Labor Supply and Entertainment Innovations: Evidence From the U.S. TV Rollout*

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Abstract

We use a historic quasi-experiment during the regulated U.S. rollout of television to estimate the impact of entertainment technologies on the labor-leisure tradeoff. Using Social Security work histories and variation in television exposure across local areas, we find that the launch of an additional channel is associated with a decline in the probability of working on the order of 0.2-0.5 percentage points. The estimates translate to an hour of TV viewing crowding out about three minutes of work–meaning most TV time substitutes for other leisure activities–and a long-run decline in employment rates of around two percentage points. The effects are largest for older workers and help explain post-war retirement trends. Our results also address the puzzle that employment is declining while wages increase. Increasingly compelling outside options to work can rationalize these trends without large income effects.

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> *Here we must begin with the most fundamental fact about the impact of television on Americans: Nothing else in the twentieth century so rapidly and profoundly affected our leisure.*
>
> Robert Putnam[1]

> *The fact that even in 1950 the average television household was watching for four and a half hours per day makes clear what a dramatic improvement television was over previous entertainment technologies.*
>
> Matthew Gentzkow[2]

1 Introduction

Home entertainment has undergone a massive expansion in variety, quality, and availability, from the early advent of radio and TV to more recent innovations like YouTube and Netflix. Economic theory predicts that an increase in the value of leisure time will reduce labor supply. Aguiar et al. (2021) have recently argued that video games have had exactly that effect, explaining half of the sharp increase in younger men's leisure time in the 2000's. We study this question in the context of the most important leisure innovation of the twentieth century: the launch of television. Moreover, we are able to do so with a well-identified natural experiment, leveraging idiosyncrasies in government rollout rules to generate exogenous variation in the timing of television's introduction across local areas in the U.S.

Next to sleep and work, nothing occupies more of Americans' time than TV.[3] Figure 1 shows that since the early days of television availability, Americans have spent more than an hour and a half hours per day watching television. In a study of time use in the twentieth century, Aguiar and Hurst (2006) find that "More than 100 percent of the increase in leisure can be accounted for by

[1] See Putnam (1998), p. 221.

[2] See Gentzkow (2006), p. 970.

[3] TV is still far more popular than browsing the internet or computer gaming. Watching television takes up over half of American leisure time–55.2% between 2013 and 2017, according to the American Time Use Survey. Note that the BLS counts streaming as television watching, independent of whether this happens on a television screen or computer monitor.

the increase in the time spent watching television" (p. 987). Putnam (1998) notes that "television privatized leisure time" and meant for "less of virtually every form of civic participation and social involvement." While TV undoubtedly crowded out other leisure activities, little is known about its effects on labor markets. This paper exploits the staggered and regulated 1950's introduction of TV stations across the U.S. to estimate the effects of TV on labor supply, a design pioneered by Gentzkow (2006) to study the impact of TV on voter turnout.

Our identification strategy leverages quasi-random variation generated by the Federal Communications Commission's (FCC) rule-based approach to television deployment. This approach addresses concerns that individuals with large amounts of spare time self select into television viewing. The most compelling source of variation arises during an unexpected interruption of the TV rollout. The interruption generates several "ghost stations" that were meant to go live but could not because of the interruption. Constructing and operating a broadcast tower required FCC licensure, and in September 1948, the FCC ceased issuing new licenses while it revised its spectrum allocation plan. The interruption was expected to last about six months, but was ultimately not lifted until nearly four years later, creating credible treatment and control groups during this period. We leverage this quasi-experiment in two ways. First, we compare treated areas (where applications were approved) only to areas where applications were frozen, rather than to the entire untreated sample. We also show difference-in-differences (DiD) results that use all television launches. This approach assumes that the FCC deployment is unrelated to local demand. Historic records indeed suggest that the FCC did not take local demand into account when making its decisions but rather relied on rigid rankings based on fixed local characteristics. The rollout interruption provides a clean placebo test to verify whether the process is orthogonal to demand in practice. We run a placebo test where we estimate the effects of "ghost stations" whose applications were in fact denied *as though they had been approved* to test for spurious effects. Blocked stations did not affect labor supply, lending credibility to the full-sample DiD.

We find statistically significant but modestly sized impacts on work. Specifically, our main results, from individual-level DiD regressions of Social Security work histories on TV exposure, show that the launch of an additional channel is associated with a decline in the probability of working on the order of 0.2-0.5 percentage points. We control for trends in labor demand with different sets of year and place fixed effects, and for individual selection into television viewing with individual fixed effects. The effects arise within age and demographic groups–older people, notably–and are not confounded by demographic changes in the population. We also show that different *trends* in labor force participation between education, gender, racial or marital groups cannot explain our findings.[4]

The estimates translate to an hour of TV viewing crowding out about three minutes of work–meaning most TV time substitutes for other leisure activities–and a long-run decline in employment rates of around two percentage points. For these back of the envelope calculations we use additional data and assumptions. First, we approximate the long-run impact of television by studying when adding more television stations stops having an effect. Our results suggest that this happens relatively quickly after the first three or four stations. Additionally, we translate our results into a total hours worked effect by adding intensive margin estimates. For this exercise, we supplement the Social Security records with data on work hours for a sub-sample of manufacturing jobs and find that work hours were relatively unresponsive, consistent with anecdotal evidence of rigid work schedules in this time period. Our results show that the impact on total work hours comes from an extensive margin decline in lifetime work hours, particularly from earlier retirements. We finally use data on television time use to convert work hours effects into time use elasticities. Taken together, the findings show significant but moderately sized time use elasticities. The overall effect on labor supply, by contrast, is non-trivial. Such aggregate estimates multiply the time-use elasticities with television time investment, and given the immense time spent with television, particularly among some population groups, the overall effects on the labor market are substantial.

We find that the main effects are driven by retirement-aged workers, consistent both with economic intuition relating to workers on the margin of labor force participation, and with the mid-

[4] Several influential studies highlight diverging employment trends among demographic groups (studies of education, gender and racial groups include Binder and Bound (2019); Bayer and Charles (2018); Krueger (2017); Juhn (1992).) Most of these documented trends do not coincide with our sample period in the 1950s and 1960s, though.

century cultural shift of retirement from a mere necessity to an opportunity for "golden years" of relaxation.[5] The share of those aged over 64 who were working halved between 1940 and 1970 (McGrattan and Rogerson (2004)), and among the possible reasons given for this trend in Costa (1998) is the greater availability of compelling, low-cost entertainment like TV.[6] The fact that older workers–who are at the margin of labor force participation to begin with–are most responsive, aligns with the predictions of economic theory. This older population group is also among the most frequent users of television according to time use records (see Figure 1), which lends further plausibility to the view that easily available, high-quality entertainment affects leisure decisions.

Another contribution of this paper is to build the first data set measuring TV signal strength during the U.S. rollout. To date there exist no comprehensive measurements of broadcast reach in this period. Many economics studies, beginning with Gentzkow (2006) on TV's effects on voter turnout, approximate the coverage of 1950's stations with the boundaries of Designated Market Areas (DMA's) from the 2000's.[7] We discuss why this approach generates measurement error, and we produce precise local estimates of historical broadcast reach. Specifically, we digitize information on the technical characteristics of all commercial towers in operation from 1948 to 1960 from annual editions of the *Television Factbook*. We then run the data through the Irregular Terrain Model (ITM) of signal propagation to compute decibel-level signal strength at receiving locations. The chief advantages of the new data are that we more accurately measure the historical boundaries of a given channel, and that we measure coverage intensity–the number of channels available in an area–which makes for an improvement over the binary DMA measure of TV availability.

Our study contributes to three broad literatures, the first relating to the impact of non-wage factors on labor supply decisions. Non-pecuniary attributes of work play a major role in motivating or discouraging work (Le Barbanchon et al. (2021); Maestas et al. (2019); Sorkin (2018); Mas

[5] Interestingly, this is the opposite of the younger demographic emphasized in Aguiar et al. (2021).

[6] Note also that one need not believe people were consciously choosing to stay home and watch TV in order for TV to have had effects on labor supply. Our empirical estimates will aim to approximate an experiment in which some cities had strong TV access while otherwise similar cities had little or none, and one could imagine people in TV cities finding time at home more appealing and entertaining without themselves explicitly attributing subsequent behavior changes to TV.

[7] Subsequent work using this DMA approximation include: Gentzkow and Shapiro (2008); Baker and George (2010); Campante and Hojman (2013); Thomas (2019); Kim (2020); and Angelucci et al. (2020).

and Pallais (2017); Krueger (2017)). Our paper takes this idea one step further and studies how attributes of leisure time affect the labor-leisure tradeoff. Specifically, we focuses on the impact of new leisure technologies.[8]

This idea goes back to classic work in Becker's (1965) "A Theory of the Allocation of Time," which argues that labor supply research primarily focuses on the opportunity cost from foregone earnings but is "not equally sophisticated about other non-working uses of time." This argument sparked an influential line of work into the role of home production. This work studies the impact of new technologies on the productivity in household tasks. Nieto (2020) examines the launch of digital TV in the U.K. from 2008 to 2012 and finds that TV functioned as a substitute for child care, which increased women's employment. Other work studies the introduction of dishwashers, microwaves, washers, and dryers and finds that such appliances acted as "engines of liberation" and increased women's labor force participation by reducing the burden of home production (Greenwood et al. (2005) and related work by De Cavalcanti and Tavares (2008), Coen-Pirani et al. (2010), Ngai and Petrongolo (2017), Greenwood et al. (2016), and Bose et al. (2020).) By contrast, studies on the impact of technologies on the value of leisure are scarce. Two papers in this area examined the impact of leisure technologies through a macroeconomic lens. Most relevant, as discussed above, Aguiar et al. (2021) study how video games changed the labor supply of young men during the 2000's. Kopytov et al. (2020) and Rachel (2020) find that declining prices of leisure technologies could explain employment trends. However, some scholars flag the absence of clean identification as a challenge in these settings. A review of the related literature by Abraham and Kearney (2020) concludes, "the mechanism and direction of the effect warrant consideration, but the point estimates reported unavoidably rest on a good many unverifiable modeling assumptions." Our study leverages a natural experiment to provide such a well-identified estimate of the incentive effects of leisure technologies.

Second, our study contributes to the literature on secular employment and retirement trends

[8] Previous studies of technical change find important effects on production processes and skill demand (for a review, see Acemoglu and Autor (2011)). If new technologies simultaneously affect production and leisure, this can raise identification challenges. In the case of television, the technology was rarely used in economic production, making this setting particularly suitable to isolate the impact on the leisure-labor tradeoff.

(for reviews see, e.g. Abraham and Kearney (2020); Juhn and Potter (2006) and Lumsdaine and Mitchell (1999)). The decline in participation rates among the elderly in the middle of the twentieth century represents one of the biggest shifts in U.S. employment rates over the past century (Blundell et al. (2016); Lumsdaine and Mitchell (1999); Costa (1998)). A long-standing puzzle is that increasing generosity of Social Security appears to explain a major share of the trend until 1940 (Fetter and Lockwood, 2018), but only a minor share of the later trends (e.g., Blau and Goodstein (2010); Anderson et al. (1999); Krueger and Pischke (1992); Moffitt (2012)). Costa (1998) suggests that "the lower price and increased variety of recreational goods has made retirement more attractive" and fostered a new "retirement lifestyle." We provide a simple life-cycle labor supply framework and show both theoretically and empirically that, while television affected everyone equally, the biggest responses occur at the retirement margin. Our study thus provides direct evidence of the "retirement lifestyle" channel and shows that the availability of television contributed to rising retirement rates.

Finally, our results can help rationalize the long-run decline in employment rates that has accompanied rising wages. If leisure is a normal good, increasing wages will lead to falling employment rates. The fact that incomes rose sharply in the post-war years could therefore, in theory, explain the increasing prevalence of earlier retirements (Costa (1998); Boppart and Krusell (2020)). However, the canonical labor supply model requires a backward-bending labor supply curve to rationalize higher wages reducing work. Such behavior is inconsistent with the evidence that in fact *falling* wages are responsible for recent labor supply trends (e.g. Moffitt (2012)) and is outside the range of elasticities typically estimated in microeconomic studies of labor supply (e.g., Imbens et al. (2001); Gelber et al. (2017); Cesarini et al. (2017)). Our paper provides a simple framework to reconcile rising wages, falling employment, and standard substitution elasticities. We argue that wage growth has been accompanied by a simultaneous increase in the opportunity cost of work, so incentives to work have not increased as much as wage growth alone suggests. Taking this into account enables the canonical labor supply framework with standard substitution elasticities to explain trends in labor supply.[9]

The rest of this paper is organized as follows. Section 2 presents a simple model relating innovations in entertainment technology to the labor supply decision, generating a testable prediction. In Section 3, we discuss how we construct the data on TV access, and we then introduce our two sources of labor market data. Section 4 presents the design and main results, followed by placebo tests and heterogeneity analysis showing that the effects are concentrated among workers near retirement age. Section 5 offers a brief discussion on the implications of the findings, including how the results relate to broader trends in wages and labor force participation, and section 6 concludes.

2 Entertainment Technology and Labor Supply

Here we present a simple labor supply framework to study the impact of entertainment technologies. In a standard setup with increasing returns from leisure with age, such technical change has intensive and extensive margin effects, as well as life-cycle implications. Our framework builds on the leisure-labor approach in Becker (1965) and Aguiar et al. (2021). These frameworks are static, one-period models; we additionally allow the value of leisure time to vary over the life cycle. Structural models of retirement introduce similar age-specific utility shocks and model dynamic lifetime optimization problems (see, Blundell et al. (2016) for a review). Our model is similar in spirit, but for simplicity, we assume individuals are hand-to-mouth consumers and abstract away from inter-temporal savings decisions. This more stylized framework still provides useful insights into labor supply over the life cycle.[10]

Consider an individual with preferences over leisure (l) and consumption (c) and utility func-

[9] A complementary interpretation of our findings is that the availability of easily accessible entertainment helped shape new norms around retirement choices. This is in line with evidence that finds a central role for norms in retirement decisions (e.g., Seibold (2020); Costa (1998)).

[10] The retirement literature typically takes one of two approaches. The static approach models retirement as a tradeoff between lifetime income and retirement, analogous to a labor-income tradeoff (Mitchell and Fields, 1984; Burtless, 1986). The dynamic approach models a dynamic, inter-temporal life-cycle decision problem (Gordon and Blinder, 1980; Gustman and Steinmejer, 1986; Blundell et al., 2016; French and Jones, 2017). Our approach is a simplified middle ground between the two. It allows for different choices over the life-cycle but abstracts from inter-temporal savings decisions.

tion $U(c, \xi(a)l)$. The parameter $\xi(a)$ captures heterogeneity in the value of leisure in the population. In particular, we assume that the value of spending time at home increases relative to the value of working as people age; alternatively, one can interpret the assumption as work becoming more taxing as people age, as modeling a rising cost of working or rising value of leisure are isomorphic. Assume that $\xi(a)$ is an increasing function of a, denoted by $\beta(a)$ with $\beta'(a) > 0$, and a shock ν that is independent of age: $\xi(a) = \beta(a) + \nu$.

