Casting roles, casting votes:

Lessons from Sesame Street on media representation, racial biases, and voting

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Abstract

There is little evidence on whether media exposure, particularly in childhood, can reduce prejudice and impact decisions in the long-run. *Sesame Street*'s representation of minority characters, egalitarian minority-white interactions and portrayal of working women was distinctive in the mass media landscape of 1969, when it started airing. By exploiting both age variation and technological variation in broadcast reception, this paper contributes to the media and contact theory literatures by showing that positive representations of minorities and working women via mass media can reduce long-run prejudice and impact voting, an important societal outcome. First, we find that for preschool-age children, a 20 percentage point (1 standard deviation) increase in *Sesame Street* coverage induced a 4.4% increase in voter turnout. Second, we show that exposure to the show's coverage reduced adulthood measures of racial biases, and increased reported voting for minority and women candidates to the U.S. House of Representatives by 13% and 9.7% respectively. Voting for Democratic candidates increased because of the increase in voting for diverse candidates. On ballots featuring white men, turnout gains are split between parties.

1 Introduction

Can child mass media shape prejudices and racial biases in adulthood? Can it impact who we elect as representatives as adults? Women and minority groups in the U.S. have been underrepresented in media roles or have been represented through portrayals of negative and damaging stereotypes. Concern has been drawn in the popular press and scientific community to the persistence and consequences of these patterns. As a result, much research on mass media has focused on its detrimental effects. Yet media technology does not intrinsically dictate harmful content. Indeed, we know little about mass media's potential to *reduce* prejudice in the long-run, particularly when exposure occurs in childhood. Media instruments, such as television, present great opportunity, with the potential to impact large populations, increasing their exposure to, knowledge of, and respect for social groups. This is particularly true for young children. For many young kids, a blockbuster show like *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. In this paper, we study the effects of exposure to *Sesame Street* coverage, a high quality children's educational television show that portrayed an inclusive, egalitarian and diverse America. We examine impacts on voting behavior as elections are a context where millions of Americans choose between political candidates, expressing preferences that may be influenced by candidate demographics. Our analysis finds that decades later, childhood exposure to *Sesame Street* coverage increased reported voting for minority and women candidates to the U.S. House of Representatives. This effect is consistent with the reductions in racial biases that we document and evidence of increased awareness of gender biases. We also find that exposure increased voter turnout in adulthood, consistent with the increases in political interest and knowledge we observe. 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. We find that child mass media programming can positively impact long-run patterns of prejudice and bias, demonstrating that representation in mass media is important, not just as role models for underrepresented groups, but also for other audience members whose biases and prejudices are shaped by such representation. Finally, we show that these changes carry important implications, namely for the vote shares received by

underrepresented candidates in elections.

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 racially diverse urban community, modeled off of New York's African-American neighborhoods. Its human cast was exceptionally diverse from the outset, and all human characters, 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, a psychiatrist and 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, allow-

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

[2]*Mr. Rodger's 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.

[3]Human characters in season 1 were Susan, an African-American nurse; her spouse Gordon, an African-American veteran and history teacher; Bob, a European-American music teacher; and Mr. Hooper, a European-American shopkeeper. David, an African-American law student; and Linda, a deaf European-American librarian joined in season 2. Season 3 added Luis, the Mexican-American owner of the Fix-It shop; and Maria, a Puerto Rican librarian.

[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)).

ing them to form strong bonds with fictional characters whose race did not always match their own.[5]

Recognition that biases and prejudices carry important social and economic costs (Lang and Spitzer (2020)) motivates an extensive literature in child development (Rutland and Killen (2015)), psychology (Paluck et al. (2021)), and economics (Bertrand and Duflo (2017)) seeking to understand, and reduce prejudices. Contact theory, the idea that prejudices can be reduced by interacting and cooperating with equal status and common goals (Allport et al. (1954)), has guided much of this work. 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), Finseraas et al. (2019), Rao (2019), Mousa (2020), Lowe (2021)). Several recent papers have examined the long-run effects of interpersonal childhood contact finding impacts on political preferences (Billings et al. (2021), Brown et al. (2021)) and neighborhood choice (Merlino et al. 2022). Beyond interpersonal contact, reading interventions highlighting inter-group friendships, such as those frequently depicted on *Sesame Street*, have found positive effects (Cameron et al. (2006), Slone et al. (2000), Liebkind and McAlister (1999)), supporting the theory that prejudices can be reduced by learning about positive inter-group relationships (Wright et al. (1997)). Other school based curriculum interventions, have had mixed results.[6]

Whether media based contact with celebrity role-models or out-group characters can impact prejudices (Schiappa et al. (2005)) has been less studied, especially in the long-run (Paluck et al. (2021)). A few papers show that famous minority role models, such as President Obama or the soccer star Mohamed Salah, reduced indicators of prejudice in adults in the short-run (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 (Kearney and Levine (2020)) evidence on how minority

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[5]Upon the death of Emilio Delgado, a long time cast member, memorial Twitter comments often expressed that he was their first exposure to Hispanic culture. Loretta Long who played Susan, recalled of a 1970 cast trip to Jackson 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?"*

[6]In economics, Dhar et al. (2022) find that an intensive curriculum intervention in India had meaningful effects on gender attitudes. In psychology, evaluations of school based multicultural curricula show mixed and null results, though variation in duration, content and targeted age complicates comparisons across studies (Paluck et al. (2021), Bigler (1999), Hughes et al. (2007)).

role models impact majority biases is limited (Bertrand and Duflo (2017)).[7] A growing literature is exploring the media's role in worsening racial and ethnic tensions (Wang (2021), Ang (2023), Müller and Schwarz. (2023), Adena et al. (2015), Yanagizawa-Drott (2014)), but only a 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) show the important effects TV had on gender attitudes in India.

With respect to *Sesame Street*, some evidence suggests that shows similar to *Sesame Street* have reduced 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)).[8] Indeed, exposure to *Sesame Street* occurred in early childhood, when biases are thought to be more malleable having not been compounded over the lifespan (Beelmann and Heinemann (2014), Rutland and Killen (2015)). Though responsiveness to gender and race are detectable in infancy, they continue to evolve in the preschool years (Hughes et al. (2023)).[9] Furthermore, cross-sectional patterns and experimental evidence suggests this evolution is shaped by children's environment (Heron-Delaney et al. (2011), Raabe and Beelmann (2011)), which could include the media they are exposed to.[10]

The sense that children may be especially sensitive to media content has made child media, particularly representations of race and gender topics in child media, a contentious topics for policy makers and parents. Yet despite frequent discussions in the popular press on how minority

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[7]Beaman et al. (2009) see less stereotyping of women by men in Indian villages that randomly had to elect women.

[8]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*.

[9]While newborns exhibit no differences, by 3 months infants look more at own-race faces than other-race faces and by 9 months can better distinguish between own-race faces (Kelly et al. (2005), Kelly et al. (2007)). Most children can correctly identify race by ages 2 to 3 (Raabe and Beelmann (2011)). At age 3, children mimic choices made by individuals of their own gender (but not race) (Shutts et al. (2010)). 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 (Raabe and Beelmann (2011)). However, it is not until elementary ages that children begin to understand that race is an immutable characteristic. See Hughes et al. (2023) for an overview of the developmental literature on race.

[10]Showing infants images of other-race faces can attenuate the emergence of own-race face preferences (Heron-Delaney et al. (2011)). In a large meta-analysis, Raabe and Beelmann (2011) find that the development of explicit prejudices in majority-group children correlates with environments where contact with the minority group is low.

and gender representations in children's media impacts kids, evidence on such impacts is limited. Existing economic research on children's media has generally focused on education and human capital outcomes, finding a range of effects likely due to differences in content, as well as differences in the activities from which time is substituted away from.[11] In their examination of *Sesame Street*'s impacts on academic and labor market outcomes, Kearney and Levine (2019) found 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.[12] Evidence on whether prejudices can be influenced by the representation of minorities and gender roles in children's media is still limited. Adukia et al. (2023) documented how minorities and females are underrepresented in prize winning children's books and showed that representation in the children's books consumed in local communities correlates with local politics, substantially increasing 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 build upon this by identifying the implications of such patterns as we can estimate the causal effects of exposure to representation on prejudice 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 new media source increases exposure to political information (DellaVigna and Kaplan (2007), Oberholzer-Gee and Waldfogel (2009), Sørensen (2019)) or reduces political information due to crowding out (Gentzkow (2006), Falck et al. (2014), Ellingsen and Hernæs (2018)). Work has focused on adult news media though not exclusively. Durante et al. (2019) show that exposure to light entertainment TV in Italy in the 1980s increased populist voting, especially for those exposed in childhood. They hypothesize exposure reduced political knowledge. In contrast, we find an increase in political knowledge and voter turnout, likely due to the educational content of *Sesame Street*.

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[11]These include positive human capital impacts (Gentzkow and Shapiro (2008), Riley (2019)), negative impacts (Hernæs et al. (2019), Durante et al. (2019)) and ambiguous correlations in the cross section (Fiorini and Keane (2014)).

[12]Early academic studies in psychology and education as well as impact evaluations commissioned by governments and its producers generally conclude that *Sesame Street* was effective at teaching its targeted curriculum (Fisch et al. (1999), Fisch and Truglio (2014) and Murphy (1991)), and had positive effects on pro-social behaviors (Paulson (1974), Leifer (1975), Bankart and Anderson (1979), Zielinska and Chambers (1995)).

We identify the causal impact of *Sesame Street* coverage on adult biases and voting patterns using two dimensions in exposure variation: age when the show started airing, and technologically induced variation in geographic coverage rates. This allows us to compare the differences we observe between younger and slightly older cohorts in high coverage counties, to the cohort differences observed in low coverage counties, controlling for county and state level cohort fixed effects. This identification strategy was first used by Kearney and Levine (2019) in their work investigating *Sesame Street*'s impact on educational outcomes. Using data from a large election survey and an online survey of racial biases we apply the Kearney and Levine (2019) strategy to explore these novel and socially important questions, addressing an area where rigorous empirical evidence is lacking. We find that exposure to *Sesame Street* coverage impacted elections in two distinct ways. First, *Sesame Street* coverage improved political engagement and knowledge, increasing voter turnout in all the elections we examine. Second, exposure to *Sesame Street* coverage shaped voter preferences in adulthood. Exposed survey respondents were more likely to report voting for minority and women candidates to the U.S. House of Representatives. As a result, exposed respondents were more likely to report voting for Democratic candidates, because Democratic candidates are more likely to be women and minorities. In elections featuring two white men, turnout gains were split between parties and we find little evidence that exposure to *Sesame Street* coverage changed policy views or political identities in ways that favored one party over another. Instead, we find that *Sesame Street* coverage reduced measures of white racial bias against Blacks, and increased interest in gender biases, suggesting that a long-run reduction in biases and prejudice explains these important differences in candidate choice. We verify that our estimates are not driven by survey attrition, differential migration patterns or impacts on pro-social misrepresentation. Our estimates are also robust to alternative specifications and the addition of educational attainment and income controls, which are unaffected by exposure to the show consistent with findings in Kearney and Levine (2019).

The remainder of the paper is organized as follows. Section 2 presents the data. Section 3 details our empirical strategy. Section 4 documents our first finding, that *Sesame Street* coverage

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increased electoral participation and engagement. Section 5 examines our second result, showing how *Sesame Street* coverage shaped respondents' preferences over candidates. Section 6 examines which candidate attributes drive the observed change in voter behaviors. Section 7 shows coverage impacts on racial biases. Finally, section 8 concludes.

2 Data

Reported voting behavior and ballot composition: This paper uses post-election survey responses on political behavior and public opinion collected by the Cooperative Congressional Election Study (CCES) in election years from 2006 to 2020.[13] In addition to self-reported voting behavior, the CCES also provides validated voter registration and voter turnout by matching respondents to the Catalist database of registered voters.[14] The main sample consists of 57,221 survey responses from non-immigrant citizens born between 1959 and 1968, who faced 2,957 distinct *major party ballots* for their U.S. House Representative.[15] *Major party ballots* are defined as a ballot where the two front-runners are a Democrat and a Republican, both of whom receive over 5% of their district's vote.[16] The 3,025 distinct major party ballots thus defined constitute 87% of ballots for the U.S. House of Representatives between 2006 and 2020. Race and gender demographic information was collected for the two front-running candidates and are observed for both candidates on over 99% of these ballots.[17] Of the 5,914 candidacies in our data, 522 (8.8%) are election runs by Black candidates,

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[13]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.

