Essays in Labor and Public Economics

by

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For James Fenton

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ABSTRACT

This dissertation consists of three chapters. If there is a throughline, it is that each chapter relates to the economics of partisan polarization or the media.

In the first, I show that a stylized fact on the decades-long stability in preferences for income redistribution–which has surprised public economists, given the spike in inequality–masks a divergence across parties. I then demonstrate that the widening divide owes to preferences sorting rather than strengthening. Next, I return to survey experiments on the elasticity of preferences to information treatments and, in contrast to previous literature, I find a simple treatment that induces substantial support for taxation and redistribution among conservatives. This treatment closes upwards of half the gap in preferences between liberals and conservatives, a key finding amidst worsening polarization.

The second chapter evaluates how the news media choices of one generation affect the formative environment and political preferences of the next. Focusing on the transition from broadcast to cable television, I argue that while the shift from a low-choice to a high-choice media regime benefits adults as consumers, it potentially comes at a civic cost to young people, who do not choose the environment they inherit and are more likely to be raised in polarized "epistemic bubbles" as their parents select into partisan news programs. I explore this hypothesis in three steps. First, I field a novel survey to collect descriptive evidence on the exposure of young people to partisan news media. Next, I present a simple overlapping generations model which generates a testable prediction–more media choice for parents will strengthen the transmission of beliefs from parents to children. Finally, I test that prediction with the rollout of Fox News, finding suggestive evidence that children of Republican parents raised in homes with more potential Fox News exposure are more likely to become Republicans themselves.

In the third chapter, coauthored with Felix Koenig, 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.

CHAPTER I

How Elastic are Preferences for Redistribution? New Results on Partisan Polarization

> *Repeatedly, I was to find, this rift was an emotional flashpoint... Others I talked to felt the same, only more strongly. Government-sponsored 'redistribution'? No!*

Arlie Hochschild[1]

1.1 Introduction

Has the shifting distribution of income in the U.S. changed preferences for redistribution? Two broad findings emerge from a recent literature on demand for redistributive taxation and transfers. The first is that despite a surge in income inequality, Americans' preferences for redistribution appear to have remained steady for decades, even among those with below average income (Kuziemko et al. (2015), Ashok et al. (2015)). This "puzzle in American political economy" (Bazzi et al. (2017), p. 4) has surprised public economists because both basic intuition and median voter models like Meltzer and Richard (1981) predict that as inequality increases, demand for redistribution will rise as well. Survey experiments have uncovered a second, related result–preferences for redistribution are difficult to nudge with information treatments. Facts about inequality (Kuziemko et al. (2015)), as well as intergenerational mobility (Alesina et al. (2018)) and immigration (Alesina et al. (2019)), appear to have relatively small effects on people's preferences. Taken together, the evidence

[1] See Hochschild (2016), p. 61 and p. 93.

to date has suggested that beliefs about redistribution in the U.S. are firmly held at the individual level and therefore largely stable in the aggregate.[2]

This paper argues that a focus on political party produces a quite different set of descriptive and experimental conclusions. In Section 1, I show that rising political polarization better explains preferences than rising income inequality. Specifically, I find that beneath the aggregate stability in preferences is a sharp divergence between Democrats and Republicans; since 1978, the gap in support for redistribution between the parties has more than doubled. I then demonstrate that the divide owes to preferences segregating rather than strengthening, meaning that while more extreme views have not become more common, deviations from party orthodoxy have became rarer. There are fewer and fewer Republicans who support redistribution and fewer Democrats who oppose it. This evidence offers a resolution–or at minimum a reframing–of the puzzle that preferences appear unchanged while inequality increases. Redistribution is an inherently normative and political subject, and along political lines these preferences are not stable.[3]

In Section 2, I return to the survey experiments from which Kuziemko et al. (2015) concluded, "Our results suggest that, generally, greater information can increase respondents' sense of concern about an issue, but not necessarily their support for policies that might ameliorate it. Information about inequality and poverty has only a limited (and typically statistically insignificant) tendency to increase support for higher income taxes on the well-off or transfer programs for the disadvantaged" (p. 1505).[4] Analysis of the political views of respondents offers a constructive revision of that conclusion–I find an information treatment that substantially increased support for taxation and transfers among conservatives, which is a novel result in the literature. This treatment focused not on broad inequality but on the economic circumstances of specific low-income families and how transfer policies could affect those families. Its effects among conservatives are precisely estimated and about five times the size of those previously reported, closing upwards of half the gap in preferences between the liberal and conservative control groups. Understanding this treatment and its effects is therefore especially relevant in a polarizing climate, when commentators like Klein (2020) have argued that we have entered "post-persuasion" politics. Section 3 offers brief concluding remarks on partisanship and redistribution.

[2]Throughout, I follow the literature and use the word "preferences" to mean support for redistribution, as reported in surveys and survey experiments. See Sen (2014) for a discussion of different possible meanings of preferences.