The wage rate is w and going to work incurs a fixed cost x. This fixed cost implies that working a small number of hours is undesirable and workers will either work substantial hours or not at all (see, e.g. Lazear (1986)). The budget constraint when working is $c = w \cdot l - x + b_0$ and $c = b_0$ when not working, with non-wage income b_0. The optimization problem is:

$$max\ U(m, \xi(a)l) \tag{1}$$

$$\text{s.t. } m = \begin{cases} w \cdot (1-l) - x + b_0 & l \leq 1 \\ b_0 & l = 1 \end{cases}$$

Assume the utility function is quasi-linear with $U(c, \xi(a)l) = c - \frac{\xi(a)}{1+1/\epsilon}(\frac{l}{\xi(a)})^{1+1/\epsilon}$, with ϵ representing the labor supply elasticity. The quasi-linear utility function rules out income effects (we discuss more general functional forms below). Consider a person who is just indifferent between working and not working, and denote this person's value of leisure by $\xi(\tilde{a})$. Figure 2 illustrates this case. All people with $\xi(a) > \xi(\tilde{a})$ will not work and people with $\xi(a) < \xi(\tilde{a})$ will work, implying that people with age $a > \tilde{a}$ are retired.

We can now derive the retirement age in this economy. The marginal retiree is indifferent between working and not working. The utility when not working is $U_0 = b_0 + \frac{\xi(\tilde{a})^{-1/\epsilon}}{1+1/\epsilon}$ and equals the utility at the interior point $U_0 = U^*$. Utility at the interior solution (U^*) follows from utility maximization. At an interior solution the first order conditions imply that $l^* = \xi(\tilde{a}) \cdot w^\epsilon$ and hence $U^* = b_0 + w - x - \frac{w^{1+\epsilon}}{1+\epsilon}\xi(\tilde{a})$. Combining this result with $U_0 = U^*$, we get an implicit expression for $\tilde{a}$:

$$\xi(\tilde{a})w^{1+\epsilon} + \epsilon\xi(\tilde{a})^{-1/\epsilon} - (1+\epsilon)\left[w - x\right] = 0 \tag{2}$$

We can use this expression to derive comparative statics and analyze the impact of leisure-enhancing technologies. Such technologies increase ν and have two effects on labor supply. First, it affects the optimal labor supply:

$$\frac{\partial l^*}{\partial \nu} = w^\epsilon > 0$$

For all workers at an interior solution, leisure consumption increases by w^ϵ. The greater utility of leisure leads to a marginal reduction in work hours.

Moreover, such technological changes have extensive margin effects and push a greater share of people to shift from l^* to $l = 1$. The effect operates through a falling retirement age. Using the implicit function theorem on equation 2 yields:[11]

$$\frac{\partial \tilde{a}}{\partial \nu} = -\frac{1}{\beta'(\tilde{a})} < 0$$

A rising value of leisure thus leads to earlier retirements and increased exit from the labor force. Figure 2 shows the intuition behind this result. The rising value of β_0 pivots the indifference curve upward and makes it steeper. This implies that the new marginal retiree has $\xi(\tilde{a}^{'}) < \xi(\tilde{a})$, and hence $\tilde{a}' < \tilde{a}$. The new marginal retiree is thus younger and individuals with age between $\tilde{a}'$ and $\tilde{a}$ will have exited the labor force.

The model offers three simple insights. First, leisure-enhancing technologies reduce labor supply both at the extensive and intensive margin. Second, the group that responds most are older workers whose relative value of leisure is highest. This group is at the margin of labor force participation to begin with and therefore most likely to respond to leisure-enhancing technologies by exiting the labor force. Third, while the value of leisure changes only marginally, the labor supply responses is still substantial among some groups. A fixed cost of work implies that some people jump from near full-time participation to not working at all.

[11] There is a knife edge case.

The simplicity of the results hinges on the functional form assumption, but some of these predictions hold more broadly. Intensive margin results on l^* are sensitive to parametric assumptions. If individuals have a strong income effect, the direction of the change could go the other way and the impact of entertainment technologies at the intensive margin in the general model is thus ambiguous. This highlights one of the problems with testing intensive margin effects of entertainment technologies. Studies typically assume that income effects are small or absent to arrive at unambiguous predictions about l^*. Our extensive margin predictions, by contrast, are not sensitive to the functional form assumptions. These results are one of the few predictions of the general labor supply framework that hold independently of the parametric assumptions about the utility function.

3 Data

Our study combines a newly built data set on television signal strength in the 1940's and 1950's with administrative employment records. The television data are based on archival records of broadcast towers and a model of signal propagation, and the employment data primarily rely on work histories from the Social Security Administration. We next discuss each in turn.

3.1 Measuring TV Access

To date, there are no comprehensive measurements of TV signal strength during the U.S. rollout. Previous studies typically approximate the coverage of 1950's stations with the boundaries of Designated Market Areas (DMA's) from the 2000's.[12] We digitize archival records to precisely measure television signal reach. The chief advantages of the new data set are twofold, which we describe in detail below. First, we more accurately measure the broadcast boundaries of each given station; and second, we measure coverage intensity–the *number* of channels available in an area–which makes for an improvement over the binary DMA approximation of TV availability.[13]

[12] Work using this DMA approximation includes: Gentzkow (2006); Gentzkow and Shapiro (2008); Baker and George (2010); Campante and Hojman (2013); Thomas (2019); Kim (2020); and Angelucci et al. (2020).

[13] In appendix section 8.2, we revisit the results in Gentzkow (2006) and Gentzkow and Shapiro (2008) using the new ITM data.

Commercial television was first licensed for broadcast in 1941, with experimental stations in a few major cities like New York and Los Angeles. The rollout took off after World War Two, and the post-war expansion was a staggered city-by-city process over the following two decades whose timing was governed in part by a sharp regulatory freeze. The freeze came about due to signal interference between neighboring stations, an issue that occured due to an error in the FCC's signal model. This interruption plays an important role in our identification strategy and we return to this topic below. Most of the growth in coverage and viewership occurred in subsequent years, during the 1950's; in 1950, less than 20 percent of households owned a TV, and by 1960, 87 percent did (see Gentzkow (2006) for a detailed discussion of the rollout process). Our first contribution is to produce precise measurements of TV access in this period.

We use the Irregular Terrain Model (ITM) to calculate signal reach during the rollout. The ITM computes signal strength in decibels at a receiving location as a function of the distance of that location from a broadcast tower, tower technical specifications, and topography between the tower and receiving location.[14] The new data has two advantages. First, we reduce measurement error and discuss such improvements in detail in Appendix A. Second, the DMA approximation ultimately produces a binary coverage variable. Since different cities also had different numbers of channels, and some pioneering stations had limited broadcast hours, a binary treatment indicator can miss variation of interest in the intensity of TV "treatment." With the ITM, we can separately calculate signal strength for each individual channel and therefore track the rising availability of TV at both the extensive and intensive margins.

Using the ITM requires detailed information on broadcast towers. We collect three sets of data on broadcasting technology from early editions of the *Television Factbook*, a trade publication for advertisers and other industry players. First, beginning in 1948, the *Factbook* published the technical characteristics of all commercial stations in operation. We use these as inputs for the ITM. Specifically, for each station in each year from 1948 to 1960, our digitized *Factbook* data

[14]The ITM model has also been used in other countries: Olken (2009); Enikolopov et al. (2011); Della Vigna et al. (2014); Yanagizawa-Drott (2014); and Durante et al. (2019). Wang (2020) also uses the ITM to estimate the effects of a 1930's populist radio program in the U.S..

include latitude and longitude, height above ground, channel number and frequency, visual and aural power, and other details like call letters and start date. There were 41 stations on air in 1948. Already by 1960, there were 570.[15] We estimate the signal strength of each station at the geographic center of each U.S. county from 1948 to 1960.

The second and third groups of data involve secondary extensions of original broadcasts. A town across a mountain range from a nearby city would be cut off from that city's TV signals, and the ITM would correctly measure that town as having no TV access through the air. However, some towns constructed antennas on top of the mountains to capture signals and then wire the broadcasts into the otherwise obstructed homes. This was the birth of cable TV and was known at the time as Community Antenna Television (CATV).[16] We have digitized the *Factbook* directories of CATV locations, start dates, and estimated number of subscribers. Finally, an alternative to piping a signal through a CATV system was to rebroadcast it through the air with small antennas called translators. The *Factbooks* record the locations of licensed translators beginning in 1957, and we have digitized them through 1960.

Finally, we use data on pending applications to the FCC for broadcast licenses from *TV Digest*. These data aid identification because an unexpected FCC licensing freeze halted approval of all new stations from September 1948 to April 1952. Stations whose applications were approved before the 1948 freeze were allowed to continue broadcasts, but those pending approval when the freeze took place could not begin broadcasting until the freeze was lifted four years later. Data on frozen applications combined with the ITM allows us to implement a novel empirical strategy like that in Koenig (2020), which is to compute the signal strength of stations that were in reality blocked by the FCC *as though they had been approved*, which produces a powerful placebo test. If a regression specification using these "ghost towers" shows effects of TV where there was none, then that specification must reflect spurious correlations.

[15] Latitude and longitude are first published in the 1952 *Factbook*. Earlier years give station addresses, which we geocode. The *Factbook* was published four times per year in 1948 and 1949 and twice per year from 1950 to 1960. We digitize the latest edition available in each year.

[16] In 1966, both the *American Economic Review* and the *Quarterly Journal of Economics* published articles on CATV; see Fisher (1966) in the references.

Figure 3 shows a snapshot of the ITM output in 1950. Here we have mapped the strongest signal available in each county. The units are decibels, where zero indicates top-quality signal strength. Any signal below -50 decibels was effectively unwatchable, and we have colored the figure to indicate that coverage transition as the map shifts from red to blue. City centers are clearly visible, as is the fading strength of the signals–a typical broadcast reached about 100 miles from its tower, leaving some counties well outside of urban centers still within reception rings but others out of range. This is an extensive margin perspective on the data, in the sense that the map displays whether a county could receive a watchable signal from any station. We also estimated the number of stations available in each county in each year.

3.2 Employment Data

Our main source of labor market data is the Current Population Survey (CPS) Social Security Earnings Records Exact Match file (henceforth "SSA-CPS"), which matched respondents from the March 1978 CPS to their Social Security earnings histories.

The full SSA data covers work histories of the near universe of U.S. workers going back to the 1930's. Our data is a sample based on the individuals in the March 1978 CPS, and matches those individuals to their full Social Security history.[17] The data is a worker-level panel and is one of the only micro data sets that covers years between the decadal Censuses during this period. We focus on the adult population in the mainland U.S. and study changes in working behavior between 1937 and 1960.[18]

A key appeal of the data is that it is a panel that tracks individuals over time. This allows us to address a major challenge for location based studies: changes in the composition of local labor markets. In the panel data, we can control for individual fixed effects, hold observed and unobserved fixed individual characteristic constant, and identify effects through changes in individuals

[17] This dataset was initially compiled by the Bureau of Labor Statistics to evaluate survey responses in the CPS; aside from such evaluations, the data has been underutilized by researchers. A notable exception is Acemoglu et al. (2004) who study labor supply behavior of women in the post-war period. The data is available as ICPSR repository 9039.

[18] Adult age was 21 at the time.

careers.

A further appeal of the administrative data is that the records are based on employer reports to the SSA, and relative to retrospective survey data such third-party reported data tends to be more accurate. A drawback of administrative data is usually the lack of detailed demographic information. Since our data is based on the CPS, we can link the SSA records to information from the CPS. This allows us to use information on workers' age, race, education, occupation, and place of residence. The residence information is the metropolitan statistical area (MSAs) and rest of state for non-MSA residents, and we run the regressions at this geographic level.[19]

For each individual we observe the number of qualifying quarters worked per year and we code an individual as employed if they worked at least half a qualifying quarter.[20] The data reports are at annual frequency during the 1950's, however in earlier years multiple years are grouped together and multi-year summary records are available.[21] Our baseline sample includes the annual data for 1951-1960 and two multi-year observations representing the average of 1937-1946 and 1947-1950, respectively. Appendix section 9 provides further details on the data.

We additionally account for the expansion of Social Security coverage and the Korean War in the 1950's. The Social Security administration expanded their definition of employment during the 1950's. We drop individuals who are affected by the coverage expansion to work with a consistent sample.[22] The start of the Korean War led to a draft and we exclude drafted soldiers from the analysis to avoid spurious employment effects from the draft.

Our sample is representative of the 1978 CPS cohort and thus not representative of the U.S. in earlier years. Note that this does not affect the validity of our local average treatment effect (LATE) estimates. We are, however, also interested in whether the LATE generalizes. Appendix 9 therefore constructs weights and estimates effects on a representative U.S. population. The results

[19]Finer geographic data would provide little additional variation, since television signal reach usually coincides roughly with MSA boundaries. As a result, we lose relatively little information by aggregating data at the MSA level.

[20]SSA qualifying quarters may differ from quarters worked if earnings in a quarter are below the qualifying threshold or if a person works in non-qualifying employment (e.g. some self-employment).

[21]While the SSA imputes annual values, we do not make use of these imputations. The imputations assign total quarters consecutively across the years until they run out and hence the timing does not contain additional information relative to the raw data files.

[22]Details on the data cleaning process are reported in appendix section 9.

are similar, with slightly bigger effects.

Typically, administrative data lack demographic information. Our matched SSA-CPS data provides a rare opportunity to combine administrative labor market records and detailed demographic information from the CPS. However, demographic information is collected in 1978 and we do not observe it at a yearly level. This means, for instance, that we only observe the place of residence in 1978. Previous studies note this difficulty and treat demographic information as fixed throughout the sample period (e.g., Acemoglu et al. (2004)). As with these previous studies, this approach has drawbacks as people may not have lived in the place we assign them to. Such measurement error thus works against us finding effects. In our baseline approach we follow Acemoglu et al. (2004) and treat demographics as fixed throughout the sample period; our regressions thus have the spirit of an intent-to-treat (ITT) effect. Keeping people's location fixed rules out that spurious moves towards television areas affect our results. Still, we present an ITT estimate, which is a lower bound for the average treatment effect on the treated (ATT). Appendix 9.3.4 compares our ITT estimates to the ATT and shows that the results are close to the ATT for realistic migration patterns.

We use an additional data source to study hours worked and the intensive margin labor supply responses to television. In the 1950's, data on work hours was collected for national statistics but rarely reported at geographically disaggregated levels. However, several regional offices of the Bureau of Labor Statistics published local area breakdowns of hours data. These records are summarized in the Current Employment Statistics (CES). The data come from surveys of non-agricultural employers in the manufacturing sector and include average hours worked by location. These reporting areas in the CES are typically MSAs or state level aggregates. Our sample includes 51 local areas and covers the period 1947-1960. The panel is thus relatively small but provides a glimpse into intensive margin effects.

4 Empirical Analysis

We now turn to estimates of the impact of television on labor supply. We make use of the natural experiment that arises from the regulated rollout of television in the 1940's and 1950's. Television station launches were staggered over two decades, leading to substantial regional heterogeneity in access. Our analysis uses this variation in the following difference-in-differences regression:

$$E_{aigt} = \gamma_{gt} + \delta_i + \beta_g \cdot TV_{at} + \pi \cdot X_{aigt} + \epsilon_{aigt}, \tag{3}$$

Here the outcome E_{aigt} is a dummy with value 100 if individual i of gender g in area a at time t is employed, and TV_{at} denotes the number of available TV channels in area a at time t. An alternative specification would use only the first television launch and we explore this further below. We do not use such an approach as our baseline since first stations were typically experimental and had limited broadcast hours, moreover many of these first launches happen before our outcome data becomes annual. Time fixed effects (γ_{tg}) absorb aggregate trends in labor supply. We allow for different year effects by gender since employment trends were different in the post-war period. Individual fixed effects (δ_i) control for individual preferences and characteristics; these also absorb area effects, since we assign individuals to a time-invariant area a. Finally, X_{aigt} is a vector of control variables. The main effect of interest is captured by β_g, which we allow to differ across men and women.