[14]Registration and voting is 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.

[15]Responses from Washington DC and responses missing information on either 1969 *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 uses responses from individuals who complete the post-election survey. There is no evidence that exposure to *Sesame Street* coverage is associated with post-election survey completion (see appendix table A1).

[16]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).

[17]Basic race (Black, Hispanic, other, white) and gender (man, woman) demographics were compiled using three existing sources. Information for candidacies between 2006 and 2014 is from Fraga and Hassell (2021); data for the 2018 and 2020 elections was collected by 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.

320 (5.4%) are by Hispanic candidates, 4,891 (82.7%) are by white candidates, and 181 (3.1%) are by candidates of other races (East Asian, South Asian, Native American, Middle Eastern, Pacific Islander...). 1,288 candidacies (21.8%) are election runs by women. 689 of the major party ballots feature a white candidate running against a minority candidate and 1,034 feature a woman running against a man. Table A2 of the appendix provides, for the major party ballots in our sample, a full picture of ballot composition by major party candidate demographics and the number of survey responses associated with each type of ballot. Table A2 also reports summary statistics for election results by ballot composition. On average, 16% of these U.S. House elections are won with less than a 10 point margin of victory.

Race and gender biases: Data on race and gender biases is available from Harvard's *Project Implicit*, a research platform that makes online implicit association tests (IATs) available to the public. Anyone can log on to their website, take an IAT, and complete the accompanying survey which also includes questions on explicit biases.[18] Implicit association tests (IATs) are psychological tests designed to measure implicit associations respondents have about individual characteristics. For this project, we focus on the race IAT and the gender-career IAT data.[19] IATs have been used in psychology, and in economics, as a way of measuring implicit attitudes (Glover et al. (2017), Carlana (2019), Corno et al. (2022), Lowes et al. (2015)). Our main datasets consists of the 350,080 race IAT scores and the 84,479 gender-career IAT scores of U.S. resident citizens born between 1959 and 1968, along with their survey responses.[20]

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[18]Because test takers self select into the sample, *Project Implicit* data is not representative and comparisons to a representative sample are not available (Ratliff and Smith (2021)). Potential impacts on selection into taking an IAT test are discussed in section 3.2.

[19]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 increased scrutiny in the psychology literature (Ratliff and Smith (2021)). There is generally agreement that these measures are relevant and predictive, particularly for socially sensitive topics (Greenwald et al. (2009), Bertrand and Duflo (2017)), aggregate regional measures (Hehman et al. (2019)), and voting outcomes (Gawronski et al. (2015), Jost (2019)). Disagreements have centered on the validity of the *implicit* bias construct, and if it differs from *explicit* biases, and whether the low test-retest reliability is due to high measurement error, or the construct itself being time variant (Gawronski (2019), Schimmack (2021), Connor and Evers (2020)).

[20]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 the *survey year − age* for surveys

Sesame Street **coverage:** The predicted share of households in a county that could watch *Sesame Street* in their homes in 1969 was estimated by Kearney and Levine (2019). These predicted coverage rates were calculated using data from the 1968–1969 edition of the trade publication, *TV Factbook*. This lists all commercial and non-commercial 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.[21] Using this information, the authors estimate the empirical relationship between a station's technical specifications and a county's coverage rate using the commercial station sample. This relationship is then applied to the non-commercial stations that broadcast *Sesame Street* to generate a simulated estimate of their coverage rates. Finally, counties are assigned the simulated coverage rate associated with the best simulated signal received from surrounding towers. Figure 1 shows the Kearney and Levine (2019) estimates of simulated coverage levels in each county. Note that because this simulated estimate of the coverage rate is estimated using the average relationship between broadcast technology factors and signal receipt, it is not a function of the quality of receiving television sets in the county which is likely correlated with household wealth. Though national ratings data is not available for the early years of *Sesame Street*, these estimated coverage rates can be validated using the limited Nielsen ratings that are available for 28 metropolitan areas (Haydon (1973)). They are strongly predictive of *Sesame Street* ratings with viewership in the past week increasing by 0.59 percent in the 2 to 5 year old demographic for each percentage point increase in predicted coverage. Consistent with the 69.4% national coverage rate that was estimated for *Sesame Street* in 1970, these measures generate a national predicted coverage rate of 65% with a standard deviation of 20 percentage points.[22]

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taken between July and December, or the *survey year* − (*age* + 1) for surveys taken between January and June.

[21]Particularly important to coverage rates 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. However 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. Television sets were found in 95 percent of households, but a UHF signal could only be received in 54 percent of these (Kearney and Levine (2019)).

[22]More details on the construction of predicted coverage rates are available in Kearney and Levine (2019) and its appendix.

3   Empirical strategy

To identify the effects of early childhood exposure to *Sesame Street* coverage on voting patterns we employ the same identification strategy as Kearney and Levine (2019). We limit our analysis to individuals born between 1959 and 1968 who were between the ages of 1 and 10 when *Sesame Street* began airing in 1969.[23] We then compare the outcomes of cohorts born between 1959 and 1963, who would have been attending elementary school when the show started airing, to slightly younger individuals in the same county, born between 1964 and 1968, who would have been 5 and younger. Preschool attendance was uncommon in this period with only 9% of 3 year olds and 19% of 4 year olds attending preschool in the 1970 census. For 5 year olds, 57% attended kindergarten with 88% of these kindergartners enrolled in half day programs (Kearney and Levine (2019)). Not only were these preschool aged children the targeted demographic of the show's producers, they would generally have had the opportunity to watch *Sesame Street* if the household could receive the broadcast signal. Indeed, our limited viewership data from 28 metropolitan areas confirms that 2 to 5 year olds accounted for 71% of the show's viewership, while 19% were between 6 and 11, and 9.7% were older than 12.

In addition to the variation in cohort exposure, differences in broadcast coverage rates between counties generates a second dimension of exposure variation for identification. Treated kids are thus cohorts that were under the age of 6 when the show first aired *and* who lived in high broadcast coverage counties. Untreated kids are those who were 6 or older in 1969 as well as those who were younger but who lived in counties with a poor broadcast signal. The main empirical models used in estimations using the CCES data are thus specified below,

$$Y^{CCES}_{icjdy} = \beta_0 + \beta_1(preschool69_i * SSCov_j) + \beta_2 X_i + \gamma_{scy} + \delta_{jdy} + \epsilon_i, \tag{1}$$

$$Y^{CCES}_{icjdy} = \lambda_0 + \sum_{c=59, c \neq 63}^{68} \lambda_{1c}(Cohort_{ic} * SSCov_j) + \lambda_2 X_i + \gamma_{scy} + \delta_{jdy} + \epsilon_i. \tag{2}$$

Y^{CCES} is the CCES outcome variable of interest for individual i, in cohort c, residing in county j,

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[23]These respondents would have been between their late thirties and early sixties when they were surveyed in the election years between 2006 and 2020 or when they completed their IATs.

and voting in congressional district d in year y. The indicator variable $preschool69_i$ is set to 1 if the individual would have been below the age of 6 when *Sesame Street* first aired. This is interacted with the variable $SSCov_j$, the predicted level of *Sesame Street* coverage in county j. Figures also present disaggregated cohort level estimates of λ_{1c}, as specified in equation 2 where $Cohort_{ic}$ is an indicator variable set to one if the respondent was born in cohort c.

Our preferred specification includes X_i: controls for an individual's gender and race, in addition to the γ and δ fixed effects. 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 would 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 particular congressional election that affect all respondents in the congressional district in the same way (the presence of third party candidates, idiosyncratic shocks, up-ballot elections, etc.).

For outcomes from the IAT data, controlling for voting patterns in a particular election is not necessary, or possible. These results are estimated using the following specification which is identical to the specification used in Kearney and Levine (2019),

$$Y_{ijc}^{IAT} = \beta_0^{KL} + \beta_1^{KL}(preschool69_i * SSCov_j) + \beta_2^{KL}X_i + \gamma_{sc}^{KL} + \delta_j^{KL} + \epsilon_i, \qquad (3)$$

$$Y_{ijc}^{IAT} = \lambda_0^{KL} + \sum_{c=59,c\neq63}^{68} \lambda_{1c}^{KL}(Cohort_{ic} * SSCov_j) + \lambda_2^{KL}X_i + \gamma_{sc}^{KL} + \delta_j^{KL} + \epsilon_i. \qquad (4)$$

These specifications employ a (*state* × *cohort*) fixed effect, γ_{sc}^{KL}, and a *county* fixed effect, δ_j^{KL}. Estimates of β_1^{KL} using the CCES data will also be reported.

Robustness of key results to the inclusion of additional controls is explored in appendix table A3. Given the educational nature of the show, we estimate specifications that control for a respondent's education, and income. We also estimate specifications that include interactions between cohort indicators and county characteristics as local socio-economic conditions that may correlate with *Sesame Street* coverage might have impacted cohorts differently. Inclusion of additional controls does not alter our main estimates meaningfully.

It is important to note that our treatment measures a county's exposure to *Sesame Street* broadcast coverage, not actual viewership. Our estimates should thus be interpreted as "intent-to-treat" estimates, identifying the impact of making the show more available to children rather than the impact of watching the show on an individual child. As such, we will report the effects of a 20 percentage point (about one standard deviation) increase in the predicted coverage rate of a county. These intent-to-treat estimates are the estimate of interest for policy makers who might be able to alter media access but are unlikely to control actual viewership.[24]

It is also important to highlight that treatment in our context is composed of both increased exposure to *Sesame Street* coverage, but also, for viewers, a time substitution away from other activities. These alternate activities could have been viewership of other TV programs, or entirely different uses of children's time.[25] 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.

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[24]Though of great interest, 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 where viewership data is available is reported with heterogeneity results in appendix A2.

[25]Data on children's time-use for this period is not available limiting our ability to examine patterns in time substitution when the show began airing.

3.1 Migration: Selection and attenuation bias

Ideally we would observe respondents' counties of residence in childhood. As this is unobserved in the CCES and IAT data, our measure of *Sesame Street* coverage exposure is based on respondents' counties when surveyed, between 2006 and 2020. Substantial out-of-county migration between childhood and adulthood will generate attrition bias, biasing our results towards zero. The possibility that exposure affected out-of-county migration is also a concern. We explore these issues below.

We find no evidence that *Sesame Street* coverage impacted the probability of residing in one's childhood city, county, birth state, or in counties with more or less 1969 *Sesame Street* coverage. 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 never-movers who have lived in their city since they were two or younger and estimate $\hat{\beta}_1$ and $\hat{\beta}_1^{KL}$ on this indicator. Results are reported in the first row of table A4. For both specifications, we fail to reject the null of no effect on city mobility. We confirm this with restricted-use data from the Panel Survey of Income Dynamics (PSID). We identify 1,204 original panel members born between 1959 and 1968 that are also observed in 2019.[26] In table A4, we estimate $\hat{\beta}^{KL}$ on indicators for residing in the same state (row 3), county (row 4), as well as the 1969 *Sesame Street* coverage rate of respondents' adult county (row 5). Though the small sample generates imprecise estimates, we find no evidence that exposure to *Sesame Street* coverage impacted the propensity to move across state or county lines. We can rule out that a 20 percentage point (1 sd) increase in the coverage rate impacted the probability of moving out-of-county beyond the [9.8, -13] percentage point range and the 1969 *Sesame Street* coverage rate in adult counties of residence outside of a [3.3, -4.3] percentage point range with a 95% confidence level. Impacts on inter-state mobility can also be ruled out with more precision using the American Community Survey (ACS). ACS respondents report their state of birth allowing the identification of inter-state movers. We

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[26]The original household sample included 4,312 members from these cohorts. In the second row of table A4 we estimate $\hat{\beta}^{KL}$ on an indicator for attrition between the original PSID sample and 2019. We find no evidence that exposure to *Sesame Street* coverage impacted attrition in the PSID.

build $SSCov_s$, a coarse state level measure of *Sesame Street* coverage exposure as the population weighted mean coverage rate across a state's counties. We estimate a similar specification on an indicator for residing in one's birth state, replacing $SSCov_j$ with $SSCov_s$ and using state and cohort fixed effects. Results are reported in the last row of table A4. Here too we fail to reject the null of no effect on mobility away from one's birth state and can rule out that a 20 percentage point (1 sd) increase in the coverage rate impacted the probability of inter-state moving outside of the [0.77, -0.31] percentage point range with a 95% level of confidence.