[3]Alesina et al. (2020) highlight different perceptions of inequality, mobility, and immigration across Democrats and Republicans and compare those perceptions to reality, whereas my findings are on policy *preferences*.

[4]I also discuss the comparable findings in Alesina et al. (2018) and Alesina et al. (2019) in Section 2.

1.2 Partisan Trends in Preferences

The left panel of Figure 1.1 reproduces Figure 1 from Kuziemko et al. (2015), which illustrates a surprising stylized fact in political economy: Preferences for redistribution in the U.S. show little movement since 1978, despite a dramatic increase in income inequality over that period. The plot shows responses to a General Social Survey (GSS) question on whether the government should reduce income differences.[5] Note that the trends in beliefs are flat even among below average income respondents. More inequality appears not to have led to more demand for redistribution, at least as measured by this standard survey instrument.

The right panel shows a widening gap in redistributive preferences between Democrats and Republicans. Beneath the stable population average, there has been a modest but steady increase in support for redistribution among Democrats and a somewhat more pronounced decline in support for redistribution among Republicans.[6] In 1978, the average difference between Democrats and Republicans on this seven-point scale was 0.97 points; by 2016 the difference had more than doubled to 2.17 points. What one can conclude from Figure 1.1 alone is only that self-identified Democrats and Republicans are further apart today than in previous decades. Whether one ought to call that divergence "polarization" or whether it might be driven by semantic or compositional changes that do not reflect underlying changes in preferences depends on precisely what polarization and party identification mean.

There are at least two types of preference polarization. The first and most obvious is illustrated in the top panel of Figure 1.2. This shows a stylized case in which a bimodal distribution of preferences emerges, with fewer people holding centrist views and more people holding more extreme views on both the left and right. The bottom panel of Figure 1.2 shows a second possibility–even with the overall distribution of beliefs in the population remaining relatively stable, average differences between parties can widen if those parties become better sorted, such that, for instance, it becomes less likely for a Republican to

[5]Here is the full question for the GSS *eqwlth* variable: "Some people think that the government in Washington ought to reduce the income differences between the rich and the poor, perhaps by raising the taxes of wealthy families or by giving income assistance to the poor. Others think that the government should not concern itself with reducing this income difference between the rich and the poor. Here is a card with a scale from 1 to 7. Think of a score of 1 as meaning that the government ought to reduce the income differences between rich and poor, and a score of 7 meaning that the government should not concern itself with reducing income differences. What score between 1 and 7 comes closest to the way you feel?" Researchers often reverse the responses so that higher numbers mean more support for redistribution.

[6]These larger changes among Republicans are consistent with the idea of "asymmetric polarization" advanced in Mann and Ornstein (2012a), Mann and Ornstein (2012b), and Mann (2014). The extent to which asymmetric polarization characterizes policy preferences more broadly is disputed. Murray (2014) argues that a comprehensive Pew study on polarization "undermines the notion, popular in Washington, of 'asymmetrical polarization'—which blames Republicans for causing the division."

support redistribution and less likely for a Democrat to oppose it. It is this latter form of polarization through sharpening party segregation that better characterizes redistributive beliefs.[7]

Figure 1.3 shows rising partisan sorting over time, plotting changes in the distribution of redistributive preferences from the start to the end of the GSS sample, 1978 to 2016. The left panel, for the full population, shows small changes and little evidence of polarization. While there is currently a slightly smaller share of Americans holding centrist views on redistribution than in 1978, there is no broad movement out to the left and right poles of the distribution. The within-party changes are larger and more clearly segregated. The distribution among Republicans, in the center panel, has shifted uniformly towards opposition to redistribution, such that while in 1978 fully 61 percent of Republicans had either neutral or supportive preferences for redistribution, just 37 percent did by 2016. Democrats have aligned less dramatically behind the opposite view, more uniformly favoring redistribution. In 1978, 44 percent of Democrats held positions neutral to or against redistribution, and by 2016 only 30 percent did. Deviations from the party position on redistribution have become rarer.

What could be driving the sharpening partisan sorting in preferences? There are several possible explanations. In an ethnography of Tea Party Republicans, Hochschild (2016) finds that income redistribution has become a "flashpoint" as modern Republican identity has consolidated around a worldview in which taxation and transfers amount to the government encouraging the unemployed and the undeserving to cut in line at working people's expense. The evidence I have presented is consistent with those qualitative findings, as well as with the polarizing climate more generally. But even without broad ideological changes, sorting along economic and demographic dimensions could also account for the observed party differences. If, for instance, poorer people more strongly support redistribution, and poorer people have become less likely to be Republicans, we would observe the divergence across parties shown in Figure 1.1 and the sorting in Figure 1.3. It could also be that redistributive beliefs across the wealthier and poorer, the more and less educated, white and non-white, or men and women have diverged, or that moderates have gradually left both the Democratic and Republican parties, leaving behind more strident partisans who now drive raw cross-party differences.[8] I

[7]And other views as well. Political scientists Fiorina and Abrams (2014) note that "What has changed is how partisans are distributed in terms of their ideology and issue opinions. Self-identified Democrats have become more homogeneously liberal and self-identified Republicans more homogeneously conservative." See Fiorina and Abrams (2008) for a review of the evidence on sorting. Klein (2020) (p. 31-36) also offers a thorough discussion of the distinction ("Polarization is not extremism, but it is sorting."), interpreting changes like those in the top panel as polarization around beliefs and those in the bottom panel as polarization around party identities.