The main identification assumption is that television launches are orthogonal to other local labor market trends. We will use the freeze experiment, as well as conventional parallel trend checks to probe this assumption. Another potential threat to identification are moves across boundaries and changes to composition in the local labor force. We address this in two ways, first we use individual fixed effects to control for observed and unobserved individual characteristics and thus alleviate most selection concerns. Second, we address spurious moves by treating the location of individuals as fixed throughout the sample.

4.1 Results: Social Security Records

Table 1 shows estimates of the differences-in-differences specification in equation 3 using the Social Security data. During the 1950's several changes to national policies and trends in norms shaped aggregate labor market trends[23] and our identification uses variation at a local level to separate the impact of television from these aggregate trends. The analysis compares locations differentially affected by television and uses year fixed effects to absorb the impact of aggregate trends and individual fixed effects to control for generation-specific work patterns. The results show that an additional TV channel reduced the probability that an individual was employed by between 0.2 and 0.6 percentage points. These are relatively modestly sized effects, given the high employment rates around 78% for men and 40% for women in our sample.

A large literature has documented that employment probabilities evolve over the life cycle. Accordingly, in our preferred specifications we account for these changes by controlling for age group fixed effects (from Column 2 onwards). And find similar results after adding these controls.

Another potential worry is that aggregate year fixed effects do not adequately capture the impact of broad societal trends. We probe this possibility by introducing different time trends across demographic groups (Column 3). Specifically, we allow for different trends by schooling, age, race and marital status groups, while continuing to control for gender-specific year effects. Such controls thus explicitly address trends introduced by shifting gender and family norms, as well as by expanded schooling, more generous retirement packages, and changing life-cycle work patterns. These controls have little impact on the results, which suggests that the aggregate year fixed effects do reasonably well at absorbing relevant trends. We next repeat this exercise in a more flexible way and introduce region- and state-specific time trends (Columns 4 and 5). Such controls capture potential spurious trends that could arise not only from demographics but also from unobserved factors. The results are again similar across these specifications.

Studies of local labor markets can also be biased by workers who move across labor market

[23] See Albanesi and Olivetti (2016); Goldin and Katz (2002); Fernández et al. (2004) for evidence on changing household decision making, Goldin and Margo (1992) for rising demand for skilled workers, and Smith et al. (1989) for anti-discrimination policies.

boundaries. For example, departures of people with strong labor market attachment from areas in which TV first launched would lead to spurious negative employment effects. Here the panel structure of our data is of great help. Recall that we control for individual fixed effects and thus hold both observable and unobservable characteristics of workers constant. Composition effects are thus not a concern in this setting.

A more subtle problem arises if moves occur at the same time as changes in employment. Take a person who becomes unemployed and moves to a city where television is available. In this case individual fixed effects will not resolve the resulting biases. By their nature, individual fixed effects are time invariant and do not capture the persons' employment status change. Fixed effects alone therefore do not fully resolve the potential challenges from migration. To address these more subtle issues, we can again leverage the data's panel structure again and treat an individuals' location as fixed throughout the analysis. By assigning individuals to the same locations, we rule out that migration drives the findings.

Ideally, we would like to assign people to their places of birth and estimate an intent-to-treat (ITT) effect. As described above, such data is unavailable in the Social Security records, and we instead follow the approach of Acemoglu et al. (2004) and assign individuals to their 1978 residence. This has a similar interpretation as the conventional ITT effect with one additional drawback–if people stop working and move to an area in the exact year of a television station launch in that area, our baseline specification picks this up as an effect of television. In this case, we cannot distinguish causal effects from coincidental moves. Appendix 9.3.4 performs a bounding exercise to assess the potential biases from this source and shows that the impact on our estimates is minor.

Overall, our results show similar responses by men and women. In absolute terms, the effects are smaller and less significant for women (Table 1). An important driver of these differences are the lower baseline employment rates among women. Importantly, the effect sizes for men and women are comparable in relative size; both experience roughly a 0.6% decline in employment upon the launch of an additional station. The comparable responsiveness suggest that both groups have similar underlying utility functions and share fundamental preferences.

An underlying assumption of the identification strategy is that the rollout process is unrelated to local trends. Historical records of the rollout rules indeed suggest that this is likely the case. The FCC processed launch permits according to its internal priority ranking of locations. The position in this ranking was based on largely fixed location characteristics (e.g. in 1956 on population and distance to nearest antenna). The determining factor of the launch timings was thus the rank position of a place and speed of FCC processing. Local demand conditions, by contrast, had no effect on the timing of television launches. These institutional features thus give us reason to be optimistic about the baseline difference-in-difference results. The following section will go further, examining pre-trends and using disruptions of the planned process to probe the validity of the research design.

The difference-in-differences analysis is credibly causal only if the treatment and control groups have parallel pre-trends. Since our treatment variable is continuous, we use two versions of pre-trend checks. The first uses simple leads and lag values of our treatment and is reported in the Appendix 9.3.1. The second approach uses a distributed lag model, as suggested in a series of recent work on difference-in-differences settings like ours (Fuest et al. (2018), Serrato and Zidar (2016), and Drechsler et al. (2017), and Schmidheiny and Siegloch (2019)). This uses the following first-difference transformation of equation 3:

$$\Delta E_{iagt} = \alpha_{gt} + \underbrace{\sum_{j=0}^{a} \beta_{g,j}\ \Delta\text{Channels}_{a,t-j}}_{\text{Lagged Stations}} + \underbrace{\sum_{k=1}^{a} \beta_{g,k}\ \Delta\text{Channels}_{a,t+k}}_{\text{Future Stations}} + \mathbf{\Pi}\boldsymbol{X}_{iagt} + \Delta\epsilon_{iagt} \tag{4}$$

the β_j coefficients capture the past impact of lagged stations and β_k the impact of future stations. The time pattern of a station's impact is plotted in Figure 4. The figures show that treatment and control regions evolve in parallel in the years leading up to the launch of a TV channel. The differences are close to zero and insignificant in the lead up to television launches, and after the launch of a TV station employment declines in the affected location. The clear change at the time of treatment indicates that the difference-in-difference specification is capturing the effects of TV and we can rule out that differences in trends are driving our results.

4.2 Identification Tests

Having explored a variety of controls for alternative trends, we now leverage additional policy variation to sharpen the tests. The unexpected interruption of the rollout process provides a natural experiment where some locations narrowly miss out on television launches. We use affected areas in a first exercise to test for spurious effects.

We perform two tests–first, a horse race between blocked TV station launches and actual launches and investigate if labor effects are different in places with launches compared to locations where such launches are blocked. We find that negative employment effects arise only from launched and not from blocked television stations (Table 2, Columns 1 and 2). The rollout thus does not appear to be related to spurious local labor demand shocks. This is a powerful identification check, as we observe places that were meant to be treated in an untreated state of the world. We can thus inspect spurious effects at the time period of the supposed treatment. The results confirm that the rollout rules are unrelated to such spurious shocks.

Second we narrow in on places that received television around the rollout interruption. The idea here is that close to the cutoff places are economically and demographically similar, and the gap in TV dates arises because of the interruption of the rollout. To exploit this source of variation we repeat our analysis and compare places that received television right before the interruption to those where television was launched right after the interruption.[24] These estimates show a clear negative effect of television; the effect of a station launch is again around a 0.4 percentage point decline in employment. This is close to our baseline estimates and suggests that the raw rollout variation provides reasonably reliable estimates.

Next, we stretch the experiment further and hone in on areas that had recently received tele-

[24]Recall that the interruption lasted from September 1948 to April 1952; we here focus on areas with launches between 1947 and 1954.

vision and those that were next on the priority list but had the launch blocked. Relative to the previous test, this excludes areas that leapfrogged in the priority ranking during the rollout revision and received television immediately at the end of the interruption. Instead, it focuses on places that were ranked consecutively in the initial priority ranking. For this test, we focus on the years when launches are affected by the interruption, either due to the hold-up or the subsequent catch-up period (1947-1954).[25] Such estimates stretch our sample thin but have the advantage that they exclusively rely on years when differences in TV access are caused by the policy intervention. Our estimates again show significant negative effects of television and confirm that the effects arise at the time of television launches and only if a station is actually launched. These interruption experiment estimates are again relatively close to our baseline results, which helps to rule out that spurious correlations are driving our previous difference-in-differences specifications. Columns 2, 4 and 6 additionally allow for separate time trends by demographic groups and show similar results.

4.3 Results: Current Employment Statistics

We next investigate the impact of television on work hours. So far we have focused the analysis on extensive margin responses, and we now additionally allow for changes to hours worked. The Social Security data does not contain information on work hours so we supplement our analysis with data from the CES. Recall that this data focuses on manufacturing workers. This data is available at the MSA level and we therefore run the difference-in-differences analysis at this more aggregated level.

We first replicate the employment regressions in the CES data. The results show negative employment effects and broadly align with our baseline SSA results (Panel A in Table 3). The smaller sample size of the CES, however, reduces the power of these estimates and the results are therefore not statistically significant. Because of the reduced sample size, we first show results that

[25] Ideally, one would also exclude 1947, the year before the interruption. However, the reporting of multi-year averages in the SSA data of the 1940's does not allow to separate 1947 from the 1947-1950 bin. Results that exclude the 1947-1950 observation show similar effects.

replace the flexible year fixed effects with more restrictive year trends and subsequently allow for more flexible time effects (cubic, state specific trends) and ultimately non-parametric year effects. All the specifications show similar effects, with point estimates around a one percentage point decline in employment.

We can now turn to analyze work hours. Panel B estimates the impact of television on total hours worked, the product of employment and average hours worked. We again find a decline by about 1 percentage point. The employment effect thus explains nearly all of the change in total hours worked, whereas average hours worked are unaffected by the launch of television stations. This result aligns with historical accounts of the labor market in the 1950's, when workers had only limited control over working hours. Work hours were largely set through union agreements and there was minimal scope for part-time work. The extensive margin was thus the main plausible margin of adjustment and that is indeed what we find in the data.

4.4 Heterogeneous Effects: The Role of Retirement

We next turn to job flows and study the behavioral changes that are driving the results. The main impact of television is on workers near or above retirement age, with only modest effects on workers under the age of 55.

To evaluate the retirement hypothesis more directly, Figure 5 disaggregates the overall effects into three possible transition rates by age. We differentiate entries, exits and retirements and define retirement as a permanent exit from the labor force. The long-run work histories of the longitudinal Social Security data allows us to observe whether individuals return to work later in life and we define retirement as permanent exits from the workforce. The results show a large and significant increase in retirement rates among older workers. Among the age group over 65 the probability of retirement increases roughly 2 percentage points, while reassuringly we find no discernible effect on the retirement of age groups below 55 (Figure 5). These retirement effects are also substantially larger than the effects on other labor market flows. Figure 5 shows some modest changes in labor market entry rates, however these effects are dwarfed by the magnitude of retirement effects.

Moreover, the increase in the exit rate among older workers is largely due to rising retirement probabilities.

The results are consistent with prior evidence that the 1950's were a period that transformed the perception of retirement. In earlier decades retirement happened when people could no longer work; in the middle of the century attitudes shifted and retirement became seen as a desirable third stage of life with additional time for leisure activities (Costa (1998)). Our finding supports Costas' hypothesis that the cheap availability of around the clock entertainment contributed to this trend.

5 Discussion

We next analyze how such entertainment innovations have affected labor supply trends. In a first step, we look at the steady state effect of the television rollout and calculate the employment effect of universally available television. To do this, we explore how the impact of TV changes with a growing number of local stations. We run separate regressions that exploit a growing number of station launches, starting with only the first station, the second and so forth.[26] We then multiply the effect per station with the number of active stations in treated areas and obtain the implied "steady state" employment effect. Figure 6 shows that the negative employment effect increases as we take the first few stations into account. Using only the change from zero to one station, we find modest, insignificant negative effects. These small effects of the initial station may seem surprising at first, but are likely driven by the extremely limited broadcast hours that were typical of pioneering stations. Hours and variety expanded with the entry of competing stations. In line with this, we see the employment effects grow once we take subsequent station launches into account. This pattern holds up to the third station, after which we do not observe additional effects from more launches.

The employment effect stabilizes at a 2 percentage point decline in the employment-to-population ratio. Since the results are relatively unmoved by further television launches, we consider this the long-run steady state effect that prevails when television is universally available. Most places had

[26] Note that this approach is more flexible than imposing a specific polynomial structure of effects.

three or more stations by 1960 but coverage was far from universal. That universal coverage was likely reached in the 1980s or 1990s when cable and satellite television broadcast to all U.S. areas. We thus evaluate our results relative to the employment trends over this period. A decline in the employment to population ratio of around 2 percentage points compares to a 12 percentage point decline among men between 1950 and 1990. Most of this decline coincides with the peak television rollout in the 1950's and 1960's but the magnitude of our results could at best explain a sixth of the overall decline. This is a non-trivial effect but is modest relative to findings on the effect of other factors, such as the launch of the Social Security two decades earlier. Fetter and Lockwood (2018) find that Social Security led to a decline of labor force participation among older workers of around 8.5 percentage points between 1930 and 1940. Importantly, female labor market trends were markedly different during this period and employment rates were growing rapidly. Our results show negative employment effects for women too, albeit smaller than for men.

Note that our results do not aim to distinguish socially optimal responses from self-control problems. Modern entertainment technologies are designed to captivate and draw users in, and some argue that this has led to excessive consumption of the likes of mobile phones, social media and television series. Our results do not distinguish such effects but rather aim to quantify overall employment changes and the resulting impact on labor market trends.[27]

Finally, we use our estimates to provide benchmark estimates that can help inform the broader debate on entertainment technology and labor supply. If we are willing to assume that leisure preferences are similar, our setting can shed light on the impact of such technologies on work behavior more generally. To compare different types of entertainment technologies, we first need to measure the "quality" of an entertainment technology. People will likely disagree on what is high vs. low quality entertainment, and we aim to cut through this debate with a revealed preferences approach that uses observed behavior to infer quality. Specifically, we measure the time spent on an entertainment activity. The intuition is that people will spend more time with a more appealing

[27]For a welfare analysis one would distinguish addictive behavior from socially optimal behavior. Our estimates thus do not provide a full welfare analysis, in keeping with the drug addiction literature, which typically estimates aggregate labor market effects (Krueger (2017)). It seems highly likely to us that the addiction component is far weaker in the case of television.

entertainment activity. Data on time use shows that in 2010 the average American spent 19.3 hours watching television per week and provides a numeraire for our estimate.[28] With this data we can compute time use elasticities which capture how many minutes of work are lost from one hour of television watching.[29]

The numerator of the time use elasticity is the steady-state labor supply response. We convert the two percentage point decline in employment into a work hour effect. Using a 40 hour work week, this estimate implies a decline of $0.02 \cdot 40 = 0.8$ hours per week.[30] Since we found no additional intensive margin effects, these estimates represent the total decline in work hours.

