving the Environmental Protection Agency power to regulate carbon dioxide emissions	0.631	0.029 (0.049) [33,413]		
D	Supports requiring a minimum amount of renewable fuels (wind, solar, and hydroelectric) in the generation of electricity even if electricity prices increase somewhat	0.585	0.038 (0.046) [33,442]		
Abortion policy				***Male***	***Female***
E	Supports making all abortions illegal	0.127	-0.017 (0.028) [49,724]	0.007 (0.041) [23,018]	-0.009 (0.039) [26,196]
F	Supports a woman always being able to obtain an abortion as a matter of personal choice	0.538	0.018 (0.041) [56,619]	0.009 (0.063) [26,331]	0.015 (0.058) [29,773]
Immigration policy				***Non-hispanic***	***Hispanic***
G	Supports fining U.S. businesses that hire illegal immigrants	0.675	-0.025 (0.066) [15,328]	0.019 (0.067) [14,675]	-0.964** (0.378) [469]
H	Supports granting legal status to all immigrants who have held jobs and paid taxes for at least 5 years, and not been convicted of any felony crimes.	0.481	-0.077* (0.046) [38,601]	-0.066 (0.046) [36,705]	-0.002 (0.281) [1,614]
I	Supports increasing the number of border patrols on the U.S.-Mexican border	0.621	0.073 (0.047) [38,595]	0.085* (0.045) [36,699]	-0.157 (0.233) [1,614]
J	Supports allowing police to question anyone they think may be in the country illegally	0.425	0.111* (0.058) [20,173]	0.141** (0.059) [19,155]	0.455 (0.388) [773]
	Controls: Gender and race		Yes	Yes	Yes
	FE: County		Yes	Yes	Yes
	FE: State x cohort		Yes	Yes	Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1^{KL}$ estimates are reported using $(county)$ and $(state \times cohort)$ fixed effects. All outcome variables other than question A are binary indicators set to 1 if the respondent supports the stated policy. Question A is a scale from 1 (prioritize the environment) to 5 (prioritize the economy). Not all statements are administered in all survey years. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

Table A17: Question estimates of how younger cohorts in high *Sesame Street* coverage counties differ in responses to race policy and race relations questions