[8]Deeper questions of how respondents might differentially interpret a fixed survey question over time

turn next to a discussion of these possibilities, finding no strong evidence that demographic sorting or mechanical attrition are beneath the political divide.

Following Alesina et al. (2011), Table 1.1 reports results from regressions on the descriptive relationships between preferences for redistribution and individual demographic characteristics in the GSS, with a new focus on the evolving role of political party. Panel A of the table compares the first and last years of the GSS sample, 1978 and 2016. In columns 1 and 2, I regress preferences on party affiliation and demographics. The outcome is an indicator equal to one if the individual supports redistribution (coded as a response greater than four on the seven-point GSS scale), and independents, who identify as neither Republicans nor Democrats, are the excluded category. The main results again highlight the partisan divergence. Column 1 shows that in 1978 Republicans were 7.5 percentage points less likely to favor redistribution than independents, while Democrats were 7.6 more likely, implying a rougly 15 percentage point gap between Republicans and Democrats (though the coefficients on the parties are not statistically distinguishable in 1978, given the relatively small differences between them and the small sample size). By 2016, shown in column 2, that partisan gap in the probability of supporting redistribution had tripled to 45 percent. To address the small samples and possible sensitivity to the start and end years, Panel B of the table repeats the exercise using the first and second halves of the sample. Columns 5 and 6 show that between the pooled first (1978-1996) and second (1998-2016) periods, the gap increased from 19 to 31 percent.

Table 1.1 also shows little evidence that demographic sorting underlies the partisan polarization.[9] Consider first the possibility that preferences have diverged across various demographic groups based on income, education, etc. Columns 1 and 2 show that income has if anything become a less important predictor of preferences over time, and that differences across other demographic dimensions are either steady or declining as well.[10] This result bears on a second possibility, discussed above, which is that characteristics like income or education determine preferences, and the income or education of the average Republican or

are difficult to address. The GSS is useful because it has asked an identically-worded question about redistribution since the 1970's. But one could view this as problematic, as people might have had quite different redistributive policies in mind in 1978 and 2016. One solution is data on beliefs about specific policies, which I use in the next section on experimental results.

[9]Figure A.2 in the appendix shows more detailed year-by-year demographic trends.

[10]See Table A.1 in the appendix for results with dollar income, rather than the binary income variable used here, *finrela* in the GSS. The qualitative conclusions are unchanged. Use of *finrela* has two limitations, which are that it is by design a perception–whether or not respondents *believe* they have below average income–and it will not capture granular sorting at the top or bottom of the income distribution. I report it in Table 1.1 in part for interpretability. One can more meaningfully compare the roles of party and income with a binary income variable. The *finrela* variable also features in Figure 1.1 above and in Kuziemko et al. (2015), and Cruces et al. (2013) have shown that perceptions of one's position in the income distribution matter for preferences.

Democrat are systematically changing over time. To test for this sorting in the GSS sample, in columns 3 and 4 I regress an indicator equal to one for Republicans and zero otherwise on individual characteristics. The demographic differences in party composition between 1978 and 2016 are modest and statistically insignificant. I repeat the regressions with the first and second halves of the sample in columns 7 and 8, which show that individuals with college degrees are marginally less likely to be Republicans in the second period than the first. Otherwise, the differences are small in magnitude and not statistically different from zero. The partisan gap in preferences appears not to have arisen because the parties currently contain substantially different demographic types of people than in the past.[11]

Another plausible possibility is that over time people with moderate preferences are leaving the Republican and Democratic parties and becoming independents. If this were true, then even without any individual changing his or her beliefs, the average Republican and Democrat in the data would appear increasingly extreme, mechanically producing a divergence. While there has been an uptick in the share of those in "other" parties or no party, the changes are small relative to the doubling and tripling partisan gaps documented above. In the GSS data, from 1978 to 2016, the share of self-identified independents in the sample increased from 14.5 percent to 16.7 percent if one defines independents as excluding those respondents who say they have partisan leanings (but do not strictly identify as a Republican or Democrat), and from 36.3 percent to 41.3 percent when including the so-called partisan "leaners".[12] Assuming these observed levels of party attrition and a distribution of preferences fixed at the 1978 baseline, attrition alone creates a gap 2.2-6.4 percent the size of the actual divergence. In a review of the literature on political polarization, Gentzkow (2016) describes similar trends in partisan and ideological identification dating back to World War II, concluding from American National Election Study data that "the shares calling themselves 'conservative,' 'liberal,' or 'moderate' have remained remarkably stable, with no hint of a move toward the extremes in recent years" (p. 7).