Finally, we divide the work effects by television time use to compute the time use elasticity. This shows that one hour with television reduced work by approximately by $0.8/19.3 \cdot 60 = 2.5$ minutes. In other words, the vast majority of the time – over 57 minutes – of an hour spent with television crowds out non-work activities. Leisure innovations thus appear to primarily replace alternative leisure activities.

These results suggest that recent entertainment technologies have had a modest impact on aggregate employment so far. Given the small magnitude of the elasticity, entertainment technologies only have a sizable aggregate impact if the time investment is very large. As a benchmark, time spent with computers for recreational purposes takes up less than 3 hours per week (excluding computer games). Computer gaming is even less common. Recent evidence suggests that the heaviest user group of computer games — men aged 21-30 — spend around 3 hours weekly with computer games, while the rest of the population spends less than one hour on such games (Aguiar et al. (2021)). These time investments translate into a 0.1 decline of work hours per week for young men. While this is a non-negligible effect, it would only account for roughly 5% of the recent de-

[28]Using alternative base years for the long-run steady state analysis leads to similar results because hours of TV watching increased only slightly since the 1980's. The source for this data is the BLS series TUU10101AA01027132.

[29]Similar elasticities are popular in studies of consumer surplus created by entertainment goods, since time-spending elasticities capture demand behavior even if there is no monetary expenditure (e.g., Goolsbee and Klenow (2006)). To see the relation to consumer surplus, note that the demand and expenditure elasticities are closely related. Differentiating expenditure $e(p, q) = pq(p)$ with respect to price p yields an expression for expenditure elasticity ϵ_e in terms of demand elasticity $\epsilon_e = 1 + \epsilon_q$.

[30]Average hours worked are in this ballpark range and declined from 42 hours in 1950 to 40 hours in the 2000's (McGrattan and Rogerson, 2004)

cline in employment rates among young men. More recent data, however, suggests that screen time among the young is now reaching record levels. A 2019 study of German adolescents finds that 18 to 25 year olds spend 23.4 hours on the internet for communication, shopping, research, and entertainment purposes (BzGA (2020)). The study does not differentiate between work and leisure purposes. If most of this time was spent on entertainment, these levels of usage would generate sizable labor market effects and would lead to meaningful changes in employment patterns if it becomes widespread in the future.

6 Conclusion

Economists have recently taken an interest in the possibility that entertainment technology may affect work behavior, a hypothesis explored in the context of contemporary video games by Aguiar et al. (2021). All else equal, one would expect an increase in the utility derived from leisure time through superior entertainment to reduce labor supply, particularly for workers already on the margins of labor force participation to begin with. This paper tests that prediction with the rollout of television, the single most consequential improvement in entertainment technology in the twentieth century. Television brought large, lasting, and salient changes to daily life. Our hypothesis in this paper is that these changes could have affected not only the allocation of leisure time but also the labor-leisure tradeoff.

We find that TV led to statistically and economically significant declines in employment during the 1940's and 1950's regulated rollout of broadcasts. Two additional results lend confidence to our main findings. First, the effects of TV are largest for retirement-age workers; we see no evidence that TV led younger workers to quit their jobs, but the availability of TV did increase retirement rates among older workers. This is consistent with the change in the nature of retirement documented in Costa (1998), whereby leaving one's career began to happen not only by necessity but also for the enjoyment of "golden years" of leisure, and with the fact that today, according to data from Aguiar and Hurst (2006), people in the U.S. aged over 65 spend on average four hours a day watching TV. Second, we are able to exploit a sharp freeze in broadcast licensing to run a series of placebo tests that help rule out spurious associations between TV access and employment patterns. We show that "ghost stations" whose applications for broadcasts were just denied by the FCC have no effects on work, suggesting that it was indeed TV broadcasts themselves, rather than correlated or confounding trends in economic conditions, that led to the increase in retirement.

While research and discussion of trends in labor force participation continues to focus on labor demand topics like trade and technology, we offer novel evidence on the role of an under-explored supply-side question of technical change–how entertaining is time spent at home? TV improved the outside option for people on the margins of the labor force as it rolled out in the 1940's and 1950's. Given that our findings are from this historical period, one might also ask how relevant they are to understanding labor markets today. The proliferation of ever more compelling TV and of broader entertainment opportunities more generally speaks to the likely persistence and importance of these effects. Entertainment technology is of course far from the only or primary consideration, but it is one of many forces that operate in modern labor markets.

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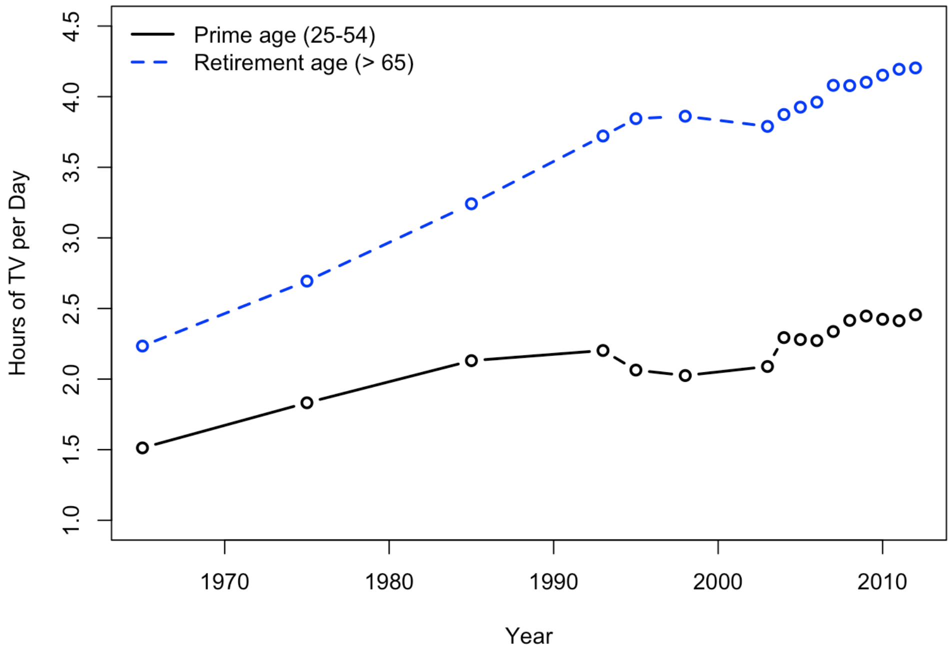
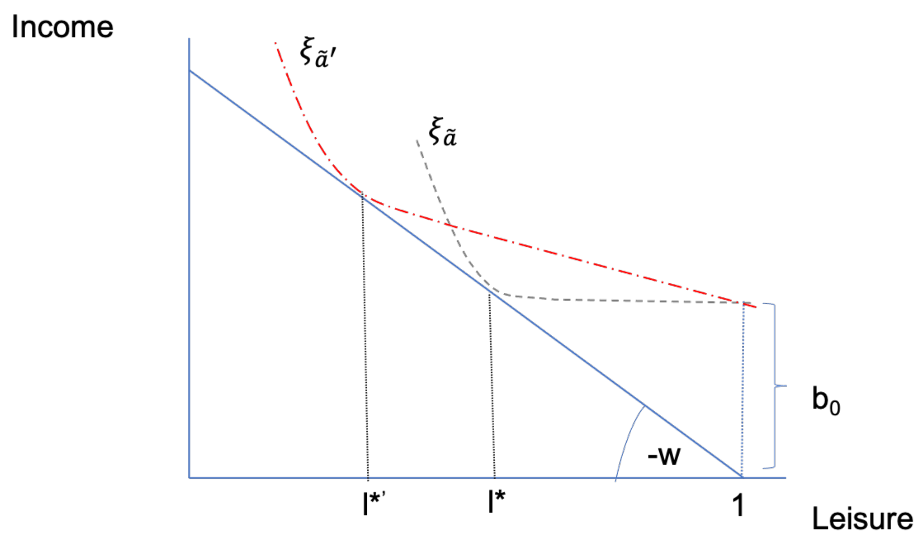
7 Figures and Tables

Figure 1: Hours of Television Watching per Day



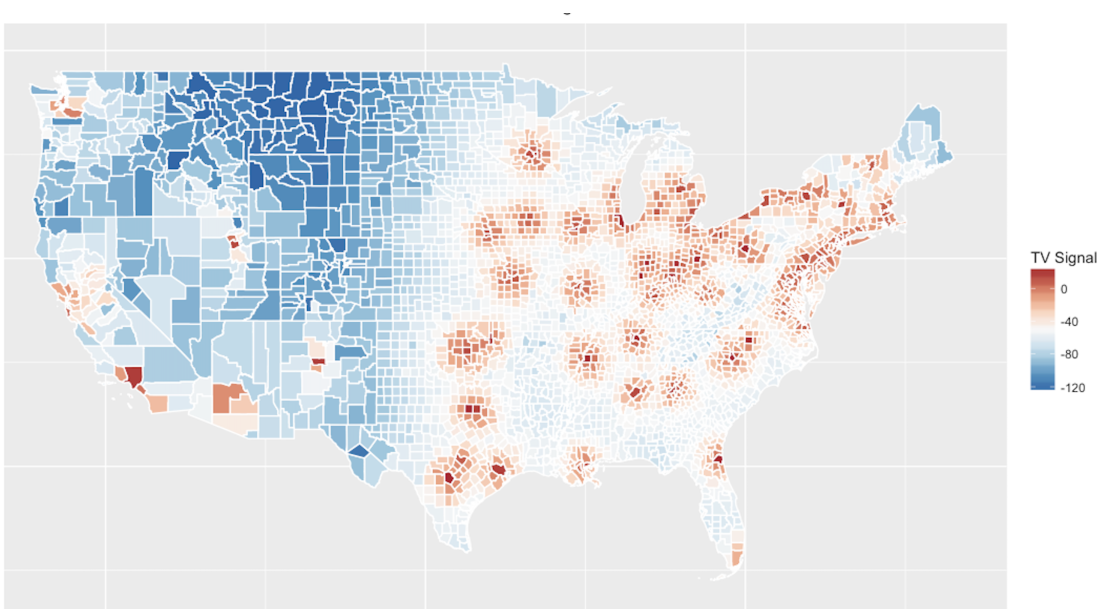
Notes: The figure shows the amount of time American's spend watching television as primary activity. Data are from the Historic American Time Use Study (AHTUS). The hours refer to "primary activity."

Figure 2: Marginal Retiree



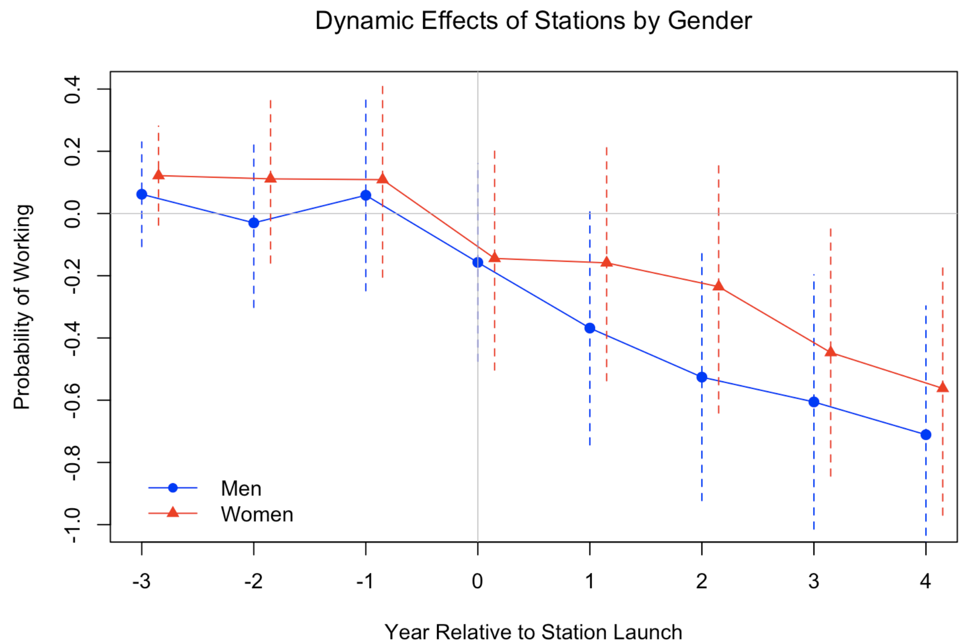
Notes: The figure shows the indifference curve of the marginal retiree, a person who is just indifferent between working and not. The age of the marginal retiree is indicated by $\tilde{a}$. The dashed line is a case with low β_0 and the dash-dot line is a case with higher β_0.

Figure 3: ITM-Measured Signal Strength in 1950



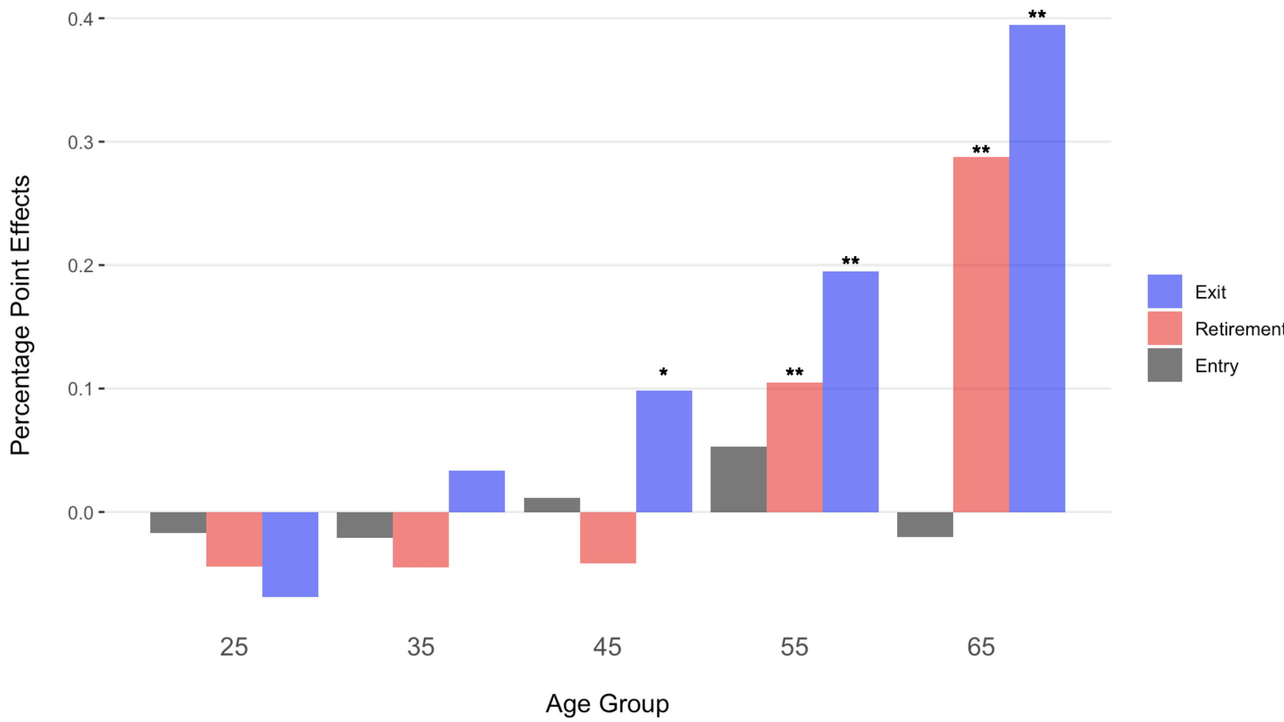
Notes: The figure shows the signal level, in decibels, of the strongest station in each county in 1950, as computed with the ITM. Broadly, counties shaded red had TV access, while counties shaded blue did not; signals whose strength was less than -50 decibels, where the map turns from red to blue, were effectively unwatchable. Not shown in this visualization of the data is the *number* of stations available locally.