Coverage rates between destination and sending counties are correlated for county-to-county migrants, counteracting attenuation bias. Only 47.3% of the observed PSID sample reside in their childhood county in 2019, raising the issue of attenuation bias.[27] Nevertheless, the correlation between the 1969 coverage rates of adult and childhood counties in the PSID is high at 0.547 (p-value<0.001) as reported in column 1, row 1 of table A5. The severity of attenuation bias is reduced as even amongst county migrants these coverage rates are correlated, as reported in column 2. 30% of inter-county migration occurs between neighboring counties. For these migrants the correlation between adult and childhood county coverage rates is high at 0.789. Yet even for long distance moves, there is a positive correlation of 0.101 (p-value=0.06) between destination and sending counties' coverage rates. These patterns can be corroborated with general US mobility data. 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 in the second row of table A5 show very similar correlation estimates. Overall, inter-county moving attenuates our results though this effect is counteracted by the high level of correlation between destination and sending county coverage rates.

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[27]9% of the CCES sample are city never-movers though this survey question will not capture respondents who moved between cities within a county, and respondents who moved, but then returned, to their childhood city.

3.2 Selection into the *Project implicit* data

A particular concern with analysis of IAT scores collected by *Project Implicit* is selection into the sample. IAT tests are publicly available online through the *Project Implicit* website and any individual can log on and takes a test. Exposure to *Sesame Street* coverage may have affected the probability individuals select into the sample, biasing estimates.[28] To check for selection, we calculate $ShareSS_j = \frac{N_{j,1964 \leq Cohort_i \leq 1968}}{N_j}$, the share of our sample observations in each county j coming from treated cohorts. We then regress $ShareSS_j$ on the county's predicted coverage rate, $SSCov_j$, to check if a larger share of a county's IAT test takers are from treated cohorts in counties that had higher levels of *Sesame Street* coverage. Results are reported in appendix table A6. Column 1 presents estimates using all the race IAT test takers, column 2 using only white race IAT test takers, and column 3 presents estimates for the gender-career IAT test takers. 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.[29] Given this selection, our analysis in section 7 will focus on the race IAT data. Nevertheless, it should be noted that this impact on selection into taking the gender-career IAT test suggest 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 exposure to *Sesame Street* coverage increased voter turnout and registration, likely due to the increase we observe in political interest and knowledge. Examination of our second set of results on voter preferences for candidate demographics is presented in section 5.

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

[29]The positive and statistically significant estimate of 0.034 implies that a 20 percentage point increase in the *Sesame Street* coverage rate increased the share of test takers from treated cohorts in a county by 0.68 percentage points.

***Sesame Street* coverage increased turnout in general elections.** Treated cohorts exposed to *Sesame Street* coverage in preschool are more likely to vote in general elections. The $\hat{\beta}_1$ estimate of 0.139 (p-value < 0.001) on verified general election turnout, reported in the first row of table 1, panel a, column 2, implies that for a 20 percentage point (1 sd) increase in the coverage rate, the probability of treated cohort members turning out to vote increases by 2.8 percentage points, a 4.4% increase. This effect is highly statistically significant, and robust to alternative specifications (column 3 and appendix table A3). Figure 2a plots the cohort level $\hat{\lambda}_{1y}$ estimates, confirming a discontinuity in validated turnout between treated and untreated cohorts. The estimated $\hat{\beta}_1$ and $\hat{\lambda}_{1y}$'s are similar when using the indicator for self-reported election turnout, plotted in figure 2a and reported in the second row of table 1, panel a.

The similarity between estimates using verified and self-reported turnout is important. 85% of respondents report voting but verified turnout rates are substantially lower, and can only be validated for 63% of CCES respondents. Recent work on the CCES data finds that this gap is due to overreporting turnout to surveyors (Enamorado and Imai (2019)). One may suspect that exposure to *Sesame Street*'s pro-social messaging could (ambiguously) affect the propensity to misrepresent one's turnout by either increasing the desire to supply a socially desirable response or by decreasing the propensity to lie to a surveyor. To check this, we generate an indicator for inconsistent validated and self-reported turnout records. Estimated $\hat{\beta}_1$'s are reported the third row of table 1, panel a. We see no impact of treatment on the probability of having inconsistent verified and self-reported turnout records. There is no evidence that exposure to *Sesame Street* coverage affected the likelihood respondents misrepresent their behavior to surveyors.

To better understand why *Sesame Street* coverage impacted turnout, we examine respondents' reasons for not voting.[30] Results are presented in appendix table A7. Exposure to *Sesame Street* coverage reduced the likelihood that respondents report not voting because they dislike the candidates and because they are not registered. These effects are robust across specifications and explain about half of the effect on reported non-turnout. The other reported estimates are less clear but taken jointly suggest an increased interest in participating in elections, increased knowledge about the

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[30]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.

voting process, and an increased willingness to incur non-pecuniary costs to vote.

Sesame Street **coverage increased voter registration.** Treated cohorts who were exposed to *Sesame Street* coverage in preschool are more likely to be registered to vote. The $\hat{\beta}_1$ estimate of 0.091 (p-value $= 0.017$) on having a verified active registration record, reported in the first row of table 1, panel b, column 2, implies that for a 20 percentage point (1 sd) increase in the coverage rate, the probability of having a verified active registration record is 1.8 percentage points higher, a 2.4% increase. This effect is statistically significant, and robust to alternative specifications (column 3). Figure 2b plots the cohort level $\hat{\lambda}_{1y}$ estimates and confirms a discontinuity in validated registrations between treated and untreated cohorts. While still present, the estimate of $\hat{\beta}_1$ on self-reported voter registration is smaller in magnitude by about half with little evidence of a discontinuity between treated and untreated cohorts.

The difference between these two indicators is interesting. 94% of respondents self-report being registered to vote while only 74% have a verified active registration. A substantial share of respondents misreport their registration status, and the estimated coefficient of -0.066 (p-value= 0.07) on an indicator of inconsistency, in the third row of table 1, panel b, implies that treated cohorts 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 treated cohorts are more politically informed, suggesting that *Sesame Street* coverage could have reduced misinformation about respondents' voter registration status.

Heterogeneity in this increase in electoral participation is examined in the appendix. Table A8 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 A9 shows that turnout increases for different ballot compositions hold regardless of candidate demographics and are also observed during midterm elections which are not affected by the demographic composition of the presidential ballot, with

$\hat{\beta}_1 \in [0.121, 0.178]$ across the sub-samples. Consequently, this effect on electoral participation should be thought of as distinct from the effect on preferences over candidate demographics that we document in section 5.

How did *Sesame Street* coverage increase electoral participation? Below we explore several explanations.

We find no evidence of a change in educational attainment and income. Given the findings in Kearney and Levine (2019) we start by considering income and education channels. The CCES survey collects coarse information on respondents' education levels and their household income. Treatment effects on years of education and family income are reported in table 2. Like Kearney and Levine (2019) we cannot reject the null of no effect on educational attainment (in years) and log family income. Though the coefficients are positive at (0.167 (p-value= 0.49) and 0.016 (p-value= 0.78) respectively, they are not statistically significant. The same estimation applied to the IAT data also fails to reject the null of no effect at 0.034 (p-value= 0.56) on educational attainment. Furthermore, estimates reported in appendix table A3 that include controls for educational attainment and family income and do not substantially differ from estimates using our preferred specification. Increased educational attainment and family income are not the main mechanisms behind our results.

Sesame Street **coverage increased political knowledge and interest.** Estimates in table 3, panel a, show that exposure to *Sesame Street* coverage increased political knowledge in adulthood. Treated cohorts in higher coverage counties are more likely to recognize the names of their senators and representatives, with an estimate of 0.179 (p-value = 0.012) out of 3 names. They report a greater interest in politics with an estimate 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.

Sesame Street **coverage increased weak political identities, but not active political engagement**

or strong political identities. With respect to political identification, a more complex picture emerges in table 3, panel b. Treated 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 in which 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.[31] 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 such as voting in primary elections, donating money to political campaigns, putting up political signs, attending political meetings and working for a candidate or campaign. For all of these indicators, we are unable to reject the null of no treatment effect (p-values $\in [0.19, 0.77]$).[32] Consistent with this, table A11 shows that effects on electoral participation are larger for marginally engaged respondents, as proxied by their verified primary election turnout status.

Overall, these 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 previous finding in the literature. Meta-analyses of the literature on voter turnout have highlighted that party identification, political interest, and political knowledge are consistently linked to individual level 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)).

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

[32]We also find no evidence of a change 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 A10.

5 *Sesame Street* coverage shaped who received reported votes

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

5.1 *Sesame Street* coverage increased reported voting for minority candidates

We begin by restricting the sample to the 13,120 respondents facing one of the 689 ballots that featured a minority candidate running against a white candidate. 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. The share of respondents in each of these categories is reported in the first column of table 4, panel a. Estimates of β_1 using each indicator as an outcome are reported in column 2 of table 4, panel a.

Treated cohorts exposed to *Sesame Street* coverage are substantially more likely to report voting for a minority candidate. The $\hat{\beta}_1$ estimate of 0.271 (p-value < 0.001) reported in the first row of table 4, panel a, implies that for a 20 percentage point increase in the coverage rate, the probability a voter reports a vote for a minority candidate is 5.4 percentage points higher, a 13% increase. This effect is highly statistically significant, and robust to alternative specifications (column 3 and appendix table A3). About half of this effect can be attributed to reductions in reported non-voting, as indicated by the negative and statistically significant coefficient reported in the fourth row. The other half is driven by reduced reported voting for white candidates, as indicated by the -0.144 (p-value= 0.09) estimate reported in the second row of table 4, panel a. That reported voting for white candidates is decreasing amongst treated cohorts, despite increases in turnout for these cohorts, signals that we should think of this effect on preferences over candidates as distinct from the impacts on electoral participation discussed in section 4.

Figure 3a plots the cohort level $\hat{\lambda}_{1y}$ estimates for the minority candidate indicator, the white candidate indicator and the non-voting indicator, confirming a discontinuity in voter behavior

between the elementary aged and preschool aged cohorts. Similar impacts are found when the sample is limited to respondents who report voting for a major party, and validated voters, as reported in appendix table A12. Potential impacts on election outcomes are discussed in appendix A1.

In columns 4 and 5, we split the sample to estimate the effect separately for white and minority voters. We find no evidence of differential effects in reported voting for minority candidates across these groups (p-value= 0.79), 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 *Sesame Street* coverage increased reported voting for women candidates

For respondents voting on ballots that feature a woman running against a man, we construct four mutually exclusive indicators representing reported votes. Estimates of β_1 on each of these indicators are reported in column 2 of table 4, panel b.

Treated cohorts exposed to *Sesame Street* coverage are more likely to report voting for a woman candidate. The $\hat{\beta}_1$ estimate of 0.192 (p-value < 0.001) reported in the first row of table 4, panel b, implies that for a 20 percentage point increase in the coverage rate, the probability of reporting a vote for a woman candidate is 3.8 percentage points higher, a 9.7% increase. This effect is highly statistically significant, and robust to alternate specifications (column 3 and appendix table A3). Most of the gains by women candidates come from a reduction in the share of reported non-voting, as indicated by the negative and significant coefficient of comparable magnitude reported in row 4 of panel b. Exposure to *Sesame Street* coverage had little effect on reported voting for men candidates as demonstrated in row 2 of panel b. Reported voting for third party candidates is lower for treated respondents though only a very small share of respondents report third party votes. Figure 3b plots the cohort level $\hat{\lambda}_{1y}$ estimates for the woman candidate indicator, man candidate indicator, and non-voting indicator, confirming a discontinuity in voter behavior between treated and untreated cohorts. When the sample is limited to validated voters reporting a major party vote, in appendix table A12, we cannot reject the null of no effect, though the direction of the coefficient suggests a

small positive effect on women candidates' vote share. Possible implications on election outcomes are discussed in appendix A1.