This section has documented political sorting in Americans' preferences for redistribution, offering a new perspective on the extant puzzle in political economy that preferences have remained stable despite a spike in income inequality. It is not increasing inequality but

[11]Boxell (2019) shows that, more generally, demographic change can account for a nontrivial share of polarization in the population, and Boxell et al. (2017) find that polarization has increased most among those over the age of 65.

[12]See appendix Figure A.1 for main findings inclusive of leaners and appendix Figure A.3 for the complete picture of party identification over the sample period. Interestingly, Smidt (2017) finds that self-identified independents in the 2000's voted more reliably for one party than self-identified strong partisans in the 1970's, and Krupnikov and Klar (2014), in an article titled "Why People Call Themselves 'Independent' Even When They Aren't," argue that the rise in self-styled independents has to do with the "socially desirable label" of independence amidst ugly politics.

rather political polarization that is driving preferences, which are segregating by party. Next I revisit experimental findings on inequality and preference formation, with a new focus on the political views of respondents, which uncovers a second result on the key role of partisanship. A Kuziemko et al. (2015) experiment reported as having small and inconsistent effects was in fact powerfully persuasive for conservatives, closing a substantial share of the gap in preferences between liberal and conservative control groups.

1.3 Political Views and Preference Elasticities

Kuziemko et al. (2015) sparked a recent literature of survey experiments in economics whose findings are broadly consistent–information treatments move people's factual perceptions but tend, with some exceptions, to change their preferences for redistributive policies modestly if at all. More specifically, Kuziemko et al. (2015) propose a natural hypothesis that could explain the coexistence of rising inequality and aggregate stability in preferences, which is that people may simply be uninformed about the extent of the increase in inequality. It could be that if people knew more about inequality, they would show more support for redistribution. The authors test that hypothesis with a survey experiment in which randomly selected respondents are treated with information about income inequality, and they conclude that the "treatment has large effects on views about inequality but only slightly moves tax and transfer policy preferences" (p. 1478). Alesina et al. (2018) and Alesina et al. (2019) reach similar conclusions on the effects of treatments relating to intergenerational mobility and immigration, respectively.[13] It appears difficult to move people's views on redistribution with economic facts, in part because information treatments could be "preaching to the choir" or "falling on deaf ears" (Alesina et al. 2018, p. 523), meaning left-wing respondents already support redistribution but right-wing respondents too firmly oppose it to be persuaded by short treatments.

The conclusions in Kuziemko et al. (2015) on inequality and redistribution, however, are drawn from results averaging over respondents of varying political views.[14] Social psychologists have established that there are deep differences in normative instincts between

[13] Alesina et al. (2018) test how responsive preferences for redistribution are to information about mobility and find that "Despite the significant and durable impact on perceptions of mobility, the treatment has no significant impact on the value ascribed to redistributive policies by the government" (p. 549). Alesina et al. (2019) study how perceptions of immigration affect redistributive preferences. Here merely asking people questions about their views on the number and origin of immigrants somewhat reduces support for redistribution through a priming effect, but "none of the favorable informational treatments is able to overcome the negative effects on redistribution of prompting people to think at length about immigrants' characteristics" (p. 4).

[14] Treatment effects in the paper are reported as divided by the average difference in preferences between liberals and conservatives, so political views feature as scaling factors but not as objects of study themselves.

right-leaning and left-leaning individuals (see Haidt (2013) for a moral foundations theory of politics), so one might expect two types of heterogeneity in response to the normative appeals tested in Kuziemko et al. (2015). First, for survey respondents of the same political views, different information treatments could have different effects on preferences; second, a given type of information treatment could affect respondents of different political views differently. Next I give summaries of the treatments in three of the Kuziemko et al. (2015) experiments–called the omnibus, emotional, and policy experiments, respectively–before presenting evidence on heterogeneous treatment effects.[15]

In the omnibus experiment, "the goal of the information treatments was to provide a large 'shock' to individuals' knowledge about inequality and redistributive policies" (p. 1484). That shock comprised three pieces. First, treated respondents saw data and figures on the distribution of income and their position in that distribution; next was a counterfactual exercise showing the respondent how much income their household *would* be making today had economic growth been more equally distributed since 1980; and lastly was information about top tax rates and the estate tax.[16] The emotional experiment took a different approach, focusing on the bottom rather than the top of the income distribution, asking respondents to fill out a budget of minimal monthly expenses necessary for rent, food, utilities, etc. for a family of the same composition as their own. That budget was then compared to poverty-line income (three-quarters of respondents tally a budget that exceeds the poverty-line, even without major expenses like health care). Finally, the policy experiment opens with the emotional treatment budgeting exercise before describing both a family making a minimum-wage income and also precisely what the food stamps program would provide that family each month, so that "the connection between poverty and a government program is made explicitly" (p. 1503).