Figure 4: Dynamic Effects of Station Launches



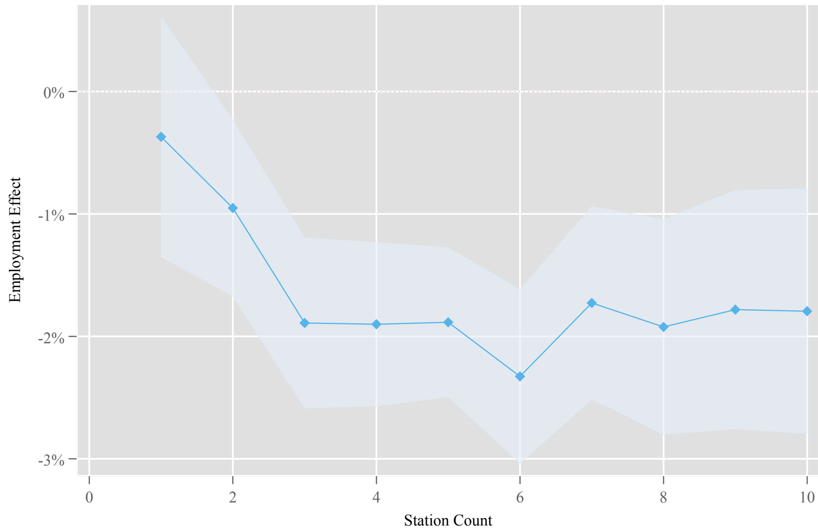
Notes: The figure shows dynamic effects of the launch of a TV station, separately for men and women. Specifically, these are estimated coefficients from individual level regression of equation 4 and 95 percent confidence intervals. See text for further details.

Figure 5: Effects of TV on Entry, Exit, and Retirement



Notes: The figure shows the impact of television on job transitions. Effects on employment entry are shown in black, on exits in blue and on retirement in red. Retirement is defined as a permanent exit from employment (proxied by the absence of a work observation until the end of our data). The plotted results are coefficients from difference-in-difference regressions of the respective labor market transitions on television exposure, allowing for separate coefficients by age group. For additional specification details see Table 1.

Figure 6: Steady State Effect of TV Accounting for Additional Stations



Notes: The steady state employment effects are the product of the average effect of stations (coefficient from a DiD regression) and the number of stations in treated labor markets. We estimate the average effects in separate regressions for each station count, restricting the sample to local television station launches up to that count. A stable steady-state effect implies that an added station has little additional effect. The shaded are 90% confidence bands.

Table 1: Individual-level Effects of TV on Employment

	(1)	(2)	(3)	(4)	(5)
$\mathbb{1}$(Men) × Stations	-0.574***	-0.585***	-0.600***	-0.315**	-0.681***
	(0.131)	(0.132)	(0.131)	(0.140)	(0.187)
$\mathbb{1}$(Women) × Stations	-0.246**	-0.246**	-0.261**	0.0281	-0.222*
	(0.112)	(0.110)	(0.111)	(0.122)	(0.123)
Observations	325,130	325,130	325,130	325,130	325,130
R-squared	0.678	0.679	0.680	0.854	0.679
Year × Gender FE	Yes	Yes	Yes	Yes	Region × Year
Person FE	Yes	Yes	Yes	Yes	Yes
Age FE	No	Yes	Yes	Yes	Yes
Trends	No	No	Demographics	State	No
Mean DV Men	78.29	78.29	78.29	78.29	78.29
Mean DV Women	38.28	38.28	38.28	38.28	38.28

Notes: The table shows individual level regressions of an employment dummy with value 100 for an employed worker on the number of TV stations available in the local area. Data are at the individual level and covers individuals over the age of 21 and spans 1937-1960, at annual frequency from 1951 onward and multi-year averages for earlier periods (see text for details). All regressions include gender-specific year fixed effects. Demographic trends allow for different time trends for high-school graduates, race (white, black, other), marital status and 5 year age bins. Regions are census regions. Television is measured at the MSA level. Standard errors are clustered at the same level and span 134 clusters. Source: SSA-CPS employment records and Television Factbooks $***p < 0.01, **p < 0.05, *p < 0.1$

Table 2: Effects of TV on Employment Using Variation from Regulator Shutdown

	(1)	(2)	(3)	(4)	(5)	(6)
			Interruption Experiment			
	Placebo Test		Broad Control Group		Narrow Control Group	
Stations	-0.321***	-0.335***	-0.386***	-0.391***	-0.447***	-0.419***
	(0.0978)	(0.0979)	(0.0987)	(0.0993)	(0.112)	(0.112)
Blocked stations	0.120*	0.107				
	(0.0703)	(0.0707)				
Observations	317,016	317,016	257,856	257,856	99,644	99,644
R-squared	0.680	0.681	0.680	0.680	0.775	0.775
Demographic Trends	No	Yes	No	Yes	No	Yes

Notes: The table shows the impact of television on employment rates, using variation from the regulator shut-down. Columns 1 and 2 compare the effect of TV stations and stations that were blocked during the regulator shutdown 1948-1952. Columns 3 through 6 focus on variation from the rollout interruption. Column 3 and 4 use a "broad control group" of untreated locations, and focuses on places with TV station launches near the interruption start and end date (1947-1954). Columns 5 and 6 use a narrower definition and only uses places with imminent launches at the start of the interruption as control group, additionally it restricts the sample years to the period when TV variation was due to the interruption (years of the interruption and the following unwind, 1947-1954). The estimates use the baseline specification in column 3 of Table 1. See Table notes for additional details $***p < 0.01, **p < 0.05, *p < 0.1$

Table 3: MSA-Level Effects of TV on Employment and Hours in Manufacturing

	(1)	(2)	(3)	(4)
	Panel A: CES Log Manufacturing Employment			
Stations	-0.0108	-0.0122	-0.0118	-0.0111
	(0.00852)	(0.00838)	(0.00854)	(0.0111)
Observations	446	446	446	446
R-squared	0.994	0.994	0.997	0.994
	Panel B: CES Log Total Manufacturing Hours			
Stations	-0.0115	-0.0133	-0.0131	-0.0101
	(0.00841)	(0.00832)	(0.00871)	(0.0110)
Observations	446	446	446	446
R-squared	0.993	0.993	0.997	0.994
Area Effects	Yes	Yes	Yes	Yes
Trends	Yes	Cubic	State	No
Year Effects	No	No	No	Yes

Notes: The table shows regressions of labor market outcomes on the number of TV stations available. Data are at the MSA level. Specifically, the outcomes are log employment and log total hours from the CES manufacturing data, respectively. Robust standard errors in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$

8 Appendix B: Measuring TV Access

8.1 Measurement Error in the DMA Data

Gentzkow (2006) approximates 1950's broadcast ranges with Nielsen media markets, or Designated Market Areas (DMAs), that are based on 2003 viewership. A DMA is a group of counties around a metropolitan area. The approximation takes the year in which the first station in a DMA began operation and assumes that each county in that DMA received a signal in that year. We found that 1960's coverage maps show differences between historical broadcast ranges and the 2003 DMAs. The DMA approximation sometimes underestimates and sometimes overestimates how far signals reached. The next two subsections give examples of each case. These are not representative, as we chose them specifically for exposition of the two types of problems with the DMA approximation.

8.1.1 An Example of DMA Underestimation (A type II error)

Proximal cities confound the DMA approximation of TV access. For example, panel (A) of figure 11 shows a coverage map of Kansas City from the 1967 *TV Factbook*. The blue line is the broadcast ring as defined by those counties that have over 50 percent coverage according to the map. Panel (B) overlays in red the Kansas City DMA. The DMA is too small–it excludes counties to the northwest that were likely covered. Moreover, for a region with little variation in terrain, the irregular shape of the DMA suggests that it cannot reflect the roughly circular true broadcast range.[31]

Let TVYEAR_i denote the year in which county i first had TV access. In panel (B), the DMA approximation assigns the highlighted counties between the two rings a TVYEAR of 1954. However, those counties fall well within the range of the Kansas City tower, and that tower started

[31] For two reasons, the *Factbook* maps ought to be taken only as suggestive regarding true 1950's signal reach. The first is that these maps were not published until the 1960's, and tower technology–power, height, etc.–improved substantially over time. The second is that the shading in the maps reflects surveys of viewership, not measures of signal strength. County coverage exceeding 50 percent for a station means that over 50 percent of households in the county watched that channel. Our measurement of signal reach will not rely on these maps.

broadcasting in 1950. Therefore the true TVYEAR of the highlighted counties is likely 1950, not 1954. This misclassification owes to the nearby DMAs, Topeka and St. Joseph, whose broadcasts began in 1954. While it is true *today* that the highlighted counties are closest to the Topeka and St. Joseph signals, and are therefore not in the 2003 Kansas City DMA, those counties are close enough to Kansas City to have viewed Kansas City broadcasts in 1950.

The TV ownership data from Gentzkow and Shapiro (2008) confirm that this is a case in which today's DMAs do not align with 1950's signals. The DMA data assign the highlighted counties in panel (B) as not receiving a TV signal until 1954, four years after the counties in the red Kansas City ring. If that were true, we ought to observe the highlighted counties buying TVs well after the Kansas City counties. Panel (A) of figure shows that in fact the timing of TV purchases is almost identical across the two groups, consistent with the hypothesis that Topeka and St. Joseph viewers received a 1950 signal from Kansas City. Substantial TV ownership in a county before that county's DMA-approximated TVYEAR is evidence of measurement error arising from signal overlap.

When signals overlap like this, DMAs underestimate coverage. The overlap between Kansas City and Topeka, for example, leads the DMA data to underestimate how many counties the Kansas City broadcast reached in the 1950's. Spot-checking coverage maps suggests that DMAs can also overestimate coverage.

8.1.2 An Example of DMA Overestimation (A type I error)

Today's DMAs sometimes extend further from city centers than historical signals did. Panel (C) of figure 11 shows a *Factbook* coverage map of Minneapolis-St. Paul. The blue line rings counties whose coverage exceeded 50 percent. Panel (D) adds the Minneapolis-St. Paul DMA in red. That DMA is too large, in that it includes the highlighted counties that were likely out of reach of the broadcast, which leads to overestimation of coverage. The highlighted counties have a DMA TVYEAR of 1948, since that is when the first Minneapolis station began operation. But many of those counties appear to be too far away from the tower to receive the early Minneapolis signals. Panel (B) of figure shows that TV purchases in the highlighted counties–the group inside the DMA but outside the mapped broadcast range–lagged purchases in the counties inside the *Factbook* coverage area, consistent with the hypothesis that the DMA overestimates 1950's signal reach. That pattern remains after controlling for county characteristics like income and population that are associated with TV ownership.

8.1.3 Causes and Prevalence of Measurement Error

This section moves beyond examples to the causes of measurement error and evidence on the prevalence of those causes. To start with underestimation, the two conditions under which the signal overlap problem arises are: Neighboring DMA towers (1) are close enough for signals to overlap and (2) started broadcasts in different years[32]. The closer the towers and the further apart the initial broadcast years, the larger the potential measurement error. To find possible areas of overlap, we ranked pairs of DMAs by their distance apart. There are 166 unique pairs of DMAs whose towers are less than 100 miles apart (a typical broadcast radius) with broadcasts beginning in different years. Among them are the Kansas City, Topeka, and St. Joseph pairs. Other metropolitan areas such as Pittsburgh and Cleveland are close enough to smaller neighboring stations like Youngstown to create the same overlap issue.[33]

Overestimation, by contrast, can arise because of improvements in TV towers over time. In most cities, the 1950's saw expanded broadcast ranges through both upgrades to existing stations and also construction of new towers. The 2003 DMAs are therefore prone to overstate early 1950's signal reach, when towers were weaker. As shown in figure 13, the average height above ground of a commercial tower in 1948 was 483 feet, and already by 1960 that had increased to 629 feet. Some stations moved to higher ground, and tower height above average surrounding terrain rose from 721 to 992 feet. Average visual power jumped from 19 to 170 kilowatts over that period, and

[32]Condition (2) is necessary because if two towers were close but started broadcasts in the same year, then all surrounding counties would get a signal in the same year, so proximity alone would not lead to misclassification. Terrain also matters–mountains could prevent overlap–and our measurement of TV access will account for variation in elevation.

[33]Table 8 lists the first 40 pairs and shows the distance between towers. Note also that in 1948 the FCC froze applications for new broadcast licenses in part because it realized it had allowed stations to be too close together.

average aurul power increased from 11 to 87 kilowatts.[34] The fixed DMAs do not capture shifts in broadcast areas that followed changes in tower technology.

These measurement issues tend to affect particular types of counties. The DMA approximation always gets major cities right. Underestimation and overestimation occur at the fringe of the broadcast areas of those cities, as the figure 11 examples show with Kansas City and Minneapolis-St. Paul, and the fringe plays a key role in estimating TV's effects. Gentzkow (2006) exploits broadcast rings to identify the causal effects of TV on voter turnout. The idea is that since TV reception reached about 100 miles from a broadcast tower, counties just inside and outside of that radius comprise treatment and control groups. Using this method, variation in access to TV is "driven by whether a county happened to fall within the roughly 100-mile radius of television broadcasts" (p. 945), so measuring that radius accurately is especially important for inference.

We took the evidence presented thus far as reason to pursue a more precise measure of TV access. Those measurements, constructed using digitized *TV Factbook* data and the Irregular Terrain Model (ITM) of signal propogation are discussed in section 3.1 of the main text. To validate the ITM measurements, we turn next to comparisons of key findings in the literature using the DMA approximation and ITM data.

8.2 TV Data Validation Exercise

As referenced in the introduction, much of our knowledge on the effects of TV relies on the DMA approximation. Among the many papers using the DMA approach are Baker and George (2010) on household debt, Campante and Hojman (2013) on political polarization, Thomas (2019) on smoking, Kim (2020) on consumer culture and spending, and Angelucci et al. (2020) on media competition and news consumption. The original DMA papers are Gentzkow (2006) and Gentzkow and Shapiro (2008) on how TV impacted voter turnout and children's test scores, respectively. Here we replicate the main results of these two papers using the ITM, and we find that the estimated

[34] Power does not map directly to broadcast reach, as higher frequency channels require more power to operate.

effects are about twice as large with the new data.[35]

Gentzkow (2006) studies how the 1950's TV rollout affected voter turnout. The direction of the effect is a priori ambiguous–it could be that TV broadened news viewership and therefore stimulated political engagement, or, alternatively, that TV crowded out news consumption with entertainment programming, which in turn dampened political knowledge and interest. Gentzkow finds robust evidence for the the latter case, using the following baseline difference-in-differences specification:

$$Y_{it} = \alpha_i + \delta_{rt} + \gamma TV_{it} + \beta X_{it} + \epsilon_{it} \tag{5}$$

Here the outcome Y_{it} is voter turnout in county i and year t, and controls include county effects α_i, region-year effects δ_{rt}, as well as flexible time trends interacted with county characteristics in X_{it}. The explanatory variable of interest TV_{it} is the number of years that county i has had TV access in year t, so the coefficient γ captures the effect of an additional year of TV access on voter turnout.