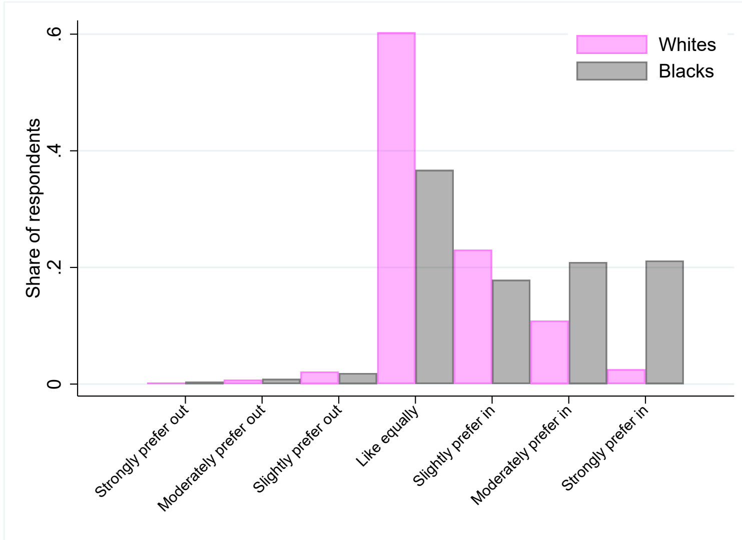
		(1)	(2)	(3)	(4)
				Respondent is	
	Dependent variable	Mean	All	White	Black
Panel a: CCES survey					
Structural disadvantage questions					
A	Irish, Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors.	3.6	-0.100	-0.219*	1.376***
			(0.128)	(0.121)	(0.492)
			[38,769]	[30,766]	[3,898]
B	Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class.	2.7	-0.132	0.056	-0.625
			(0.139)	(0.142)	(0.516)
			[38,780]	[30,772]	[3,910]
C	White people in the U.S. have certain advantages because of the color of their skin.	3.1	0.035	0.076	-0.322
			(0.165)	(0.185)	(0.556)
			[26,632]	[21,459]	[2,297]
D	It's really a matter of some people not trying hard enough, if blacks would only try harder they could be just as well off as whites.	3.0	-0.354	-0.283	4.186**
			(0.252)	(0.262)	(1.658)
			[8,112]	[6,626]	[490]
E	Over the past few years, blacks have gotten less than they deserve.	2.6	-0.083	0.122	-1.680
			(0.237)	(0.267)	(1.233)
			[8,120]	[6,633]	[490]
Other race relations questions					
F	Racial problems in the U.S. are rare, isolated situations.	2.4	-0.039	-0.098	0.169
			(0.148)	(0.166)	(0.605)
			[26,137]	[21,046]	[2,245]
G	I am angry that racism exists.	4.3	0.145	-0.027	-0.143
			(0.191)	(0.231)	(0.691)
			[7,945]	[6,290]	[560]
H	I often find myself fearful of people of other races.	2.1	0.079	-0.043	-0.373
			(0.248)	(0.251)	(1.489)
			[7,937]	[6,281]	[562]
Panel b: Race IAT survey					
Affirmative action questions					
A	A college admissions officer considers applications from African American and European American applicants with similar credentials and cannot accept all. Should the admissions officer more often accept African American than European American applicants?	0.196	0.045	0.010	0.407**
			(0.037)	(0.045)	(0.180)
			[17,934]	[12,953]	[2,416]
B	A corporate personnel officer is evaluating an African American and a European American job applicant who are identically qualified except the European American has more prior experience in related work. Is there a reasonable justification for this personnel officer hiring the African American applicant rather than the European American?	0.237	0.045	0.025	-0.145
			(0.041)	(0.053)	(0.170)
			[17,985]	[12,901]	[2,483]
Racial profiling questions					
C	Air passengers arriving in the United States must pass through a checkpoint where customs officers may examine contents of baggage in search of contraband such as illegal drugs. Should customs officers be more ready to examine contents of baggage for an African American passenger than a European American passenger?	0.032	-0.017	-0.022	-0.006
			(0.019)	(0.020)	(0.065)
			[18,091]	[13,023]	[2,473]
D	Do cab drivers in big cities who occasionally choose to pass by an African American person seeking a cab ride, then pick up a nearby European American person, have a reasonable justification for doing this?	0.150	0.092**	0.085*	0.177
			(0.036)	(0.046)	(0.140)
			[17,905]	[12,914]	[2,405]
	Controls: Gender and race		Yes	Yes	Yes
	FE: County		Yes	Yes	Yes
	FE: State x cohort		Yes	Yes	Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1^{KL}$ estimates use $(county)$ and $(state \times cohort)$ fixed effects. For CCES questions, the outcome variables represents the degree to which the respondent agrees with the listed statement with responses homogenized across years to fit a 1-5 point scale with: *1-Strongly disagree; 2-Somewhat disagree; 3- Neither agree nor disagree; 4-Somewhat agree; 5-Strongly agree*. Not all statements are administered in all survey years. For race IAT questions, the outcome variable is a binary indicating agreement with the question. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Estimates using the CCES data employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.

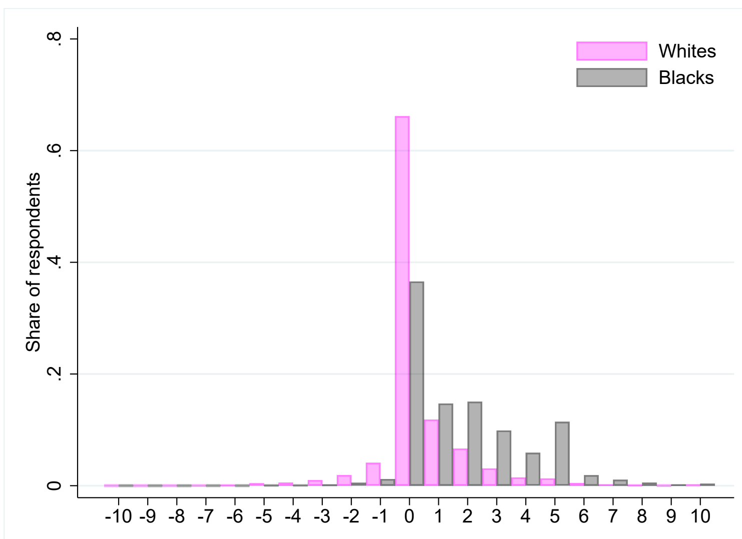
Table A18: No evidence that younger cohorts in high *Sesame Street* coverage counties differ in their reported voting for incumbents