In columns 4 and 5, we split the sample to estimate the effect separately for female and male respondents. We find no statistically significant differences in reported voting for women across these groups (p-value= 0.79), though suggestively the turnout effect is more important for male voters while female voters switch their reported votes from men to women candidates.

5.3 *Sesame Street* **coverage increased reported voting for Democratic candidates**

As in the preceding sections, we build four mutually exclusive indicators, representing respondents' reported votes: whether they report voting for the Democratic candidate, the Republican candidate, a third party candidate or not voting. Estimates of β_1 on each of these indicators, are reported in column 2 of table 4, panel c.

Cohorts exposed to *Sesame Street* coverage are more likely to report voting for a Democratic candidate. The $\hat{\beta}_1$ estimate of 0.100 (p-value $= 0.007$) reported in the first row of panel c implies that for a 20 percentage point increase in the coverage rate, the probability a voter reports a Democratic vote is 2.0 percentage points higher, a 5.0% increase. The $\hat{\beta}_1^{KL}$ estimate (column 3) is of a slightly smaller magnitude, but still statistically significant at conventional levels (p-value=0.07). Most of these Democratic gains come from a reduction in reported non-voting, as indicated by the negative and significant coefficients reported in the fourth row. Estimates on reported voting for Republican candidates are positive but smaller and not generally statistically significant. Reported voting for third party candidates is lower for treated respondents though only a very small share report third party votes. When the sample is limited to validated voters voting for major party candidates, in appendix table A12, we cannot reject the null of no effect, though the coefficient suggests a small positive effect favoring Democratic candidates.

It is worth highlighting 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 probability respondents misrepresent their voting

behavior, a concern given that we only observe how respondents report voting, not their actual vote. The data suggests this is not driving our results. First, in section 4 we compared reported and verified turnout data and found no treatment effect on respondents' propensity to misrepresent their turnout behavior to surveyors, a common form of misrepresentation in polling data. Second, columns 3 and 4 of table A12 compare estimates on the reported voting of validated voters and non-validated voters who report voting for a major party candidate. 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 effects on correlated candidate characteristics

The results presented in section 5 show that exposure to *Sesame Street* coverage increased the likelihood respondents report voting for minority candidates, women candidates and Democratic candidates. What drives these patterns is unclear. Many of these 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% as opposed to 19.4% of white candidacies. In the following section we consider which of these observable candidate characteristics voters are responding to. We show that these changes in reported votes are driven by candidate demographics rather than their party.

It is important to note that as we cannot control for all candidate characteristics, we cannot rule out that being a minority or woman candidate correlates with unobservable candidate characteristics that may be generating this voter response. In this section we explore potential mechanisms, ruling out potential confounds. In section 7 we provide evidence that exposure to *Sesame Street* coverage changed biases, the most likely explanation for the observed impacts on reported voting.

6.1 Diverse candidates drive the increase in reported voting for Democrats

Sesame Street coverage increased reported voting for Democratic candidates. Is this effect due to changed political views or a response to candidate characteristics that correlate with candidate party?

Table 5, re-estimates the effects on reported vote by party for sub-samples of ballots with different demographic compositions. Column 1 limits the sample to the 44% of respondents voting on 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 gains in turnout are split between Democrat and Republican candidates, favoring Republicans. Indeed, conditional on voting for a major party, the coefficient on reporting a Democratic vote is negative and not statistically significant at -0.011 (p-value= 0.88), as reported in column 2. On ballots featuring two white men, we cannot reject that voting for both parties was comparably affected by the turnout effect *Sesame Street* coverage had on the electorate.

In contrast, we see a strong treatment effect favoring Democrats on the sub-sample of ballots where the Democratic candidate is a woman, in column 3, at 0.183 (p-value = 0.002); and where the Democratic candidate is a minority in column 5, at 0.235 (p-value = 0.009). Overall, the increase in reported voting for Democrats is driven by women and minority Democratic candidacies.

6.2 Reported voting for all minority candidates increased

Sesame Street coverage increased reported voting for minority candidates. In table 6, panel a, we consider whether candidate characteristics that correlate with a candidate's minority identity, namely political party and gender identity, explain this pattern.[33] Columns 1 and 2 of table 6, panel a, estimate β_1 separately for ballots where the minority candidate is running as the Democrat (column 1) and the Republican (column 2). In both sub-samples, $\hat{\beta}_1$ is positive and statistically significant implying increased reported voting for minority candidates regardless of their party,

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[33]Minority candidates are more likely to run as Democrats. Of the 689 ballots featuring a minority candidate running against a white candidate, the minority candidate is running as a Democrat on 76% of the ballots. Minority candidates are also more likely to be women. 33% of minority candidacies are woman candidacies as opposed to 19.4% of white candidacies.

with suggestively larger effects for Republican minority candidates (p-value= 0.16). In columns 3 and 4 of table 6, panel a, we estimate β_1 separately on 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 statistically significant. Thus the increase in reported voting for the minority candidate holds for both minority men and women candidates, and is possibly larger for women minority candidates (p-value= 0.14).

6.3 Reported voting for Democratic women increased, with larger effects for minority women

Sesame Street coverage increased reported voting for women candidates. In table 6, panel b, we consider if this a response to candidate characteristics that correlate with a candidate's gender, namely political party and minority identity.[34] Columns 1 and 2 of table 6, panel b, estimate β_1 on ballots where the woman candidate is running as a Democrat (column 1) and Republican (column 2) separately. Treated respondents are no more likely to report voting for Republican women than for the Democratic men they run against, though estimates are imprecise given the small number of such ballots. In contrast, the $\hat{\beta}_1$ for Democratic women is large and statistically significant suggesting the main effect is driven by increased reported voting for women running as Democrats, though we cannot reject equality between the two coefficients. Columns 3 and 4 of table 6, panel b, estimate β_1 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 implying 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 by exposure to *Sesame Street* coverage.

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[34]Women candidates are more likely to run as Democrats. Of the 1,034 ballots featuring a woman candidate running against a man, the woman is running as the Democrat on 74% of the ballots. Women candidates are also more likely to be minorities. 26.2% of candidacies by women are also minority candidacies as opposed to 20.5% of candidacies by men.

6.4   No clear impacts on preferences for policies or incumbents

Beyond party affiliation, other characteristics may correlate with candidate demographics. Below we examine impacts on support for specific policies, and preferences for incumbent representatives.

Sesame Street **coverage did not consistently shape policy preferences in a pattern consistent with party platforms.** Exposure to *Sesame Street* coverage may have shaped treated respondents' policy preferences towards policies that are more likely to be espoused by minority and female candidates. To investigate this mechanism, we examine CCES respondents' views on specific policies. Table 7 presents aggregated indices of responses to broad policy topics.[35] Overall, we cannot rule out that exposure to *Sesame Street* coverage has no impact on respondents' support for environmental policies or abortion rights. Treatment effects are observed signaling increased support for same-sex marriage, an effect consistent with increased tolerance for diversity more generally. In contrast, we observe reduced support for less restrictive immigration policies. The CCES survey and the IAT survey also include opinion questions on race relations in the US, and race related policies. Panel b presents results for the CESS data and panel c for the IAT data.[36] For questions on race related policies and race relations in the US, estimated effects are generally small, not statistically significant, and when comparable, inconsistent across data sources and question topics.[37] Overall, impacts on policy views are mixed or null and do not clearly align with any particular political platform.

Sesame Street **coverage increased political and party identification without clearly favoring one end of the political spectrum.** Table 8, panel a, examines treatment effects on respondents' reported political ideology in both the CCES and IAT data. Treated respondents are more likely to report identifying with a liberal or a conservative ideology as opposed to being moderate or neutral, as

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[35]Administered questions vary across surveys. Selected questions used to construct indices were administered in multiple survey years. Disaggregated estimates are reported in table A13.

[36]Estimates for all questions estimated separately are presented in appendix table A14.

[37]For instance, while we detect 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.

reported in column 1 for the CCES data and column 3 for the IAT data. However, the shift towards having a political ideology is split between liberal and conservative. When we restrict the sample to individuals expressing a political ideology, in columns 2 and 4, 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, a question that is only asked in the CCES data. Results reported in panel b show a reduction in identifying as an independent 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).

Sesame Street **coverage did not change voting for incumbents.** As exposure to *Sesame Street* coverage increased political knowledge it may have reduced the propensity to default towards voting for incumbent candidates. We explore this in appendix table A15. There is no evidence that exposure changed relative reported voting for incumbent candidates. Turnout effects are split. Respondents report increased voting 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 candidate (p-value= 0.87).

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

7 *Sesame Street* coverage reduced measures of racial biases

In this section we present evidence that the observed changes in voter preferences are the result of a change in racial biases. *Sesame Street*'s portrayal of positive minority role models in an integrated cohesive community reduced long-run racial biases, increasing the probability of reporting a vote for diverse candidates. We focus our analysis on the race IAT given the evidence of selection into the gender-career IAT discussed in section 3.2. It should be noted however that the selection pattern

is itself suggestive that exposure to *Sesame Street* coverage also increased interest in issues surrounding gender-career biases, though we cannot make conclusive statements on how it impacted gender-career IAT scores.[38]

Sesame Street coverage reduced measures of white racial bias towards Blacks in the long-run.
Column 2 of table 9 presents $\hat{\beta}_1^{KL}$ estimates on standardized race IAT scores. Estimates in row 1 reveal a negative and statistically significant effect on this measure of implicit bias for white test takers. The $\hat{\beta}_1^{KL}$ estimate of -0.067 sds (p-value $= 0.007$) reported in row 1 of column 2 implies that for a 20 percentage point (1 sd) increase in the coverage rate, standardized IAT scores decrease by -0.01 standard deviations.[39] Like Corno et al. (2022), and consistent with contact theory, we also find an opposite sign effect for Blacks (row 2) though it is imprecisely estimated given the small number of Black test takers. Figure 4 plots the cohort level $\hat{\lambda}_{1y}^{KL}$ estimates on the IAT scores of white test takers, confirming a discontinuity in IAT scores between the elementary aged and preschool aged cohorts.

In addition to IAT test scores measuring implicit biases, the IAT survey also administers two race thermology questions which we examine in table 10. Column 1 pools results for white and Black test takers, results for white test takers are presented in column 2, and for Black test takers in column 3. Overall, 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 Americans and African Americans on a 10 point scale, 66% report the same value for both groups (appendix figures A1a and A1b).[40] In this context, it is not surprising that we detect no discernible effect of *Sesame Street* coverage on the explicit preferences expressed by white test

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[38]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 (appendix table A16).

[39]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.

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

takers (p-value $\in [0.17, 0.51]$). Estimates of $\hat{\beta}_1^{KL}$ for white test takers are small in magnitude and not statistically significant. However, when we limit the sample to test takers who do not report the same value for both groups, the magnitude of the effects become larger, and a small effect signalling reduced in-group preferences on warmth questions becomes statistically distinguishable from zero (p-value= 0.09).[41] Reporting no preferences across racial groups is less common for African Americans, with 36% reporting that they like European and African Americans equally and assigning the same level of warmth towards these two groups on the 10 point scale (figures A1a and A1b). Effects for this group are less muted and display a similar pattern of reduced in-group preferences for exposed cohorts. Overall, these patterns suggest a muted reduction in explicit measures of in-group preferences that is more pronounced when restricting the data to respondents who are willing to express variation in their 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 are driving the change in voting patterns, we would expect to observe larger effects for lesser known minority candidates. For known candidates, voters can base decisions on observed behaviors, choices, and viewpoints, potentially reducing the influence of racial biases. Consistent with this hypothesis, we find that non-incumbent minority candidates gain most from an electorate's exposure to *Sesame Street*. Table 11 presents heterogeneity results by the incumbency status of the minority candidate. Column 2 shows estimates on incumbent minority candidates while columns 1 and 3 do so for non-incumbent minority candidates that face a non-incumbent (column 1) or an incumbent (column 3). For non-incumbent minority candidates, the impact of *Sesame Street* is large at 0.313 (p-value= 0.23) and 0.363 (p-value = 0.003) respectively for columns 1 and 3.[42] For incumbent minority candidates, while still positive, the estimate is substantially smaller at 0.070 (p-value= 0.62) and not statistically significant with the difference between these two groups approaching statistically significance at the 10% level (p-value= 0.11) when comparing impacts reported in columns 2 and 3.