Row 1 of table 9 reports the main results from the paper. Columm 2, the fully-controlled, preferred specification shows that an additional year of TV availability led to 0.136 percentage point decline in voter turnout, an effect size that "explains half of the total off-year decline in turnout since the 1950's. The effect on presidential-year turnout is smaller—accounting for roughly a quarter of the total decline— and is not significantly different from zero" (p. 933). (Note that the effects in row 1 are much larger for the column 4 mid-term elections than the column 3 presidential elections.) Rows two and three show results using the ITM rather than the DMA's to measure TV access, with both a -40 and -50 decibel threshold for access. The effects are upwards of 2-3 times larger, which is consistent with a reduction in attenuation bias arising from measurement error.

We find similar results in the context of a study on TV and education. Gentzkow and Shapiro (2008) investigate how TV influenced children's test scores, providing a rigorous test of longstanding worries that TV could "rot children's brains" using data from the 1965 Coleman Report. This paper uses a two-stage least squares approach, instrumenting for TV ownership in a household

[35] We are grateful to Matthew Gentzkow for his correspondence and generous assistance with code and data.

with the availability of a TV signal; the idea is that TV ownership and viewership may well have been endogenous choices, but that conditional on a set of controls, access to a TV signal was idiosyncratic. The central results are based on the following first- and second-stage regressions:

$$y_{gc} = \beta TV_{gc} + \phi_g W_c + \delta_c + \gamma_g + \epsilon_{gc} \tag{6}$$

$$TV_{gc} = \beta_g^0 ADOPT_c + \phi_g^0 W_c + \delta_c^0 + \gamma_g^0 + \epsilon_{gc}^0 \tag{7}$$

The main outcome y_{gc} in equation 6 is average test scores for students in grade g and location c, which is regressed on the number of years of potential preschool television exposure for those students, TV_{gc}, and additional controls. Gentzkow and Shapiro (2008) instrument for TV_{gc} in equation 7 with a variable $ADOPT_c$ for the time at which location c adopted TV broadcasts, as measured using the DMA approximation.

Table 10 reports the main findings from the paper, as well as the first-stage F-statistic from equation 7. Contrary to common narratives about the harmful influence of TV, the row 1 results show that, if anything, TV exposure during childhood *increased* test scores. Many of the effects are imprecise, but they are positive, and for reading scores, the coefficient is statistically significant, "consistent with a variety of existing evidence suggesting that children can learn language-based skills from television" (p. 300). In rows 2 and 3, we estimate the same two-stage least squares specification using the ITM to measure TV access. Note first that first-stage F-statistic is larger, meaning there is a stronger association between TV signal availability and TV ownership using the ITM. We take this as validation that the ITM is more accurately measuring signal reach than the DMA's. The effects on test scores in columns 2-5 are larger and more precise as well, with the exception of general knowledge scores.

Taken together, these replication exercises suggest that future researchers studying the effects of TV should use the ITM measurements of access. The DMA approach appears to produce substantial underestimates of TV's influence. We aim to make the ITM data available for both further revisions of existing results and future original work.

9 Appendix B: Empirical Appendix

9.1 Social Security Sample

The Social Security Act of 1935 introduced Federal Old Age Insurance in the United States. Individuals over the age of 65 received benefits, and payments were based on contributions people made across their work histories. To keep track of individual contributions, the Social Security Administration (SSA) started recording individual earnings data in 1937. Initially this covered all wage and salary workers (excluding railroad workers) under age 65 who were employed in the private sector in the U.S. and Alaska and Hawaii, which were then territories (Long, 1988). From the outset, the system thus covered a substantial share of the U.S. workforce; in 1937 it was estimated that around 32 million workers, or roughly 60% of the labor force, were covered (Wasserman and Arnold, 1939). Workers not excluded from the system included certain non-covered occupations (e.g. the self-employed), workers aged 65-74, and the unemployed or workers in unemployment relief programs. Coverage was expanded over the following decades, with major expansions in 1951, 1954 and 1956. The expansions broadly affected workers in four categories: government employees, the self-employed, military personal, and agricultural workers. To work with a consistent sample, we drop occupations that first receive coverage during this period.[36] Since the data only report occupation and industry in 1977, we also exclude individuals that first appear in the earnings records in one of the three extension years in the 1950's and are older than 30.[37]

At the beginning of the sample, the Social Security system excluded the following groups: "agricultural employment, work for Federal, State and local governments, employment by certain non profit organizations or institutions, railroad employment, domestic service in private homes,

[36] This excludes 3,714 individuals. We exclude workers in occupation groups: 42, 43, 44, 36, 10, 11, 7; in occupations: 821, 822, 980, 981, 982, 983, 984, 824; in major industry group: 11; industry group: 48, 49, 50, 51; industries: 927, 937, 769; and workers in areas with a farming to population ratio over 10%. Additionally, we exclude veterans who appear in the data in 1957.

[37] This drops an additional 1,996 individuals.

and all types of self employment." Moreover, workers over the age of 65 did not contribute to Social Security in 1937 and 1938 and their employment was not recorded (Social Security Bulletin, Vol. 70, No. 3, 2010), so we set employment to missing for these cases. In 1951 the self-employed (except members of professional groups), farm laborers and domestic workers were included in the system. Additionally, worker in nonprofit organization could join the system if they received at least $100 in pay during the calendar year. Reforms broadened coverage further in 1955. These reforms relaxed restrictions on farm workers, the self-employed and expanded the scope for voluntary participation of state and local government employees. Farm laborers were included if they passed a "cash-pay" or "regularity-of-employment" test. This required a cash income over $150 from a single employer, or employment on a time basis of at least 20 days with a single employer. Finally, in 1956 soldiers on active duty, previously excluded self-employed professions and optionally police and firefighters in state and local retirement systems became covered. To avoid individuals dropping in and out of employment due to changes in the earning threshold, we code all workers as employed if they earn over $50 and non-employed if earnings are below $50.

9.2 Summary Statistics

Our baseline sample comprises of 325,130 person-year observation, 31,653 individuals and spans 134 local areas. As described above, these areas split the mainland U.S. into MSAs and rest of state areas. We present summary statistics of our sample in Table 4. A few observations are worth highlighting. First, the SSA employment measures are not directly comparable with variables from the Census. The previous section describes how the SSA defined employment and we use this definition. Also note that using SSA employment definitions has become a common practice in a sizable literature that analysis the U.S. labor market with administrative records. The picture is broadly consistent with Census data and we discuss employment trends more below. Second, it is worth exploring the representativnes of the sample. While a representative sample is not necessary for the validity of the analysis, understanding the sample helps understand the summary statistics. Our sample is based on the 1978 CPS and thus becomes less representative of the U.S. population as we go further back in time. In particular, groups with higher mortality or migration rates are underrepresented. As a result, the sample includes somewhat fewer men (41% instead of 49%) and minority workers (9% instead of 10%) and is younger (38 instead of 44) than the U.S. population of the time. All in all, the sample is reasonably close to the aggregate U.S. population. A major strength of the experiment is that it touches broad range of society and we can measure heterogeneous effects by sub-groups and strengthen the external validity of our results. For instance, the effect of television may look differently in a population with a different demographic make-up. Below we explore this formally and re-weight our sample to obtain the average treatment effect for the U.S. society.

Fianlly, we provide additional detail on the variation from the television rollout. Figure 9 shows the time series aspect of the rollout. At the start of the license freeze in 1950 substantial differences existed across the U.S.. Multiple stations were already available in a few early adopting locations but most Americans had only limited exposure to television. This changes with the lift of the license freeze in 1952. In the following two years television spread throughout the country. The figure illustrates that much of the variation in the television rollout over time is down to the license freeze "accident," which helps our identification strategy. And we can explo

9.2.1 Retirement Trends

Retirement rates grew sharply in the 1950's. Figure 7 shows that the retirement rates for over 65 year olds almost doubled from around 30% to nearly 60%. Our measure of retirement differs somewhat from Census definitions of labor market activity. We define retirement as a permanent with-drawl from the labor force, as measured by Social Security contributions. Census measures typically focus on employment in one specific reference week. These definitions make a difference to the level but not the trend in inactivity, both series show a sharp decline in labor market activity among the over 65 year olds during the 1950's. A second striking feature of Figure 7 is the rise in retirement among "younger" cohorts. Retirement is less common among people aged between 50 and 65 but the trend in the 1950's clearly points upwards too. Retirement rates among these "younger" workers almost doubled in the 1950's. This trend is particularly remarkable because these age groups are typically not eligible for Social Security, which suggests that other factors beyond social insurance played a role in growing retirement trends.

9.2.2 Employment over the Life-Cycle

Employment rates evolve over the life-cycle. This pattern during the 1950's is familiar from Census data and we show the results in the CPS-SSA data. The employment to population rate for men follows a U-shaped patterns. The employment rate rises until age 30, then plateaus and starts declining from age 50. For women, employment rates start at a lower level and decline during the child bearing years, then recover somewhat in the late 30s until they start declining later in life. These patterns are well known and are broadly consistent with the Census data reported in McGrattan and Rogerson (2004). This shores up our confidence that the SSA data paints a reasonable picture of labor market activity.

9.3 Robustness Checks

9.3.1 Leads and Lags

A popular method to check for pre-trends is to include leads and lags of the treatment in the event study designs and analyze changes in labor supply in the lead up to an event. The intiuition is that effects should arise after television launch events and not before. We implement this through a dynamic DiD which replicates DiD 3 and additionally allows for leads and lags of the treatment:

$$E_{a,i,t} = \gamma_t + \delta_i + \sum_{j=-4}^{3} \beta_{t+j} \cdot TV_{a,t+j} + \pi \cdot X_{a,i,t} + \epsilon_{a,i,t},$$

these leads and lags capture the evolution of the treatment effect in the 4 years before and after the launch of a new TV channel. Conventional event studies have to omit one lead or lag regressor, because these regressors are otherwise co-linear with the year dummies. In our case, we have one more degree of freedom because the television treatment varies in intensity. More than one television station is launched in some years, which breaks the perfect co-linearity of leads and lags and time FE. We could therefore estimate coefficients for all lead and lag periods but since we are mainly interested in trends, we follow the standard event-study design and normalise the effects in period t-1 to zero.[38] This eases the interpretation of the results, as coefficients then capture the deviation in employment relative to the t-1 period. Table 5 shows that treatment and control regions evolve in parallel in the years leading up to the launch of a TV channel. And we see a sharp change after the launch of a TV station. The clear change at the time of treatment indicates that the difference-in-difference specification is capturing the effects of TV and we can rule out that differences in trends are driving our results. The following columns control for alternative aggregate and regional trends and find similar results.

9.3.2 LATE vs ATE: sample weights

Our SSA-CPS data follows the 1978 CPS cohort throughout their life. The sample is representative of the 1978 population but becomes less representative as we go back in time. The lack of representativeness does not cause problems for the internal validity of the results, but it does limit the external validity. Specifically, the measured LATE in our sample may not be representative of the ATE in the population. We can recover the ATE on the population of interest by re-weighting our sample. To do this we obtain data from the U.S. population Census on the target population. We linearly interpolate values in between the 1950 and 1960 Census and then construct weights to match those population totals. Specifically, we target population aggregates in an MSA, as well as their education and age demographics.

Table 6 shows the baseline results with the weighted sample. The main takeway is that the results are broadly similar to those reported in our baseline results (see Table 1). If we use weights for the steady-state estimates, we again find consistent results (see Figure 10). The impact of television increases with the first few stations and then steadies out. The point estimate is a 3%

[38] The effect in t-1 is typically positive around 0.1, reflecting that places with multiple simultaneous launches have higher rates of employment. For display purposes, we purge the impact of this level effect and subtract this value from all coefficients.

decline in the employment to population ratio. This is somewhat larger than the baseline 2% estimate but this difference is not significant. The weighted and unweighted results thus show qualitatively and quantitatively similar results.

9.3.3 Effect Heterogeneity

We here analyze heterogeneity in the response across demographic groups. The first column allows for different effects among more mobile individuals. Mobile individuals are more likely to leave the fixed MSA that we assign them to during the analysis and by testing treatment effects on this sub-group, we can assess how much such moves may attenuate the results. We define a dummy for high vs low mobility individuals and look at differences in the effects. We do not have data on moves in the 1950's and instead use the CPS migration supplement to infer moving propensity. We classify people as mobile if the moved out of MSA between 1975 and 1976 and test how much mobility attenuates results. The difference in effects is insignificant and quantitatively small (Table 7, column 1). This suggests that the attenuation bias from mobility is relatively minor.

The next columns show heterogeneity cuts for other demographic groups. Column 2 looks at age differences and again highlights that the effect is much bigger among workers near retirement. Column 3 and 4 look at effects by schooling and marital status. The effect on both groups is similar to the baseline estimates.

9.3.4 Migration and Intention to Treat

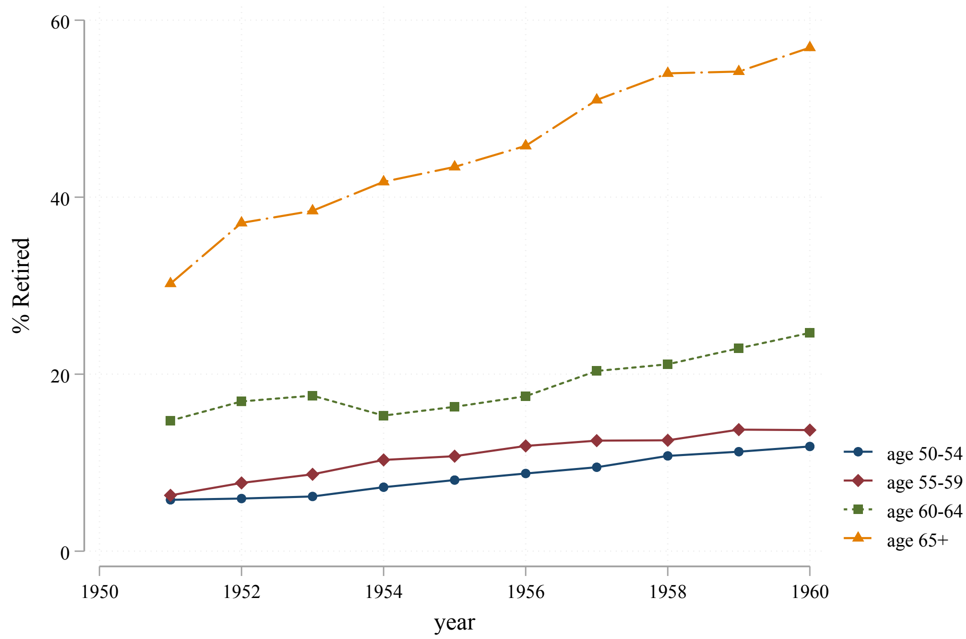
The baseline estimates treat place of residence as fixed and estimate intent-to-treat (ITT) effects. This appendix explores how these ITT effect relate to the local average treatment effect. Generally, migration could have two potential effects on the results. First, endogenous moves towards television could lead to selection effects, second random moves will lead to mis-measurement of television exposure. The first issue, selection effects, are taken care of by the individual fixed effects in our analysis. The focus of this section is instead on the second problem, which we call the imperfect compliance challenge, in the spirit of ITT effects. The standard approach in the literature is to divide the ITT estimates by the rate of compliance. In our setting, the denominator would be the fraction of people who migrate outside the treatment area. We additionally require information on the treatment effect in the non-complier population. In a set-up with a binary treatment non-compliers don't access the treatment and have a zero treatment effect. However, with multiple treatment dosages, non-compliers may still experience some treatment effects. The relation of the ITT and ATT can be expressed as: $ITT = ATT \times \sigma + NCTE \times (1 - \sigma)$. Where σ is the compliance rate, or the share of people who lived in a different MSA than we observe, and $NCTE$ is the treatment effect experienced by these non-compliers. Note that with a binary treatment $NCTE = 0$ and the ATT becomes the familiar IV estimate that scales the ITT up by the compliance rate: $ATT = ITT/\sigma$.