I find that this latter test, the policy experiment, has strong effects that were hidden by the average results. Table 1.2 contrasts the average, full-sample findings with those from separate regressions for conservatives, with the outcome in each column increasing in support for that tax or transfer policy.[17] The scaled effect in the tables is reported both in Kuziemko et al. (2015) and here for interpretability; it refers to the raw treatment effect divided by the

[15]I omit two other experiments in the paper that focused more narrowly on the estate tax and trust in the government.

[16]Readers can take the survey at `https://hbs.qualtrics.com/SE/?SID=SV_77fSvTy12ZSBihn`.

[17]In the policy experiment survey, Kuziemko et al. (2015) did not ask for the party of the respondent, but did ask whether individuals considered themselves liberal, moderate, or conservative on economic issues, which is the variable *policyview* in the data. I use *policyview* for the main analysis and show in appendix Table A.2 that the results are similar using the preferred presidential candidate of the respondent. See also Appendix B for a full breakdown of the omnibus, emotional, and policy experiment results by all available measures of political beliefs.

average difference in beliefs between control-group liberals and conservatives.[18] Kuziemko et al. (2015) concluded from this experiment that "with the exception of the minimum wage, these effects are still relatively small (roughly about 10-15 percent of the political gap)" (p. 1503) and also that "the treatment does not have a consistent effect in terms of inducing treatment participants to desire higher taxes (even if only on the wealthy) to pay for these programs" (p. 1504). Panel A of Table 1.2 shows that those two claims are true on average. In the full sample, preferences for taxation (columms 1 and 2) and various policy interventions (columns 3 through 8) move little.

Table 1.2 also shows effects from separate regressions for conservatives. Note that the samples are small, a feature of the respondent pool on the Amazon mTurk platform where the surveys were administered. Yet the policy experiment had reliably large and precise effects on conservative preferences for redistribution, effects 25-45 percent of the political gap, meaning this concise treatment was so compelling that, for a typical conservative, it closed upwards of half the distance between liberal and conservative beliefs, which is a new finding in a literature whose effects on preferences have otherwise been reported as relatively small.[19] Among all respondents, the treatment had a modestly sized and marginally significant effect on preferences for the top tax rate and a small (about 5 percent of the political gap) and statistically insignificant effect on preferences for a tax on millionaires. But among conservatives, these effects on tax preferences are statistically and economically significant, on the order of 30 percent of the political gap. Similarly, on the expenditure side, with support for redistributive policies like food stamps, housing assistance, and aid to the poor more generally, the average effects reported in the paper and again here in Table 1.2 are small, while for conservatives, the treatment induced large changes both in support for specific policies and in broader beliefs on the appropriate role of government in redistributing income. The policy experiment moved conservatives 44 percent of the way to the liberal position on the minimum wage, 43 percent on food stamps, 40 percent on housing assistance, and, on more general views, 37 percent on government aid to the poor and 25 percent on the scope of government. The interactions in Panel B of Table 1.2 show that, even with small samples, the majority of these effects among conservatives are statistically distinguishable not only from zero but also from the treatment effects for liberals.

To restate the central finding: Information about the economic circumstances of low-income families combined with details on policies that could support those families

[18]The scaled effect also allows consistent comparison of effect sizes across experiments. Some questions eliciting preferences were worded slightly differently in the omnibus than in the emotional and policy experiments.

[19]One might wonder about persistence–weeks or months later, did the effects on preferences last? There were no follow-up surveys to the policy experiment, so data for persistence tests are not available.

induced conservatives to support substantially more taxation and redistribution. What was it about the policy experiment that was so convincing to conservatives? Recall first that the policy treatment was identical to the emotional experiment, with the addition of specific policy information. The fact that the emotional experiment was much less effective at moving preferences among conservatives (shown in Appendix B) suggests that the explicit connection between disadvantaged households and the actual policies designed to aid those households matters. It could also be that what the policy treatment did not contain is as important as what it did. The policy experiment, notably, did not include a discussion of high-level inequality. In fact, unlike the omnibus treatment, the policy treatment did not use the word "inequality," a politicized subject that may constitute a tribal trigger for right-leaning respondents that precludes persuasion.[20] Lukianoff and Haidt (2018), in a discussion of the role of tribalism and identity in contemporary politics, write that "When the 'tribe switch' is activated, we bind ourselves more tightly to the group, we embrace and defend the group's moral matrix, and we stop thinking for ourselves... In tribal mode, we seem to go blind to arguments and information that challenge our team's narrative" (p. 58). Moreover, they note, "Local conditions can turn the tribalism up, down, or off... making people highly attentive to signs that reveal which team another person is on" (p. 59). The relevance of these tribal identities and local conditions in shaping preferences for redistribution is one avenue for future study.