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[41]The -0.192 point estimate suggests that for a 20 percentage point (1sd) increase in the coverage rate, the difference in expressed warmth for European versus African Americans for these white non-zero test takers shrinks by -0.04 points, a 0.017 sd decrease.

[42]Estimates in column 1 are imprecise due to the small number of white-minority ballots with no incumbents.

Given these findings, it is interesting to consider how the impacts of *Sesame Street* may have 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, making this an interesting question for empirical research. Unfortunately, the lack of national county level viewership data complicates interpretation as the county characteristics we examine may have affected both viewership probability as well as how the show impacted its viewers. Nevertheless, heterogeneity in effects by county characteristics are presented and discussed in appendix A2. Overall, we find no evidence of significant heterogeneity across a number of relevant variables that help characterise the social and racial environment of counties.

8   Conclusion

Can child mass media shape prejudices and racial biases in adulthood? Can it impact who we elect as our representatives as adults? The importance of representation in the media has increasingly been recognized and discussed in the popular press. How gender and race are approached in child media has also been the subject of recent politicized debate. Yet despite this flood of attention, there is little causal evidence on how positive non-stereotyped representation affects social and economic outcomes.

This paper help inform these debates. In this paper, we show that preschool-age exposure to *Sesame Street* coverage, and its portrayal of an inclusive, egalitarian and diverse America, had long-run effects on adult voting patterns and racial biases. We find that decades later, exposed cohorts were more interested and engaged in the political process, registering and turning out to vote at higher rates than slightly older cohorts in their same county. When voting, these cohorts were more likely to report voting for minority and women candidates. Voting for Democratic candidates increased because of the increase in voting for diverse candidates. When we limit the sample to ballots featuring two white men, turnout gains were split between both parties, consistent

with other evidence that the show increased moderate political engagement across the political spectrum. Overall, the evidence suggests that the change in voting behavior stems from a change in voter preferences for candidate demographics, consistent with the reductions in racial bias measures of white IAT test takers we observe for these cohorts.

Our results suggest multiple new directions for future research. Research in economics on the effects of child media is limited. This is particularly true for long-run effects on outcomes other than education. These results show 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 had important effects on majority group members, a relationship that has not been the focus of work in the past. Finally, this paper shows 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
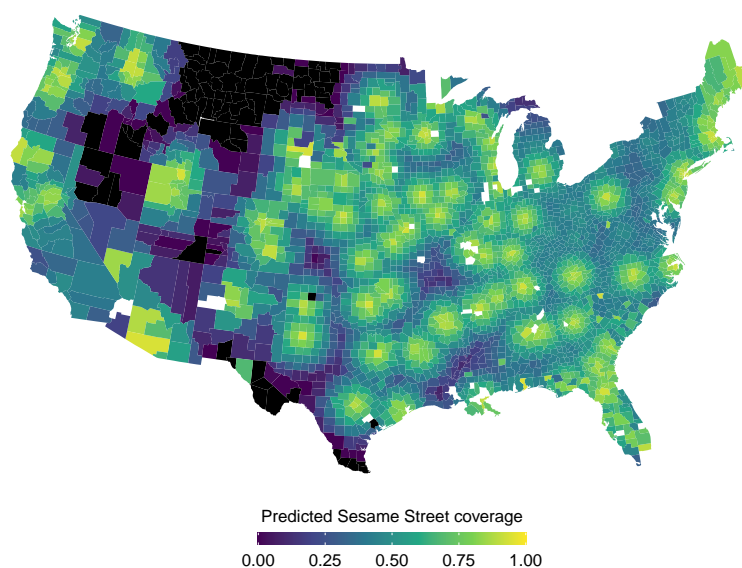
Figure 1. Broadcast signal towers and estimated county coverage rates of

Sesame Street **in 1969**

Notes: Color gradient represents predicted share of households that could watch *Sesame Street* in that county in 1969 as calculated in Kearney and Levine (2019). Estimated coverage is simulated using the average relationship between the broadcast technology of signal towers and signal receipt.
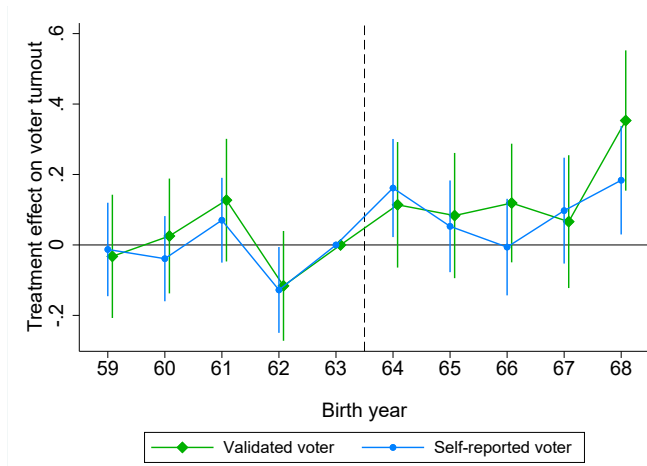
(a) Self-reported and validated turnout
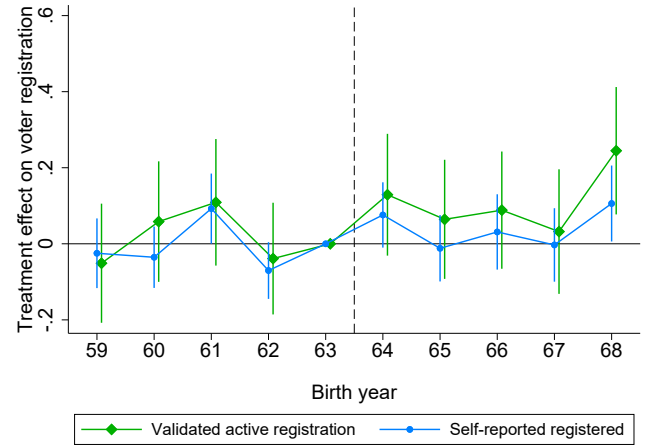
(b) Self-reported and validated registration

Figure 2. *Sesame Street* **coverage increased voter turnout and registration**

Notes: These figures plot the $\hat{\lambda}_{1c}$ estimates from the interaction between birth year indicators and the county's predicted *Sesame Street* coverage in 1969 as specified in equation 2. Controls for race and gender as well as (*state×cohort×year*) and (*county×congressional district×year*) fixed effects are included. The outcome variables are indicators variables. The 1963 cohort is the omitted category. Estimates include survey weights. 95% confidence intervals are depicted using standard errors clustered at the county level.
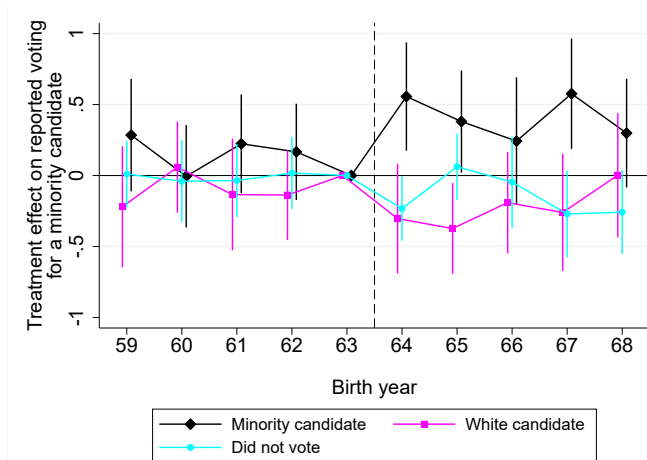
(a) Reported voting on minority-white ballots
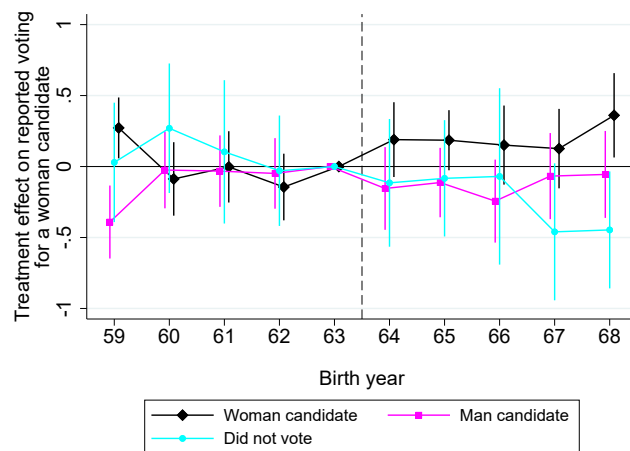
(b) Reported voting on woman-man ballots

Figure 3. *Sesame Street* **coverage increased reported voting for minority and women candidates**

Notes: These figures plot the $\hat{\lambda}_{1c}$ estimates from the interaction between birth year indicators and the county's predicted *Sesame Street* coverage in 1969 as specified in equation 2. Controls for race and gender as well as (*state×cohort×year*) and (*county×congressional district×year*) fixed effects are included. 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. The sample is limited to respondents who face minority-white ballots (figure a) or woman-man ballots (figure b). The 1963 cohort is the omitted category. Estimates include survey weights. 95% confidence intervals are depicted using standard errors clustered at the county level.
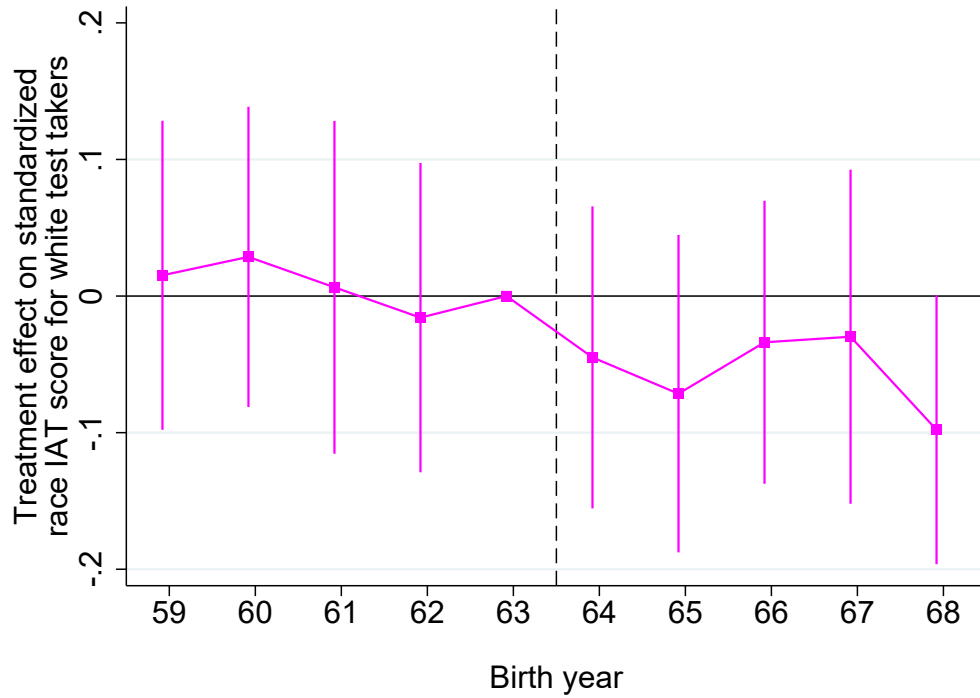
Figure 4. *Sesame Street* **coverage reduced measures of implicit bias**

against Blacks for white test takers

Notes: These figures plot the $\hat{\lambda}_{1c}^{KL}$ estimates from the interaction between birth year indicators and the county's predicted *Sesame Street* coverage in 1969 as specified in equation 4. Controls for race and gender as well as (*state×cohort*) and (*county*) fixed effects are included. The 1963 cohort is the omitted category. Larger positive values indicate stronger Black-bad and white-good associations. 95% confidence intervals are depicted using standard errors clustered at the county level.