We first calculate the approximate level of non-compliance in our sample (σ). This requires data on migration patterns. The CPS-SSA linked data only includes imperfect information on these rates and we use the matched 1978 CPS migration supplement to estimate migration rates. Many people move every year, but only a small fraction of these moves affects our results. In particular, only moves that cross MSA boundaries are relevant. According to the 1978 CPS migration supplement, 5% of our sample left an MSA during the three year window 1975-1978. This group are clearly non-compliers and we can use this group for a benchmark exercise with $\sigma = 0.95$. To calculate the ATT we also need an estimate of the $NCTE$ and Table 7 reports treatment effects for this non-complier group in column 1. Using $\sigma = 0.95$ and $NCTE = -0.301$ in the ATT formula yields an ATT of -0.397, very close to the ITT estimate of -0.392.

The previous estimate is likely a lower bound for the true ATT as it only takes migration between 1975 and 1978 into account. The share of people who left the MSA in the 18 year window from our sample period to the 1978 CPS is larger. If we assume stationary migration rates, we can extrapolate the 18 year rate as: $\sigma = 0.05 + \sum_{t=1}^{5} 0.05(1-p)^t$, where p is the rate of repeat migration. A high value of p implies that some people are intrinsically more mobile and move frequently. We use panel data from the NLSY79 to get a sense of these repeat migration rates and find rates around $p = 0.3$. This implies $\sigma = 0.15$ and together with our previous $NCTE$ estimate yields an ATT of -0.408, again similar to the baseline estimates. To push this to an extreme, assume next that people only move ones ($p = 0$). In this scenario the ATT=-0.431, and therefore still in the same ballpark as our baseline estimates. This is of course an unrealistic assumption but illustrates that the results are reasonably robust to alternative assumptions about migration patterns.

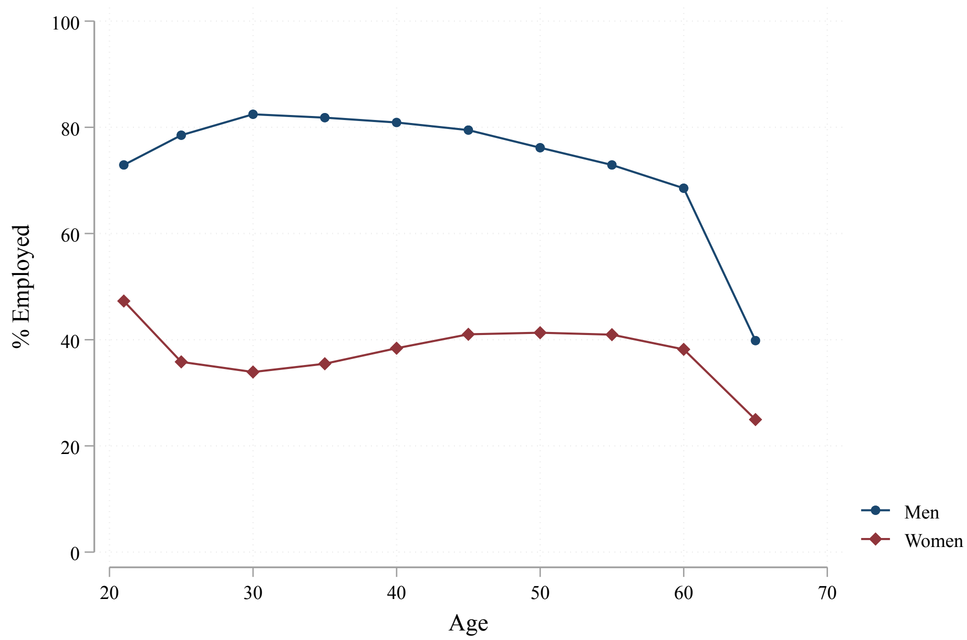
10 Appendix Figures and Tables

Figure 7: Retirement Rates



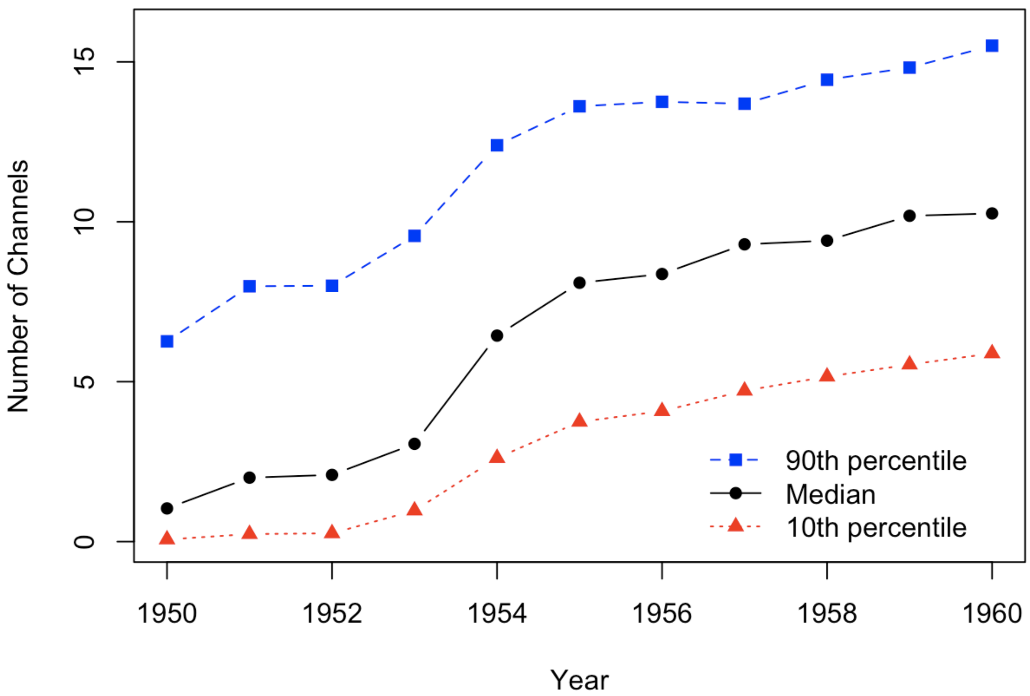
Notes: The figure shows retirement rates among older workers during the 1950's. Retirement is defined as no observed employment in the Social Security records until the end of our sample (1978). Source: linked SSA-CPS data.

Figure 8: Employment-to-Population Rates over the Life-Cycle



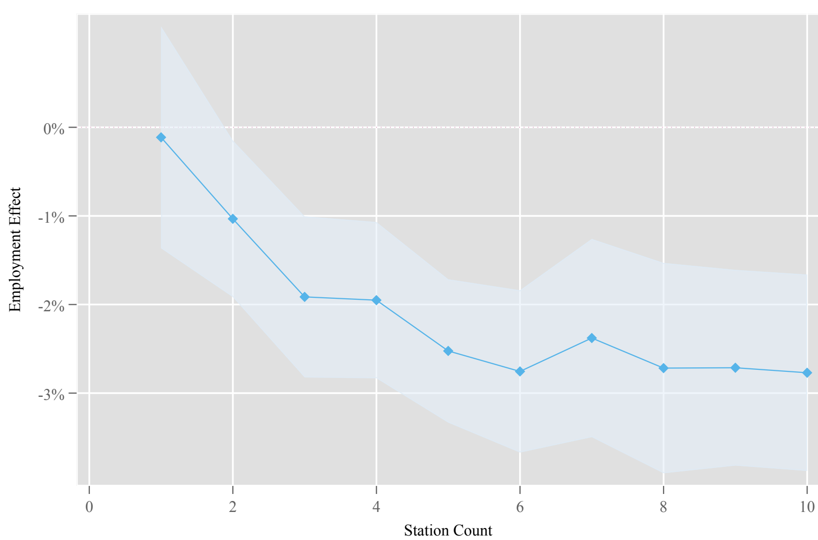
Notes: The figure shows employment rates by age and gender. Each dot shows the average for a five year age window, averaging employment rates over the full sample period. The first and last bins respectively show averages for the age groups 21-24 years and 65+. Source: linked SSA-CPS data.

Figure 9: Number of Stations Available Over Time



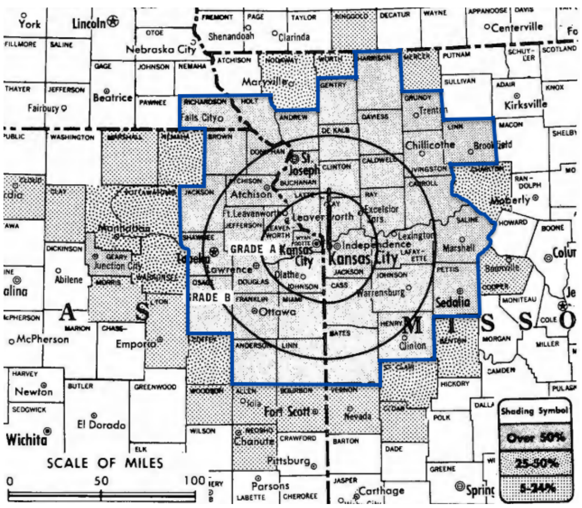
Notes: The figure shows the number of television stations in the U.S. between 1950 and 1960. It shows this for a median person, as well as at the 90th and 10th percentile of the distribution.

Figure 10: Steady State Effect of TV Accounting for Additional Stations - Weighted Sample

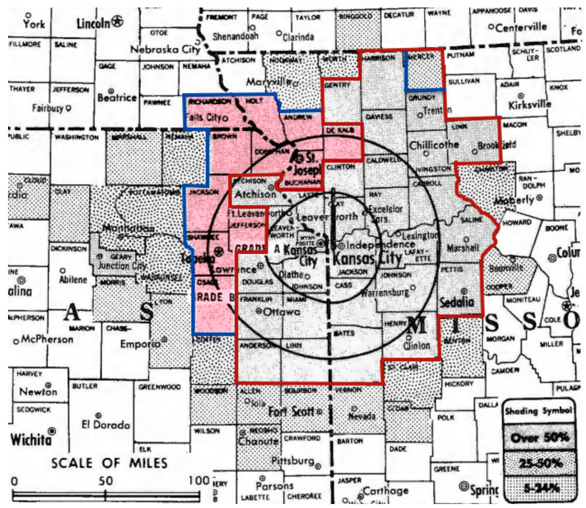


Notes: The figure replicates Figure 6 while using sample weights. Weights are constructed to make the sample representative of local population demographics at the annual level.

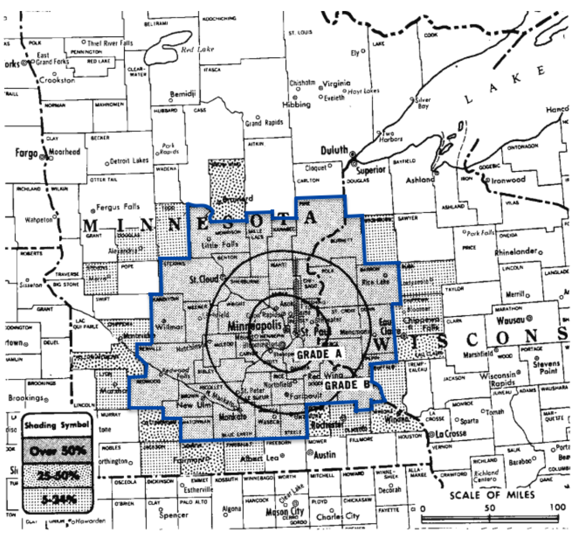
Figure 11: Coverage Maps and Designated Market Areas



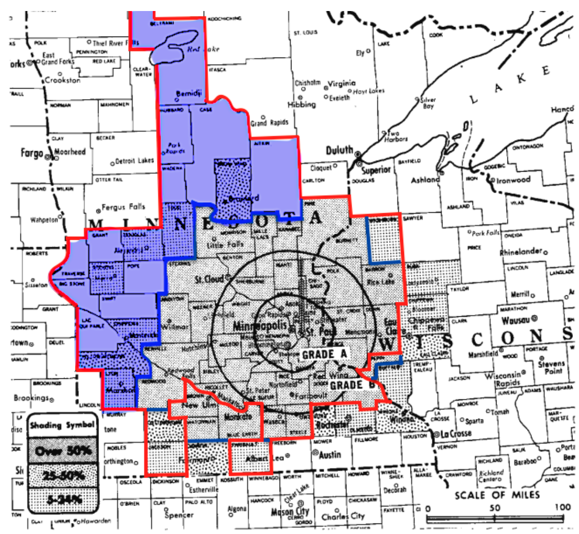
(A) Kansas City coverage map ring (in blue)



(B) Kansas City DMA ring (in red)

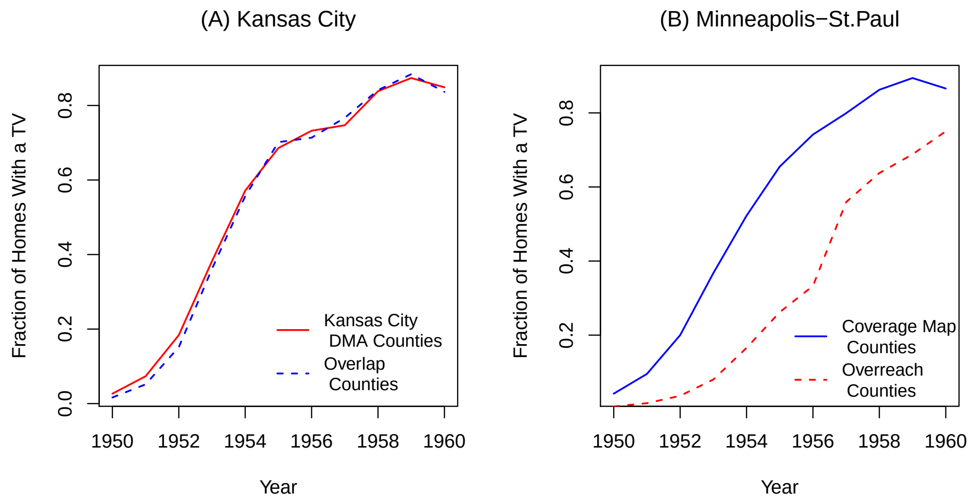


(C) Minneapolis coverage map ring (in blue)



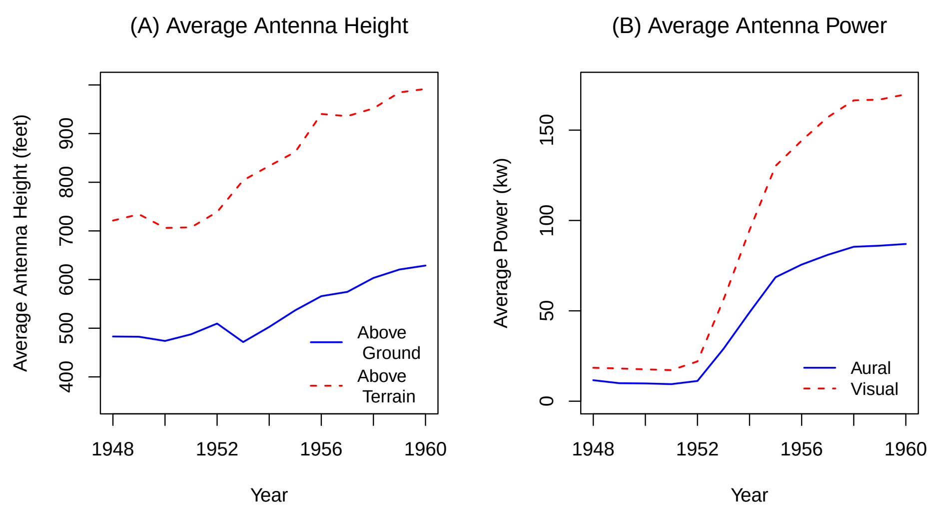
(D) Minneapolis DMA ring (in red)

Figure 12: TV Purchases Patterns



Notes: Panel (A) shows average TV ownership around Kansas City for counties in the groups indicated in the legend. "Overlap Counties" refers to those highlighted in Figure . In Panel (B), for Minneapolis-St. Paul, "Coverage Map Counties" refers to those ringed in Figure 4, whose coverage exceeds 50 percent according to *TV Factbook* coverage maps. "Overreach Counties" refers to those highlighted in Figure 5, which fall inside the Minneapolis-St. Paul DMA but outside the *TV Factbook* broadcast range.