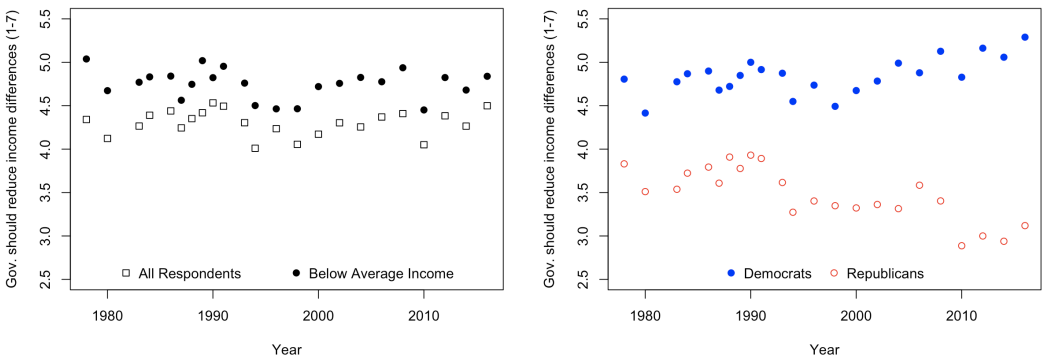
1.4 Conclusion

Previous research has puzzled over the absence of comovement between income inequality and preferences for income redistribution. Preferences appear to have moved little amidst dramatic increases in inequality. I have documented that preferences are in fact polarizing along political lines, with partisan sorting producing a widening gap between Democrats and Republicans. This focus on party produces new experimental conclusions as well. Kuziemko et al. (2015) summarized their experimental findings as follows: "Our results suggest that, generally, greater information can increase respondents' sense of concern about an issue, but not necessarily their support for policies that might ameliorate it. Information about inequality and poverty has only a limited (and typically statistically insignificant) tendency to increase support for higher income taxes on the well-off or transfer programs for the disadvantaged" (p. 1505). I have shown that an information treatment combining data on poverty and policy led to large and statistically significant increases in support for

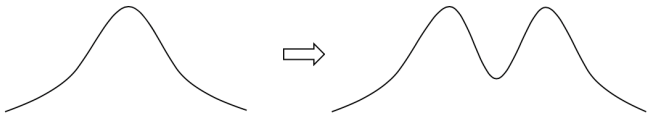
[20]One way to test this hypothesis would be to use data on whether respondents believed the treatments to be politically biased. Unfortunately, while that question was asked in the Alesina et al. (2019) work on redistribution and mobility (see p. 530 for a brief discussion), it was not asked in Kuziemko et al. (2015).

taxation and redistribution among conservatives. This finding–that information treatments have potential to meaningfully change people's views on normative economic questions–is especially relevant given my descriptive evidence that redistributive beliefs are segregating by party. Even in a broadly polarized climate, preferences for redistribution are not closed to ethical and empirical appeals.

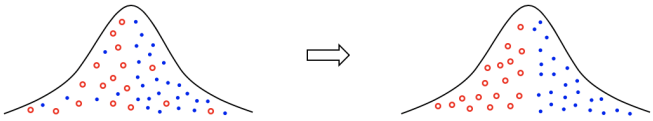
Figure 1.1: U.S. Preferences for Redistribution, 1978-2016



Notes: The figures show responses to the GSS *eqwlth* question on whether the government ought to reduce income diffences. Higher numbers indicate more support for redistribution. Party identification comes from the GSS *partyid* variable; support for redistribution among independents, not shown in the right panel, has increased slightly, less than among Democrats. Black oversamples are dropped in this and subsequent GSS figures and tables. See appendix Figure A.1 for plots with alternative party definitions and sample weighting that show similar divergences across Democrats and Republicans.



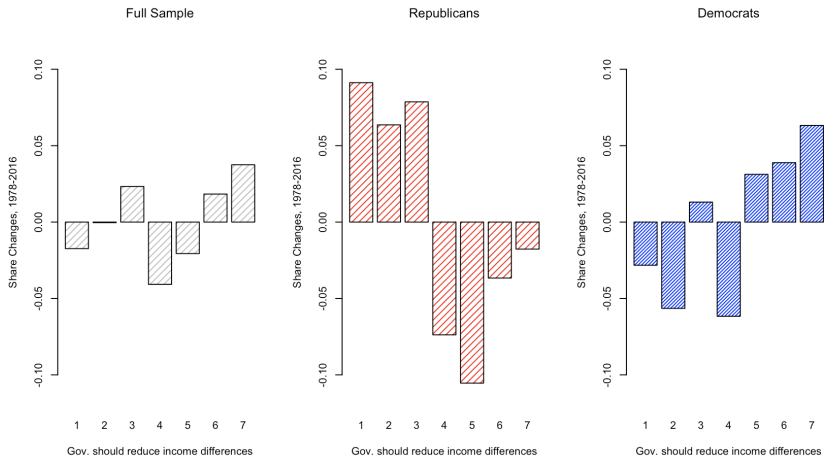
(a) Polarization as a bimodal distribution.