	(1)	(2)	(3)	(4)	(5)
				Major party voters	
	Share	$\hat{\beta}_1$	$\hat{\beta}_1^{KL}$	**All**	**Validated**
Incumbent	0.495	0.087* (0.044)	0.087** (0.043)	0.008 (0.050)	-0.044 (0.057)
Non-incumbent	0.313	0.051 (0.040)	0.041 (0.039)		
Third party	0.025	-0.029** (0.012)	-0.023* (0.013)		
Not voting	0.167	-0.108*** (0.034)	-0.105*** (0.036)		
N		[43,968]	[48,252]	[34,623]	[24,615]
Controls: Gender and race		Yes	Yes	Yes	Yes
FE: County		.	Yes	.	.
FE: State x cohort		.	Yes	.	.
FE: County x cong. district x year		Yes	No	Yes	Yes
FE: State x cohort x year		Yes	No	Yes	Yes

Note: Each coefficient is the result of a separate regression. All regressions control for the race and gender of respondents. $\hat{\beta}_1$ is estimated using equation 1 which controls for $(county \times congressional\ district \times year)$ and $(state \times cohort \times year)$ fixed effects. $\hat{\beta}_1^{KL}$ estimates use $(county)$ and $(state \times cohort)$ fixed effects. The sample is limited to respondents voting in U.S. House elections that feature a major party incumbent and non-incumbent candidate. Each observation in the sample has one of the mutually exclusive voting behaviors listed set to 1 and all others set to 0. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. All estimates employ survey weights. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.



(a) Preference for in-group and out-group Americans



(b) Difference in warmth towards in-group and out-group Americans

Figure A2. Distribution of explicit racial preferences between European and African Americans

Note: Figure a presents responses to a seven option question on preferences for European and African Americans asked in the race IAT survey. Responses are flipped for African Americans for comparability. Figure b plots the net difference between race IAT test takers' rated warmth (0-coldest to 10-warmest) on two questions, one for their in-group and the other their out-group. Negative values indicates greater expressed explicit warmth towards their out-group while positive values indicates greater expressed explicit warmth towards their in-group.

A3 Heterogeneity by county characteristics

How did the impacts of *Sesame Street* differ based on the racial context in which it aired? A priori it is not clear whether media "contact" with minority role models would have a larger effect in areas where opportunities for other forms of contact were high or low. Where contact opportunities were few, the show may have filled a vacuum allowing for a large effect. Alternatively, effects may have been larger in areas where preschool age exposure to the show could later be reinforced through other interactions. This question is of academic interest as it would shed light on contact theory mechanisms.

To explore this, using the sub-sample of white respondents, we estimate modified versions of our specifications that include the relevant interactions with a county characteristics ($CountyChar_j$),

$$\begin{aligned} VotesMinority_{icjdy} &= \alpha_0 + \alpha_1(preschool69_i * SSCov_j) + \alpha_2(preschool69_i * SSCov_j * CountyChar_j) \\ &+ \alpha_3(preschool69_i * CountyChar_j) + \alpha_4 X_i + \gamma_{scy} + \delta_{jdy} + \epsilon_i, \end{aligned}$$

$$\begin{aligned} IATscore_{icj} &= \alpha_0^{KL} + \alpha_1^{KL}(preschool69_i * SSCov_j) + \alpha_2^{KL}(preschool69_i * SSCov_j * CountyChar_j) \\ &+ \alpha_3^{KL}(preschool69_i * CountyChar_j) + \alpha_4^{KL} X_i + \gamma_{sc}^{KL} + \delta_j^{KL} + \epsilon_i. \end{aligned}$$

Estimates in column 2 of Table A19 report α_1 and α_2 on reporting a vote for a minority candidate using several different county characteristics that help characterize the racial context of counties. Estimates of α_1^{KL} and α_2^{KL} on race IAT scores are reported in column 4. Subsequent odd numbered columns report $\hat{\beta}_1$ and $\hat{\beta}_1^{KL}$ using the same sample of observations, as not all county characteristics are available for all counties.