Figure 13: Broadcast Technology Improvements



Notes: The figure shows the increases in broadcast tower height and power over time. Data are digitized from the *TV Factbook*, as discussed in the main text.

Table 4: Summary Statistics

	Observation	Average	s.d.	Min	Max	Men	Women
Employed	325,130	54.54	49.79	0	100	78.29	38.28
Quarters worked	325,042	1.909	1.877	0	4	2.886	1.239
TV channels	325,130	6.904	4.697	0	16.65	6.910	6.899
Years of schooling	325,130	11.80	3.419	1	19	11.69	11.87
High school graduate	325,130	0.541	0.498	0	1	0.508	0.563
Age	325,130	38.16	11.38	21	79	38.54	37.91
Ever married	325,130	0.950	0.217	0	1	0.947	0.953
Female	325,130	0.594	0.491	0	1	0	1
Minority	325,130	0.0883	0.284	0	1	0.0922	0.0855
Recent move	321,196	0.0521	0.222	0	1	0.0526	0.0518

Notes: The table reports summary statistics for the SSA-CPS sample. Employment and age information is based on SSA records and spans the years 1937-1960. The data is annual from 1951 to 1960 and includes multi-year averages for the periods 1937-1946 and 1947-1950. We restrict the sample to adults (over age 21 at the time). Data on gender, marriage, mobility, race and schooling is based on linked 1978 CPS records. Data on TV channels is computed using records from digitized Television Factbooks in an ITM signal propagation model.

Table 7: Heterogeneous Effects of TV on Employment by Demographic Groups

	(1)	(2)	(3)	(4)
Stations	-0.392***	-0.345***	-0.432***	-0.466***
	(0.0977)	(0.0966)	(0.101)	(0.144)
Stations $\times$ $\mathbb{1}$(Mobile person)	0.0878			
	(0.141)			
Stations $\times$ $\mathbb{1}$(Age 60+)		-0.584***		
		(0.133)		
Stations $\times$ $\mathbb{1}$(High school dropout)			0.0874*	
			(0.0500)	
Stations $\times$ $\mathbb{1}$(Married)				0.0849
				(0.121)
Observations	322,139	326,089	326,089	326,089
R-squared	0.680	0.680	0.680	0.680

Notes: The table shows regressions of employment on available TV stations with interactions for the listed demographic groups. The specification is the baseline specification in column 3 of Table 1. Mobile: person moved MSA between 1975 and 1976. Robust standard errors in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$

Table 5: TV Effects on Employment – Leads and Lags

	(1)	(2)	(3)	(4)	(5)
t-4	0.005	0.001	-0.002	0.221	-0.0005
	(0.122)	(0.123)	(0.125)	(0.155)	(0.141)
t-3	-0.1029	-0.109	-0.1076	-0.0964	-0.0961
	(0.119)	(0.119)	(0.120)	(0.145)	(0.132)
t-2	0.003	0.008	0.001	0.037	0.0636
	(0.0959)	(0.0966)	(0.0969)	(0.115)	(0.106)
t-1	0	0	0	0	0
t	-0.273	-0.274	-0.268	-0.256	-0.2384
	(0.105)	(0.104)	(0.105)	(0.103)	(0.112)
t+1	-0.247	-0.2399	-0.2382	-0.1769	-0.1767
	(0.107)	(0.107)	(0.107)	(0.117)	(0.125)
t+2	-0.256	-0.26	-0.259	-0.1916	-0.1563
	(0.116)	(0.116)	(0.117)	(0.118)	(0.118)
t+3	-0.265	-0.253	-0.246	-0.0573	-0.2694
	(0.129)	(0.128)	(0.128)	(0.168)	(0.135)
Observations	161,483	161,483	161,483	161,483	161,483
R-squared	0.782	0.782	0.782	0.902	0.782
cluster	134	134	134	134	134
Year $\times$ Sex FE	Yes	Yes	Yes	Yes	Region $\times$ Year
Person FE	Yes	Yes	Yes	Yes	Yes
Age FE	No	Yes	Yes	Yes	Yes
Trends	None	None	Demographics	State	None

Notes: The Table shows the timing of television effects by reporting coefficients on the leads and lags of the television variable. Period $t-1$ is normalised to 0 to illustrate changes in the effect around the time of television launches. See Table 1 for variable definitions and additional specification details. $***p < 0.01, **p < 0.05, *p < 0.1$

Table 6: Individual-level Effects of TV on Employment - Weighted Sample

	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(Men) × Stations	2.833***	-0.431***	-0.453***	-0.468***	-0.202	-0.369***
	(0.165)	(0.129)	(0.128)	(0.129)	(0.150)	(0.125)
$\mathbb{1}$(Women) × Stations	-0.724***	-0.260*	-0.254*	-0.257*	0.00238	-0.0576
	(0.102)	(0.145)	(0.143)	(0.153)	(0.134)	(0.146)
Sum of Weights (thsd.)	531,307	530,603	530,603	530,603	530,603	530,603
R-squared	0.112	0.705	0.707	0.708	0.873	0.707
Year × Sex FE	No	Yes	Yes	Yes	Yes	Region × Year
Person FE	No	Yes	Yes	Yes	Yes	Yes
Age FE	No	No	Yes	Yes	Yes	Yes
Trends	No	No	No	Demographics	State	No
Mean DV Men	78.31	78.29	78.29	78.29	78.29	78.29
Mean DV Women	38.34	38.28	38.28	38.28	38.28	38.28

Notes: The table replicates Table 1 and additionally uses sample weights. $***p < 0.01, **p < 0.05, *p < 0.1$

Figure 14: Revisiting TV's Effects on Voter Turnout (Gentzkow, 2006)

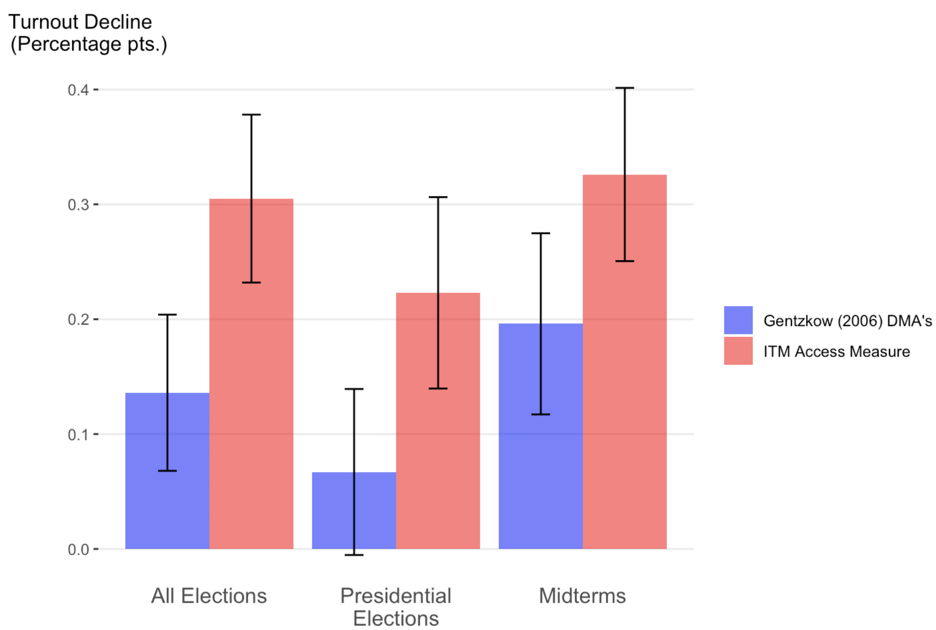


Table 8: Proximal Market Areas

DMA 1	DMA 2	Miles Apart	Years Apart
Pittsburgh (PA) [1949]	Steubenville (OH) [1954]	32.79	5
Washington (DC) [1946]	Harrisburg (PA) [1949]	35.86	3
Harrisonburg (VA) [1954]	Charlottesville (VA) [1960]	36.04	6
Harrisburg (PA) [1949]	Johnstown (PA) [1950]	42.47	1
Cleveland (OH) [1948]	Youngstown (OH) [1953]	42.53	5
Grand Rapids (MI) [1949]	Lansing (MI) [1950]	45.46	1
Binghamton (NY) [1950]	Elmira (NY) [1953]	45.67	3
Syracuse (NY) [1949]	Utica (NY) [1950]	46.36	1
Kansas City (MO) [1950]	St. Joseph (MO) [1954]	48.35	4
Cincinnati (OH) [1948]	Dayton (OH) [1949]	48.48	1
Lake Charles (LA) [1954]	Beaumont (TX) [1955]	49.55	1
Youngstown (OH) [1953]	Steubenville (OH) [1954]	50.28	1
Columbus (OH) [1949]	Zanesville (OH) [1953]	52.28	4
Binghamton (NY) [1950]	Wilkes Barre (PA) [1953]	52.39	3
Zanesville (OH) [1953]	Parkersburg (WV) [1954]	52.44	1
Cleveland (OH) [1948]	Steubenville (OH) [1954]	52.49	6
Detroit (MI) [1947]	Toledo (OH) [1948]	53.08	1
San Francisco (CA) [1949]	Sacremento (CA) [1954]	54.15	5
Baton Rouge (LA) [1953]	Lafayette (LA) [1955]	54.94	2
Pittsburgh (PA) [1949]	Youngstown (OH) [1953]	57.01	4
Hartford (CT) [1948]	Springfield (MA) [1953]	57.39	5
Nashville (TN) [1951]	Bowling Green (KY) [1960]	58.19	9
Grand Rapids (MI) [1949]	South Bend (IN) [1953]	58.36	4
Indianapolis (IN) [1949]	Lafayette (IN) [1953]	58.74	4
Lima (OH) [1953]	Ft. Wayne (IN) [1954]	58.86	1
Kansas City (MO) [1950]	Topeka (KS) [1954]	59.70	4
South Bend (IN) [1953]	Ft. Wayne (IN) [1954]	60.10	1
Birmingham (AL) [1949]	Montgomery (AL) [1953]	60.13	4
Memphis (TN) [1949]	Jonesboro (AR) [1960]	60.48	11
Jacksonville (FL) [1950]	Gainesville (FL) [1960]	61.83	10
Roanoke (VA) [1953]	Charlottesville (VA) [1960]	62.10	7
Denver (CO) [1952]	Colorado Springs (CO) [1953]	63.65	1
Rochester (MN) [1953]	La Crosse (WI) [1954]	63.69	1
Richmond (VA) [1948]	Norkfolk (VA) [1950]	63.88	2
Washington (DC) [1946]	Baltimore (MD) [1948]	63.95	2
Champaign (IL) [1953]	Terre Haute (IN) [1954]	64.67	1
Syracuse (NY) [1949]	Watertown (NY) [1955]	65.18	6

Notes: In brackets is the year in which a broadcast began in each DMA. Some DMAs are abbreviated for brevity. For example, the Birmingham (AL) - Anniston (AL) - Tuscaloosa (AL) DMA is listed just as Birmingham (AL).

Table 9: Revisiting TV's Effects on Voter Turnout (Gentzkow, 2006)

	All Elections	All Elections	Presidential	Non-presidential
DMA	-0.416	-0.136	-0.067	-0.196
	(0.0486)	(0.0412)	(0.0438)	(0.0478)
ITM_{40}	-0.468	-0.254	-0.171	-0.278
	(0.0450)	(0.0421)	(0.0481)	(0.0438)
ITM_{50}	-0.513	-0.305	-0.223	-0.326
	(0.0479)	(0.0443)	(0.0505)	(0.0457)
Full controls		X	X	X

Notes: The table replicates the Gentzkow (2006) results on TV's influence on voter turnout, with both the original DMA approximation and the new ITM data. ITM_{40} and ITM_{50} refer to measurements of TV access using -40 and -50 decibel cutoffs for access, respectively. Column 2 is the preferred specification in the paper, which shows effects on the order of 2-3 larger using the ITM. Column 3 shows results for the sub-sample of presidential election years, column 4 for off-presidential mid-term elections. See figure 14 for a plot of the DMA and ITM_{50} coefficients and 90 percent confidence intervals.

Table 10: Revisiting TV's Effects on Children's Test Scores (Gentzkow and Shapiro, 2008)

	First Stage F Stat.	Average Score	Verbal	Reading	General Knowledge
DMA	16.58	0.0225	0.0294	0.0557	0.0672
		(0.0279)	(0.0289)	(0.0302)	(0.0410)
ITM_{40}	36.69	0.0385	0.0511	0.0598	0.0384
		(0.0200)	(0.0214)	(0.0247)	(0.0310)
ITM_{50}	23.87	0.0374	0.0485	0.0604	0.0338
		(0.0231)	(0.0238)	(0.0276)	(0.0376)
Full controls		X	X	X	X

Notes: The table revisits the Gentzkow and Shapiro (2008) findings on how TV affected children's test scores. As before, ITM_{40} and ITM_{50} refer to measurements of TV access using -40 and -50 decibel cutoffs for access, respectively, while DMA refers to the DMA approximation to TV braodcast reach. These are two-stage least squares estimates, where TV ownership is instrumented with TV access; the first-stage F-statistic shows how strongly the reported measures of TV access predict TV ownership. See figure 15 for a plot of the DMA and ITM_{50} coefficients and 90 percent confidence intervals.

Figure 15: Revisiting TV's Effects on Children's Test Scores (Gentzkow and Shapiro, 2008)