10 Tables

Table 1: *Sesame Street* **coverage increased voter turnout and registration**

	(1)	(2)	(3)
Dependent indicator variable	Dependent indicator mean	$\hat{\beta}_1$	$\hat{\beta}_1^{KL}$
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]
Self-reported election turnout	0.851	0.120*** (0.032) [51,723]	0.118*** (0.033) [56,850]
Inconsistent self-reported voting status with validation	0.229	-0.034 (0.034) [51,723]	-0.018 (0.034) [56,850]
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]
Self-reported voter registration	0.944	0.044* (0.024) [47,353]	0.049* (0.028) [51,777]
Inconsistent self-reported registration status with validation	0.210	-0.066* (0.037) [47,353]	-0.062* (0.033) [51,777]
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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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. Registration data is not available for the 2006 survey. 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: No evidence that *Sesame Street* coverage changed educational attainment or family income

Dependent variable	(1) CCES $\hat{\beta}_1$	(2) CCES $\hat{\beta}_1^{KL}$	(3) Race IAT $\hat{\beta}_1^{KL}$
Educational attainment (years)	0.167 (0.244) [51,713] ⟨13.973⟩	0.283 (0.264) [56,839] ⟨13.973⟩	0.034 (0.059) [327,498] ⟨16.272⟩
Log reported family income (in $1,000)	0.016 (0.057) [46,596] ⟨4.061⟩	-0.011 (0.048) [51,729] ⟨4.061⟩	.
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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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). Numbers in carrots report the mean of the dependent variable. 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 3: *Sesame Street* **coverage increased political knowledge and moderate political identification, but not more involved forms of engagement**

	(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) [50,836]	0.139** (0.070) [55,939]		
Interest in government and public affairs (scale 1-4)	3.4	0.155** (0.079) [46,835]	0.165** (0.078) [51,248]		
Accessed no news media in the past 24 hours	0.056	-0.029 (0.020) [41,890]	-0.018 (0.021) [45,616]		
Panel b: Political identification					
Has a political ideology	0.615	0.024 (0.039) [51,575]	0.015 (0.038) [56,707]	0.804	0.037*** (0.009) [319,563]
..... Weakly identifies with a political ideology	0.396	0.017 (0.041) [51,575]	0.034 (0.038) [56,707]	0.599	0.027** (0.012) [319,563]
..... Strongly identifies with a political ideology	0.220	0.007 (0.032) [51,575]	-0.018 (0.028) [56,707]	0.205	0.010 (0.010) [319,563]
Identifies with a major political party	0.839	0.055* (0.031) [51,527]	0.069** (0.033) [56,659]		
..... Weakly identifies with a major political party	0.418	0.073* (0.043) [51,527]	0.091** (0.040) [56,659]		
..... Strongly identifies with a major political party	0.421	-0.018 (0.040) [51,527]	-0.022 (0.039) [56,659]		
Panel c: Involved political engagement					
Verified congressional primary turnout	0.347	0.026 (0.042) [47,604]	0.014 (0.041) [52,030]		
Donated money to a political campaign or organization	0.241	0.011 (0.038) [43,637]	0.016 (0.033) [47,405]		
Put up a political sign	0.198	0.033 (0.038) [43,637]	0.045 (0.035) [47,405]		
Attended a political meeting	0.134	-0.041 (0.031) [43,637]	-0.006 (0.028) [47,405]		
Worked for a candidate or campaign	0.061	-0.020 (0.021) [43,637]	-0.008 (0.018) [47,405]		
Reports having run for office	0.028	0.012 (0.013) [43,491]	0.014 (0.013) [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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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 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: *Sesame Street* **coverage increased reported voting for minority, women, and Democratic candidates**

	(1)	(2)	(3)	(4)	*p-value of difference*	(5)
	Share	$\hat{\beta}_1$	$\hat{\beta}_1^{KL}$			

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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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. 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 (panel c); 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. Numbers in brackets report the observations used in each estimation, once omitted singletons and observations with missing control variables are omitted. Values between columns 5-6 give the p-value on the interaction term of a 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 *Sesame Street* coverage changed 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 our preferred specification presented in 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: *Sesame Street* **coverage increased reported voting for women Democrats and minority candidates of both parties**

	(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 our preferred specification presented in 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 a 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: Impacts of *Sesame Street* coverage on policy preferences

	(1)	(2)	(3)
Dependent variable	**All**		

Panel a: Policy questions in the CCES survey | | | **Respondent is** |

	(1)	(2)	(3)
Index of support for environmental policies (Scale from 0 to 1)	0.019 (0.025) [56,680] ⟨0.596⟩		
Supportive of allowing gays and lesbians to marry legally	0.122** (0.050) [34,849] ⟨0.558⟩		
		Male	*Female*
Index of support for abortion rights (Scale from 0 to 1)	0.009 (0.029) [56,630] ⟨0.689⟩	-0.010 (0.044) [26,334] ⟨0.661⟩	0.006 (0.043) [29,782] ⟨0.713⟩
		Non-hispanic	*Hispanic*
Index for support of less restrictive immigration policies (Scale from 0 to 1)	-0.078** (0.033) [38,609] ⟨0.448⟩	-0.078** (0.032) [36,713] ⟨0.441⟩	-0.028 (0.170) [1,614] ⟨0.582⟩

Panel b: Race relations and race policy in the CCES survey

	(1)	(2)	(3)
		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)	-0.083 (0.118) [47,378] ⟨2.685⟩	0.070 (0.114) [37,679] ⟨2.529⟩	-0.722** (0.310) [4,741] ⟨3.837⟩
Supports minority affirmative action programs in employment and college admissions (scale 1 - 4)	-0.070 (0.097) [24,726] ⟨2.058⟩	0.033 (0.105) [19,538] ⟨1.868⟩	-0.433 (0.515) [2,403] ⟨3.327⟩

Panel c: Race relations and race policy in the IAT survey

	(1)	(2)	(3)
		White	*Black*
Supports affirmative action index Indicates support on questions regarding affirmative action in employment and college admissions (scale 0-1)	0.064** (0.032) [23,022] ⟨0.229⟩	0.057 (0.038) [16,525] ⟨0.212⟩	0.037 (0.132) [3,312] ⟨0.293⟩
Justifies racial profiling index Indicates racial profiling can be justified in certain situations (scale 0-1)	0.036 (0.023) [23,041] ⟨0.091⟩	0.032 (0.027) [16,568] ⟨0.090⟩	0.077 (0.091) [3,257] ⟨0.088⟩

	(1)	(2)	(3)
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, 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. Numbers in carrots report the mean of the dependent variable. 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 A13 and A14. 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: *Sesame Street* **coverage increased political and party and identification**

	(1)	(2)	(3)	(4)
	CESS		**IAT**	
	$\hat{\beta}_1$	Conditional on lib. or cons. dem. or rep.	$\hat{\beta}_1^{KL}$	Conditional on lib. or cons
Panel a: Impacts on political ideology				
Liberal	0.012 (0.033) ⟨0.239⟩	0.084 (0.056) ⟨0.388⟩	0.025** (0.012) ⟨0.516⟩	-0.006 (0.013) ⟨0.359⟩
Moderate/Neutral	-0.030 (0.039) ⟨0.324⟩		-0.037*** (0.009) ⟨0.196⟩	
Conservative	0.012 (0.040) ⟨0.376⟩		0.012 (0.011) ⟨0.288⟩	
Not sure	0.006 (0.024) ⟨0.061⟩			
Observations	[51,575]	[29,549]	[319,563]	[256,923]
Panel b: Impacts on party identity				
Democrat	0.071* (0.038) ⟨0.429⟩	0.057 (0.046) ⟨0.512⟩		
Independent	-0.058** (0.028) ⟨0.139⟩			
Republican	-0.016 (0.044) ⟨0.410⟩			
Not Sure	0.003 (0.013) ⟨0.022⟩			
Observations	[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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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. 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. Numbers in carrots report the mean of the dependent variable. 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 9: *Sesame Street* **coverage reduced measures of implicit bias against Blacks for white test takers**

	(1)	(2)	(3)	(4)
Sample	**Mean score**	$\hat{\beta}_1^{KL}$		
White test takers	0.145	-0.067*** (0.025) [261,189]	-0.067*** (0.025) [261,189]	-0.051* (0.028) [252,098]
Black test takers	-0.800	0.065 (0.079) [43,397]	0.052 (0.078) [43,397]	0.097 (0.098) [38,601]
Hispanic/Other/Unreported	-0.063	0.041 (0.065) [44,356]	0.043 (0.064) [44,356]	-0.104 (0.074) [38,288]
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: Larger positive values indicate stronger Black-bad and white-good associations. Each coefficient is the result of a separate regression using the indicated controls and fixed effects on standardized race IAT scores. 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 10: *Sesame Street* **coverage had muted impacts on explicit racial preferences reported on the race IAT survey**

Dependent variable and sub-sample	(1)	(2)	(3)
		Test taker is	
	White and Black	**White**	**Black**
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)	-0.028 (0.024) [288,344] ⟨4.580⟩	-0.016 (0.025) [247,539] ⟨4.480⟩	-0.175* (0.105) [40,376] ⟨5.183⟩
.........Reported preference for non-equal respondents	-0.045 (0.039) [123,781] ⟨5.347⟩	-0.031 (0.042) [97,944] ⟨5.210⟩	-0.130 (0.136) [25,443] ⟨5.870⟩
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)	-0.061 (0.040) [297,737] ⟨0.584⟩	-0.056 (0.041) [255,188] ⟨0.381⟩	-0.090 (0.178) [42,109] ⟨1.803⟩
.........Difference in warmth for non-zero respondents	-0.231** (0.095) [112,997] ⟨1.534⟩	-0.192* (0.112) [85,991] ⟨1.126⟩	-0.378* (0.200) [26,614] ⟨2.844⟩
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, 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. Numbers in carrots report the mean of the dependent variable. 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 A1. 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 11: 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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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 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 a 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.