From a policy perspective, α_2 and α_2^{KL} inform us on whether increasing coverage rates and show access might affect certain types of counties more than others. However, it must be highlighted that using α_2 and α_2^{KL} to understand how seeing the show might differently impact individuals in differing county environments is challenging and subject to ambiguity. Heterogeneity in response to predicted coverage rates could reflect a heterogeneous response to watching the show, with the environment amplifying or subduing the effects of the show. Alternatively, county characteristics could impact viewership rates and who selects into viewing the show. Given the lack of national county level data on *Sesame Street* viewership it is not possible for us to fully disentangle these competing mechanisms. Nevertheless, data on ratings for preschool age viewers is available for 28 metro media market areas. Using this, we estimate

$$Ratings_m = \pi_0 + \pi_1 MediaMarketSSCov_m + \pi_2 MediaMarketChar_m + \epsilon_m, \quad (1)$$

where $Ratings_m$ is the number of viewers in the metropolitan area m, between the ages of 2 and 5 who watched *Sesame Street*, as reported in Haydon (1973), divided by the population count for this age group in the 1970 census. $MediaMarketSSCov_m$ and $MediaMarketChar_m$ are the metropolitan area's *Sesame Street* coverage rate and the area's characteristic, calculated as the population weighted mean of these attributes over the media market's composing counties. Estimates of π_1 and π_2 are reported in column 1 of Table A19.

We examine heterogeneity by the standardized 1970 Black population share of counties, the standardized county vote share received by the staunchly segregationist presidential candidate Strom Thurmond in 1948, the standardized county vote share received by the democratic presidential candidate in 1968, the county's metro area's schools' standardized measures of white-Black segregation, as well as an indicator for being in a state that required school segregation prior to 1954.[1] Overall, we find little evidence of meaningful heterogeneity in our estimates by county characteristics. For all estimates of α_2 and α_2^{KL} we fail to reject the null of no heterogeneity by examined county characteristics. From a policy perspective, there is no evidence that increasing coverage rates and access to the show affected certain types of counties more than others.

Heterogeneity in the effects of watching the show could be masked by how these

[1]Data on the 1970 Black population share of counties is sourced from the 1972 County and City Data Book, which is available as part of the replication package for Kearney and Levine (2019). County vote share data are available from Clubb et al. (1987). Metro area school measures of white-Black segregation is available for download from the American Communities Project (Logan 2017). The measure of Black-white segregation is the index of dissimilarity which gives the percentage of children in one group who would have to attend a different school to achieve racial balance across schools in the city.

characteristics correlate with viewership rates. Estimates of π_2 using our limited sample are imprecise but suggest a small positive correlation between metro area viewership and the area's Democratic vote share in 1968. Such differences in viewership could mask heterogeneous effects of the show on the children who actually watched the show. Given our data limitations, it is not possible for us to properly evaluate this question. We leave it to be examined in future research.

Table A19: No evidence of clear heterogeneity in effects on white respondents by racial characteristics of counties