(b) Polarization as segregation in beliefs.

Figure 1.2: Two Stylized Types of Polarization

Figure 1.3: Changes in Preferences, 1978-2016



Notes: These figures depict changes in the share of respondents holding each view on the seven-point GSS scale from 1978 to 2016. The middle bar in the left panel, for example, shows that four percent fewer Americans held centrist views on redistribution in 2016 than in 1978.

Table 1.1: Party, Income, and Preferences for Redistribution

	Panel A: First and Last Years of Sample				Panel B: First and Second Halves of Sample			
Outcome	Support Redistribution		Republican Party = 1		Support Redistribution		Republican Party = 1	
	1978	2016	1978	2016	1978-1996	1998-2016	1978-1996	1998-2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Republican	-0.075	-0.264	-	-	-0.108	-0.173	-	-
	(0.049)	(0.028)			(0.010)	(0.010)		
Democrat	0.076	0.193	-	-	0.078	0.137	-	-
	(0.042)	(0.025)			(0.009)	(0.009)		
Below Avg. Inc.	0.131	0.079	-0.049	-0.056	0.103	0.106	-0.063	-0.073
	(0.040)	(0.023)	(0.035)	(0.020)	(0.008)	(0.009)	(0.008)	(0.008)
College Degree	-0.084	0.028	-0.019	-0.002	-0.087	-0.014	0.073	0.025
	(0.049)	(0.024)	(0.041)	(0.020)	(0.009)	(0.009)	(0.008)	(0.008)
White	-0.192	0.010	0.158	0.147	-0.116	-0.023	0.206	0.193
	(0.055)	(0.026)	(0.047)	(0.022)	(0.011)	(0.010)	(0.010)	(0.008)
Male	-0.055	-0.042	-0.029	0.015	-0.051	-0.032	-0.001	0.023
	(0.037)	(0.022)	(0.031)	(0.019)	(0.008)	(0.008)	(0.007)	(0.007)
Age	0.002	-0.003	0.003	0.002	-0.001	-0.003	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Independent Mean	0.448	0.519	-	-	0.475	0.473	-	-
Partisan Gap	0.151	0.457	-	-	0.186	0.310	-	-
Observations	737	1,892	737	1,892	16,514	14,787	16,514	14,787

Notes: Data are from the GSS. All regressions are linear probability models. Columns 1 and 2 regress an indicator for support for redistribution (from the *eqwlth* variable) on party affiliation and demographics, with independents as the excluded party category. Columns 3 and 4 regress an indicator equal to one for Republicans on demographics. Columns 5-8 show estimates cutting the sample by the first and second halves rather than the first and last years. "Below Avg. Inc." is the GSS variable *finrela*, an indicator equal to one if the respondent believes he or she has income below the national average. Independent mean refers to the average of the outcome variable among independents, and the partisan gap is the difference between the Republican and Democrat coefficients. See text for further details.

Table 1.2: Policy Experiment Treatment Effects

	Top rate (1)	$1M tax (2)	Estate (3)	Min. Wage (4)	Food St. (5)	Scope (6)	Aid (7)	Housing (8)
Panel A: Separate Regressions for Conservatives								
Full Sample	2.023	0.0311	-0.0217	0.323	0.313	0.0569	0.133	0.176
	(1.209)	(0.0240)	(0.0258)	(0.0949)	(0.0970)	(0.0613)	(0.0638)	(0.0636)
Control mean	33.79	0.713	0.269	2.546	1.832	3.097	2.559	2.539
Scaled effect	0.140	0.0536	0.0642	0.310	0.153	0.0362	0.0811	0.129
Observations	1111	1110	1110	806	806	1111	806	806
Conservatives	4.294	0.175	-0.00164	0.461	0.881	0.395	0.608	0.556
	(2.376)	(0.0613)	(0.0340)	(0.188)	(0.230)	(0.138)	(0.173)	(0.165)
Control mean	24.44	0.327	0.0748	1.921	0.449	2.129	1.494	1.652
Scaled effect	0.298	0.302	0.00484	0.441	0.432	0.252	0.370	0.407
Observations	280	279	280	188	188	280	188	188
Panel B: Interactions by Political Views								
	Top rate (1)	$1M tax (2)	Estate (3)	Min. Wage (4)	Food St. (5)	Scope (6)	Aid (7)	Housing (8)
Policy	0.764	-0.00277	-0.0679	0.153	0.160	-0.144	-0.0550	0.0761
	(1.792)	(0.0361)	(0.0384)	(0.140)	(0.146)	(0.0940)	(0.0980)	(0.0972)
Pol. ×Moderate	1.702	0.00992	0.0962	0.342	0.0906	0.293	0.183	0.0765
	(2.857)	(0.0576)	(0.0612)	(0.225)	(0.235)	(0.150)	(0.158)	(0.157)
Pol. ×Conserv.	3.613	0.156	0.0820	0.332	0.653	0.524	0.645	0.397
	(2.957)	(0.0597)	(0.0633)	(0.239)	(0.250)	(0.155)	(0.168)	(0.167)
Observations	1111	1110	1110	806	806	1111	806	806