Appendix

Table A1: Exposure to *Sesame Street* coverage is not associated with selection into post-election survey response

Dependent indicator variable	(1) Dependent indicator mean	(2) $\hat{\beta}_1$	(3) $\hat{\beta}_1^{KL}$
Took the post-election survey	0.888	0.001 (0.024) [59,251]	-0.008 (0.024) [64,335]
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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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. 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)
		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
White	White	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
		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
Minority	White	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
		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
White	Minority	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
		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
Minority	Minority	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
		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
All		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 robustness to adding additional controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Turnout		Reports vote for a minority candidate		Reports vote for a woman candidate		White race IAT score	
	With added controls	$\hat{\beta}_1$ on same sample	With added controls	$\hat{\beta}_1$ on same sample	With added controls	$\hat{\beta}_1$ on same sample	With added controls	$\hat{\beta}_1^{KL}$ on same sample
Main specification		0.139*** (0.040) [51,723]		0.271*** (0.082) [11,811]		0.192*** (0.058) [19,034]		-0.067*** (0.025) [261,189]
With added controls for								
Respondent's education	0.131*** (0.040) [51,713]	0.140*** (0.040) [51,713]	0.279*** (0.080) [11,809]	0.274*** (0.082) [11,809]	0.181*** (0.057) [19,031]	0.192*** (0.058) [19,031]	-0.067*** (0.025) [261,189]	-0.067*** (0.025) [261,189]
Respondent's family income	0.151*** (0.041) [46,596]	0.153*** (0.042) [46,596]	0.279*** (0.082) [10,574]	0.273*** (0.083) [10,574]	0.188*** (0.064) [17,151]	0.194*** (0.064) [17,151]		
County Black population share in 1970 × Cohort indicators	0.148*** (0.040) [51,721]	0.139*** (0.040) [51,721]	0.262*** (0.084) [11,811]	0.271*** (0.082) [11,811]	0.207*** (0.062) [19,034]	0.192*** (0.058) [19,034]	-0.067*** (0.025) [260,003]	-0.067*** (0.025) [260,003]
County low income share in 1970 × Cohort indicators	0.114*** (0.042) [51,721]	0.139*** (0.040) [51,721]	0.255*** (0.083) [11,811]	0.271*** (0.082) [11,811]	0.177*** (0.060) [19,034]	0.192*** (0.058) [19,034]	-0.067*** (0.025) [260,003]	-0.067*** (0.025) [260,003]
County Thurmond vote share in 1948 × Cohort indicators	0.138*** (0.040) [51,239]	0.137*** (0.040) [51,239]	0.263*** (0.082) [11,628]	0.262*** (0.083) [11,628]	0.194*** (0.058) [18,846]	0.191*** (0.058) [18,846]	-0.070*** (0.025) [257,687]	-0.069*** (0.025) [257,687]
County Democratic vote share in 1968 × Cohort indicators	0.153*** (0.040) [51,669]	0.139*** (0.040) [51,669]	0.279*** (0.082) [11,811]	0.271*** (0.082) [11,811]	0.201*** (0.060) [19,005]	0.192*** (0.058) [19,005]	-0.068*** (0.025) [259,916]	-0.067*** (0.025) [259,916]
School segregation × Cohort indicators	0.144*** (0.041) [48,908]	0.142*** (0.041) [48,908]	0.260*** (0.084) [11,501]	0.283*** (0.083) [11,501]	0.192*** (0.060) [17,800]	0.183*** (0.059) [17,800]	-0.073*** (0.026) [240,842]	-0.073*** (0.025) [240,842]
Controls: Gender and race	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE: County x cong. district x year	Yes	Yes	Yes	Yes	Yes	Yes	No	No
FE: State x cohort x year	Yes	Yes	Yes	Yes	Yes	Yes	No	No
FE: County	.	.	.	.	.	.	Yes	Yes
FE: State x cohort	.	.	.	.	.	.	Yes	Yes

Note: Each coefficient is the result of a separate regression. Columns 1, 3, and 5 use CCES data to estimate equation 1 on the relevant ballots 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. Columns 2, 4 and 6, report $\hat{\beta}_1$ for the same sample used in the previous column. Estimates for columns 1-6 employ survey weights. Column 7 estimates equation 3 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. Column 8, reports $\hat{\beta}_1^{KL}$ for the same sample used in the previous column. 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 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

Dependent variable	(1) Dependent indicator mean	(2) $\hat{\beta}_1$	(3) $\hat{\beta}_1^{KL}$	(4) $\hat{\beta}_1^{ACS}$
Lived in current city since age 2 (CCES)	0.088	0.032 (0.027) [31,903]	0.039 (0.028) [34,909]	
Attrits from PSID panel (PSID)	0.721		-0.024 (0.095) [4,190]	
Lives in childhood state (PSID)	0.746		0.073 (0.382) [1,052]	
Lives in childhood county (PSID)	0.473		-0.082 (0.293) [1,052]	
$SSCov_j$ of adult county (PSID)	0.630		-0.024 (0.097) [1,044]	
Living in birth state (ACS)	0.613			0.0116 (0.0137) [6,124,373]
Controls: Gender and race		Yes	Yes	Yes
FE: Birth State		.	.	Yes
FE: Cohort		.	.	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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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. $\hat{\beta}_1^{ACS}$ is the coefficient on the interaction between the treated cohort indicator and the population weighted mean *Sesame Street* coverage rate in the state, estimated with *(birth state)* and *(cohort)* fixed effects. Row one uses data from the CCES. Row 2 uses data from the full original PSID sample. In rows 3-5 the PSID sample is limited to those whose county is observed in 2019. Row 6 uses data from the ACS. 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 for columns 2 and 3, and the state level for column 4, 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)		0.3469***	0.8184***	0.1239***
		(0.0019)	(0.0046)	(0.0021)
		[236,880]	[13,964]	[222,916]

 Note: For PSID data, each observation represents a respondent. PSID regressions are weighted by survey weights. For census data, 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. Numbers in brackets report 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: 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	0.0080	-0.0018	0.0340**
	(0.0078)	(0.0092)	(0.0151)
Constant	0.55***	0.55***	0.54***
	(0.01)	(0.01)	(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 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 A7: The impacts of *Sesame Street* coverage on reported reasons for not voting

Dependent indicator variable	(1) Dependent indicator mean	(2) $\hat{\beta}_1$	(3) $\hat{\beta}_1^{KL}$
All reasons	0.151	-0.128*** (0.033) [47,592]	-0.128*** (0.035) [52,017]
...Did not like the candidates	0.024	-0.025** (0.012) [47,592]	-0.028** (0.012) [52,017]
...I am not registered	0.024	-0.030* (0.016) [47,592]	-0.024 (0.016) [52,017]
...I am not interested	0.016	-0.014 (0.012) [47,592]	-0.011 (0.012) [52,017]
...Sick or disabled	0.017	-0.009 (0.013) [47,592]	0.006 (0.014) [52,017]
...I did not feel that I knew enough about the choices	0.014	-0.001 (0.011) [47,592]	-0.019 (0.016) [52,017]
...All other reasons listed	0.057	-0.048** (0.020) [47,592]	-0.052** (0.022) [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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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. 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 A8: No clear heterogeneity in turnout and registration effects by respondent minority status and sex

Dependent indicator variable	(1)	*p-value of difference*	(2)	(3)	*p-value of difference*	(4)
	Respondent is			**Respondent is**		
	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	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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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 a 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 A9: No evidence that turnout effects differ by ballot composition

Dependent indicator variable ... and ballot sub-sample	(1) Observations	(2) Dependent indicator mean	(3) $\hat{\beta}_1$
Verified general election turnout	[51,723]	0.634	0.139*** (0.040)
... white man vs. white man	[22,143]	0.618	0.178** (0.074)
... white vs. white	[37,139]	0.632	0.163*** (0.050)
... minority vs. white	[11,811]	0.640	0.126 (0.080)
... man vs. man	[29,573]	0.620	0.158*** (0.056)
... woman vs. man	[19,034]	0.649	0.121* (0.064)
... midterm elections	[25,032]	0.565	0.171*** (0.054)
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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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 A10: No evidence that *Sesame Street* coverage impacted observed measures of civic engagement

Dependent variable	(1) Dependent indicator mean	(2) $\hat{\beta}_1$	(3) $\hat{\beta}_1^{KL}$
Donated blood	0.135	0.035 (0.030) [43,637]	0.015 (0.029) [47,405]
Was ever a union member	0.279	-0.021 (0.040) [47,646]	-0.029 (0.038) [52,114]
Was ever in the military	0.134	0.017 (0.032) [51,723]	0.003 (0.028) [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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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 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 A11: 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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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 a 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 *** $p<0.01$.

A11

Table A12: *Sesame Street* **coverage increased reported voting for minority, women, and Democratic candidates amongst major party voters, with no evidence of heterogeneity by respondents' turnout validation**

	(1)	(2)	(3)	*p-value of difference*	(4)
			Respondent is		
	Share	**All**	**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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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 a 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   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, even though younger cohorts 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 then 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 coefficients 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 A13: Question estimates of the impact of *Sesame Street* coverage on policy views

Dependent variable	(1) All	(2)	(3)
		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)	-0.140 (0.129) [22,604] ⟨2.909⟩		
B Supports strengthening the Environmental Protection Agency enforcement of the Clean Air Act and Clean Water Act even if it costs U.S. jobs	0.026 (0.050) [33,435] ⟨0.524⟩		
C Supports giving the Environmental Protection Agency power to regulate carbon dioxide emissions	0.029 (0.049) [33,413] ⟨0.631⟩		
D Supports requiring a minimum amount of renewable fuels (wind, solar, and hydroelectric) in the generation of electricity even if electricity prices increase somewhat	0.038 (0.046) [33,442] ⟨0.585⟩		
Abortion policy		*Male*	*Female*
E Supports making all abortions illegal	-0.017 (0.028) [49,724] ⟨0.127⟩	0.007 (0.041) [23,018] ⟨0.123⟩	-0.009 (0.039) [26,196] ⟨0.131⟩
F Supports a woman always being able to obtain an abortion as a matter of personal choice	0.018 (0.041) [56,619] ⟨0.538⟩	0.009 (0.063) [26,331] ⟨0.485⟩	0.015 (0.058) [29,773] ⟨0.585⟩
Immigration policy		*Non-hispanic*	*Hispanic*
G Supports fining U.S. businesses that hire illegal immigrants	-0.025 (0.066) [15,328] ⟨0.675⟩	0.019 (0.067) [14,675] ⟨0.682⟩	-0.964** (0.378) [469] ⟨0.514⟩
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.	-0.077* (0.046) [38,601] ⟨0.481⟩	-0.066 (0.046) [36,705] ⟨0.473⟩	-0.002 (0.281) [1,614] ⟨0.635⟩
I Supports increasing the number of border patrols on the U.S.-Mexican border	0.073 (0.047) [38,595] ⟨0.621⟩	0.085* (0.045) [36,699] ⟨0.626⟩	-0.157 (0.233) [1,614] ⟨0.513⟩
J Supports allowing police to question anyone they think may be in the country illegally	0.111* (0.058) [20,173] ⟨0.425⟩	0.141** (0.059) [19,155] ⟨0.433⟩	0.455 (0.388) [773] ⟨0.270⟩
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, estimated using the same specification as Kearney and Levine (2019), with (*county*) and (*state × 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. Numbers in carrots report the mean of the dependent variable. 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: Question estimates of the impact of *Sesame Street* coverage on race policy and race relations questions

	(1)	(2)	(3)
		Respondent is	
Dependent variable	**All**	**White**	**Black**

Panel a: CCES survey

Structural disadvantage questions

	Irish, Italians, Jewish and many other minorities overcame prejudice	All	White	Black
A	and worked their way up. Blacks should do the same without any special favors.	-0.100 (0.128) [38,769] ⟨3.635⟩	-0.219* (0.121) [30,766] ⟨3.768⟩	1.376*** (0.492) [3,898] ⟨2.610⟩
B	Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class.	-0.132 (0.139) [38,780] ⟨2.677⟩	0.056 (0.142) [30,772] ⟨2.512⟩	-0.625 (0.516) [3,910] ⟨3.894⟩
C	White people in the U.S. have certain advantages because of the color of their skin.	0.035 (0.165) [26,632] ⟨3.126⟩	0.076 (0.185) [21,459] ⟨2.951⟩	-0.322 (0.556) [2,297] ⟨4.468⟩
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.	-0.354 (0.252) [8,112] ⟨3.037⟩	-0.283 (0.262) [6,626] ⟨3.148⟩	4.186** (1.658) [490] ⟨2.052⟩
E	Over the past few years, blacks have gotten less than they deserve.	-0.083 (0.237) [8,120] ⟨2.634⟩	0.122 (0.267) [6,633] ⟨2.470⟩	-1.680 (1.233) [490] ⟨4.043⟩

Other race relations questions

		All	White	Black
F	Racial problems in the U.S. are rare, isolated situations.	-0.039 (0.148) [26,137] ⟨2.366⟩	-0.098 (0.166) [21,046] ⟨2.445⟩	0.169 (0.605) [2,245] ⟨1.650⟩
G	I am angry that racism exists.	0.145 (0.191) [7,945] ⟨4.256⟩	-0.027 (0.231) [6,290] ⟨4.212⟩	-0.143 (0.691) [560] ⟨4.663⟩
H	I often find myself fearful of people of other races.	0.079 (0.248) [7,937] ⟨2.072⟩	-0.043 (0.251) [6,281] ⟨2.088⟩	-0.373 (1.489) [562] ⟨1.981⟩

Panel b: Race IAT survey

Affirmative action questions

		All	White	Black
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?	0.045 (0.037) [17,934] ⟨0.196⟩	0.010 (0.045) [12,953] ⟨0.171⟩	0.407** (0.180) [2,416] ⟨0.289⟩
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?	0.045 (0.041) [17,985] ⟨0.237⟩	0.025 (0.053) [12,901] ⟨0.229⟩	-0.145 (0.170) [2,483] ⟨0.278⟩

Racial profiling questions

		All	White	Black
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?	-0.017 (0.019) [18,091] ⟨0.032⟩	-0.022 (0.020) [13,023] ⟨0.029⟩	-0.006 (0.065) [2,473] ⟨0.040⟩
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?	0.092** (0.036) [17,905] ⟨0.150⟩	0.085* (0.046) [12,914] ⟨0.151⟩	0.177 (0.140) [2,405] ⟨0.139⟩

	All	White	Black
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, estimated using the same specification as Kearney and Levine (2019), with $(county)$ 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. Numbers in carrots report the mean of the dependent variable. 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 A15: No evidence that *Sesame Street* coverage changed 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. $\hat{\beta}_1$ estimates are estimated using our preferred specification presented in 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. 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.