	(1)		(2)	(3)	(4)	(5)
	Media market preschool age viewership		White reported voting for a minority candidate		White race IAT score	
			With county characteristic	$\hat{\beta}_1$ on same sample	With county characteristic	$\hat{\beta}_1^{KL}$ on same sample
$SSCov_m$	0.595***	$SSCov_j \times preschool69_i$		0.312***		-0.067***
	(0.186)			(0.095)		(0.025)
	[28]			[8,055]		[261,189]
Standardized county Black population share in 1970						
$SSCov_m$	0.615***	$SSCov_j \times preschool69_i$	0.304***	0.312***	-0.067***	-0.067***
	(0.185)		(0.095)	(0.095)	(0.025)	(0.025)
$Std.\ 1970\ Black\ pop.\ share_m$	-0.051	$SSCov_j \times preschool69_i \times Std.\ 1970\ Black\ pop.\ share_j$	0.110		-0.001	
	(0.074)		(0.140)		(0.032)	
N	[28]		[8,055]	[8,055]	[260,003]	[260,003]
Standardized county Thurmond vote share in 1948						
$SSCov_m$	0.596***	$SSCov_j \times preschool69_i$	0.303***	0.289***	-0.064**	-0.069***
	(0.192)		(0.097)	(0.096)	(0.026)	(0.025)
$Std.\ 1948\ Thurmond\ vote\ share_m$	-0.009	$SSCov_j \times preschool69_i \times Std.\ 1948\ Thurmond\ vote\ share_j$	0.098		0.022	
	(0.079)		(0.162)		(0.039)	
N	[28]		[7,949]	[7,949]	[257,687]	[257,687]
Standardized county Democrat vote share in 1968						
$SSCov_m$	0.546***	$SSCov_j \times preschool69_i$	0.361***	0.312***	-0.070**	-0.067***
	(0.175)		(0.101)	(0.095)	(0.028)	(0.025)
$Std.\ 1968\ democratic\ vote\ share_m$	0.075*	$SSCov_j \times preschool69_i \times Std.\ 1968\ democratic\ vote\ share_j$	-0.094		0.006	
	(0.040)		(0.104)		(0.024)	
N	[28]		[8,055]	[8,055]	[259,916]	[259,916]
Standardized metro area school white-Black segregation						
$SSCov_m$	0.530***	$SSCov_j \times preschool69_i$	0.346***	0.331***	-0.071***	-0.073***
	(0.170)		(0.102)	(0.098)	(0.026)	(0.025)
$Std.\ school\ segregation_m$	-0.077	$SSCov_j \times preschool69_i \times Std.\ school\ segregation_j$	0.095		0.021	
	(0.046)		(0.080)		(0.023)	
N	[28]		[7,805]	[7,805]	[240,842]	[240,842]
Southern states requiring school segregation prior to 1954						
$SSCov_m$	0.592***	$SSCov_j \times preschool69_i$	0.245**	0.312***	-0.074**	-0.067***
	(0.197)		(0.124)	(0.095)	(0.030)	(0.025)
$Pop.\ share\ in\ state\ requiring\ sch.\ seg._m$	-0.029	$SSCov_j \times preschool69_i \times State\ required\ sch.\ seg._j$	0.149		0.027	
	(0.062)		(0.191)		(0.052)	
N	[28]		[8,055]	[8,055]	[261,189]	[261,189]
Controls: Gender			Yes	Yes	Yes	Yes
Controls: Cohort x county characteristic			Yes	Yes	Yes	Yes
FE: County x cong. district x year			Yes	Yes	No	No
FE: State x cohort x year			Yes	Yes	No	No
FE: County			.	.	Yes	Yes
FE: State x cohort			.	.	Yes	Yes

Note: Each column of coefficients in each panel are estimated together. Column 1 reports the π_1 and π_2 coefficients described in appendix A3 from a regression of 28 media markets' preschool-age viewership rates on the media market's coverage rate $SScov_m$, controlling for a different metro area characteristic in each panel. Observations are weighted by the media markets' preschool aged population. Column 2 uses CCES data to estimate α_1 and α_2 as specified in appendix A3 on white respondents facing a minority-white ballots with the addition of $(preschool69_i * SSCov_j * CountyChar_j)$, reported in the table. These specifications control for $(preschool69_i * CountyChar_j)$ as well as respondents' gender, *(county × congressional district × year)* and *(state × cohort × year)* fixed effects. Column 3 reports $\hat{\beta}_1$ for the same sample used in column 2. Estimates for columns 2 and 3 employ survey weights. Column 4 estimates α_1^{KL} and α_2^{KL} as specified in appendix A3 on white race IAT test takers with the addition of $(preschool69_i * SSCov_j * CountyChar_j)$, reported in the table. This specification controls for $(preschool69_i * CountyChar_j)$ as well as respondents' gender, *(county)* and *(state × cohort)* fixed effects. Column 5, reports $\hat{\beta}_1^{KL}$ for the same sample used in column 4. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Standard errors are reported in parentheses, clustered at the county level, with the following significance indicators: * p<0.1, ** p<0.05 and *** p<0.01.