Table A16: Within the selected gender and career IAT data, there is no evidence that *Sesame Street* coverage changed measures of gender and career biases

Sample	(1) Mean score	(2) $\hat{\beta}_1^{KL}$	(3)	(4)
All test takers	0.107	0.012 (0.047) [83,687]	0.016 (0.047) [83,687]	0.046 (0.060) [76,329]
Female test takers	0.210	-0.023 (0.059) [54,826]	-0.012 (0.059) [54,826]	0.022 (0.077) [47,638]
Male test takers	-0.089	0.091 (0.096) [28,280]	0.092 (0.096) [28,280]	-0.023 (0.142) [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 on standardized gender-career IAT scores. 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 *** $p<0.01$.
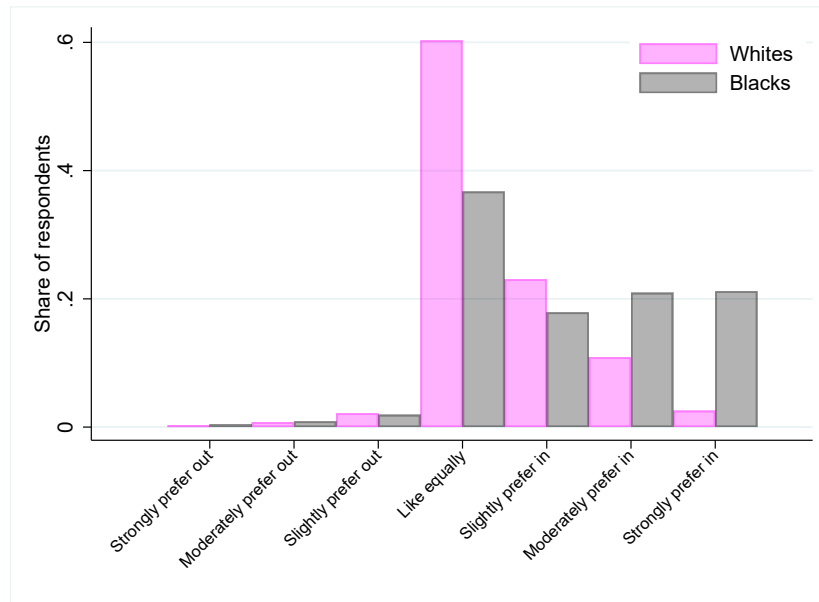
(a) Preference for in-group and out-group Americans
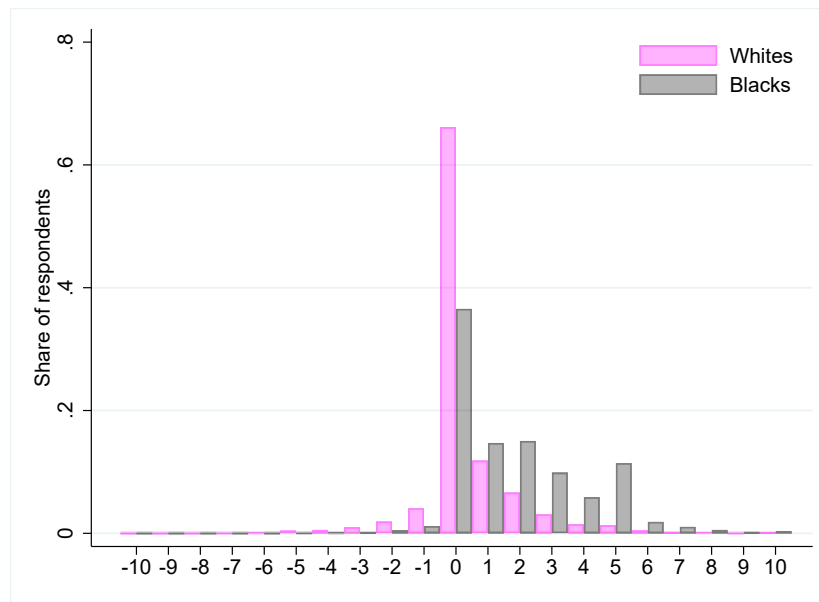
(b) Difference in warmth towards in-group and out-group Americans

Figure A1. 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.

(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

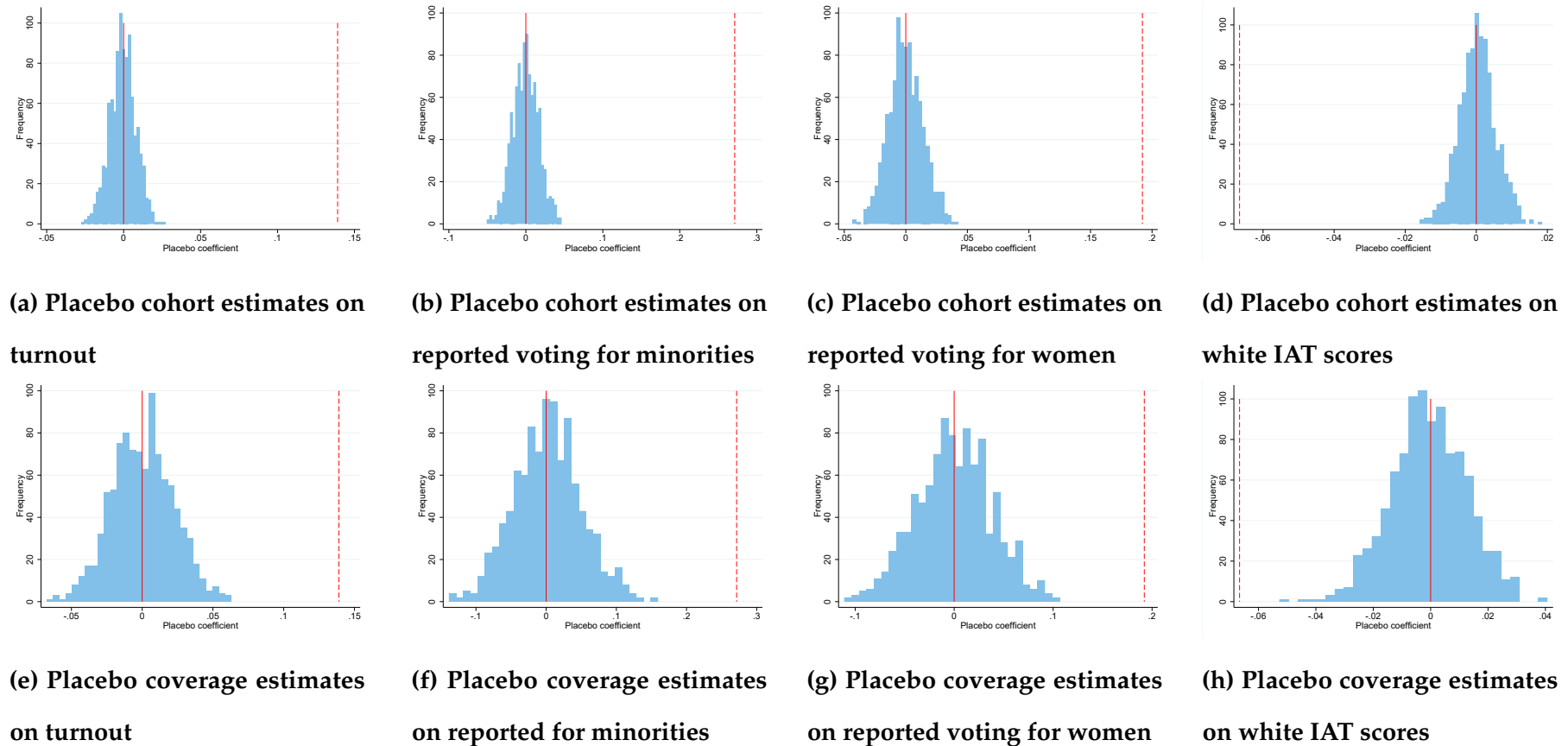**(h) Placebo coverage estimates on white IAT scores**

Figure A2. 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.

A2 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 equations 1 and 3 that include the relevant interactions with a county characteristics ($CountyChar_j$),

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

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

Estimates in column 2 of table A17 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

$$Ratings_m = \pi_0 + \pi_1 MediaMarketSSCov_m + \pi_2 MediaMarketChar_m + \epsilon_m, \tag{1}$$

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 A17.

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 characteristics

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[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.

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 A17: 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
Media market SSCov	0.595*** (0.186) [28]	SSCov × Cohort		0.312*** (0.095) [8,055]		-0.067*** (0.025) [261,189]
Standardized county Black population share in 1970						
Media market SSCov	0.615*** (0.185)	SSCov × Cohort	0.304*** (0.095)	0.312*** (0.095)	-0.067*** (0.025)	-0.067*** (0.025)
Media market mean of standardized Black population share in 1970	-0.051 (0.074)	SSCov × Cohort × Standardized Black pop. share 1970	0.110 (0.140)		-0.001 (0.032)	
N	[28]		[8,055]	[8,055]	[260,003]	[260,003]
Standardized county Thurmond vote share in 1948						
Media market SSCov	0.596*** (0.192)	SSCov × Cohort	0.303*** (0.097)	0.289*** (0.096)	-0.064** (0.026)	-0.069*** (0.025)
Media market mean of standardized Thurmond vote share in 1948	-0.009 (0.079)	SSCov × Cohort × Standardized Thurmond vote share in 1948	0.098 (0.162)		0.022 (0.039)	
N	[28]		[7,949]	[7,949]	[257,687]	[257,687]
Standardized county Democrat vote share in 1968						
Media market SSCov	0.546*** (0.175)	SSCov × Cohort	0.361*** (0.101)	0.312*** (0.095)	-0.070** (0.028)	-0.067*** (0.025)
Media market mean of standardized democratic vote share in 1968	0.075* (0.040)	SSCov × Cohort × Standardized democratic vote share in 1968	-0.094 (0.104)		0.006 (0.024)	
N	[28]		[8,055]	[8,055]	[259,916]	[259,916]
Standardized metro area school white-Black segregation						
Media market SSCov	0.530*** (0.170)	SSCov × Cohort	0.346*** (0.102)	0.331*** (0.098)	-0.071*** (0.026)	-0.073*** (0.025)
Media market mean of standardized school segregation	-0.077 (0.046)	SSCov × Cohort × Standardized school segregation	0.095 (0.080)		0.021 (0.023)	
N	[28]		[7,805]	[7,805]	[240,842]	[240,842]
Southern states requiring school segregation prior to 1954						
Media market SSCov	0.592*** (0.197)	SSCov × Cohort	0.245** (0.124)	0.312*** (0.095)	-0.074** (0.030)	-0.067*** (0.025)
Media market population share in states requiring school segregation	-0.029 (0.062)	SSCov × Cohort × State required school segregation	0.149 (0.191)		0.027 (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 coefficients from a regression of 28 media markets' preschool-age viewership rates on their coverage rate, 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 equation 1 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 equation 3 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.