Notes: Panel A shows results from the policy experiment with the full sample, averaging over respondents of all political views. Panel B interacts the policy information treatment with the political views of the respondent (variable *policyview* in the Kuziemko et al. (2015) data, which takes values liberal, moderate, or conservative), with liberals as the omitted category. The columns refer to various measures of respondents' preferences for redistribution, where higher values indicate stronger support for taxation and transfers. Specifically, in the omnibus experiment, according to Kuziemko et al. (2015), the "Top rate" variable "is continuous (respondents' preferred average tax rate (in percent) on the richest 1 percent). "Scope" is also continuous (a 1–5 variable, increasing in the preferred scope of government activities). All other variables are binary. "$1M tax" and "Estate" indicate the respondent wants income taxes on millionaires and the estate tax to increase, respectively... "Min. Wage" indicates support for increasing the minimum wage." The "Food St." outcome is similarly a binary question about support for the food stamps program. In the emotional and policy experiments, rather than binary, the variables on the minimum wage, aid to the poor, food stamps, and public housing "are all categorical 0-4 and increasing in support." The scaled effect is the treatment effect divided by the average difference in preferences between control-group liberals and conservatives. See the Kuziemko et al. (2015) web Appendix for full survey instruments.

CHAPTER II

Choice in the Market for News: A Tradeoff of Civic and Consumer Welfare

> *Most people seem to think that freedom consists of respect for consumption choices, whatever their origins and content... A similar conception of freedom lies behind many of the celebrations of emerging communications markets... But freedom requires certain preconditions, ensuring not just respect for choices and the satisfaction of preferences, whatever they happen to be, but also the free formation of desires and beliefs.*

Cass Sunstein[1]

> *When a community succeeds in turning down everyone's tribal circuits, there is more room for individuals to construct lives of their own choosing.*

Jonathan Haidt and Greg Lukianoff[2]

> *I'm probably a victim, or a product, however you want to phrase it, of my background.*

Joe Biden[3]

[1]Sunstein (2017), p. 159.
[2]Lukianoff and Haidt (2018), p. 59.
[3]Klein (2020), p. 15.

2.1 Introduction

Prior (2007) has documented in detail our shift from a low-choice to high-choice media regime. In 1970, three broadcast networks–ABC, CBS, and NBC–captured 80 percent of television viewership, and among homes with the television on at night, 75 percent watched a network newscast. These evening news programs aired similar content, all at the same time, leaving little discretion for viewers. Moreover, Klein (2020) has pointed out that with "three networks given access to public airwaves, the business model was about appealing to as wide an audience as possible" (p. 145), so these stations presented a largely neutral version of events.[4] Cable news offers more choices with more partisan perspectives. By 2005, news viewership on cable had exceeded that on broadcast networks, and today viewers are more sorted by party into like-minded cable shows. Fox News in particular has had an outsized audience and influence on the right. Gentzkow and Shapiro (2011) estimate that the ratio of conservative-to-liberal viewers of Fox News is about five-to-one, and, according to Pew, Fox was the primary source of news for 40 percent of Trump voters in the 2016 campaign (see Gottfried et al. (2017); the corresponding figure for MSNBC among Clinton voters was 9 percent).

What can economists say about this transition? One clear consequence is that news consumers, in their capacity as consumers, are better off with more choices, just as food consumers would be better off with more restaurant choices. Posner (2005) asks of the increasingly segregated media market, "Does this mean that the news media were better before competition polarized them?" "Not at all," he answers, because markets provide "what the consumer wants, and the more intense the competitive pressure, the better they do it." Another consideration in evaluating the news media market is its civic implications, which can be in tension with consumer welfare. One strand of the economics literature, for example, examines how increased choice affects the accuracy of reported news and of people's beliefs. The civic importance of a well-informed public, discussed by economists as early as Becker (1958), are summarized more recently in Gentzkow and Shapiro (2011): "Democracy is most effective when citizens have accurate beliefs" (p. 1799).[5] This paper offers a new intergenerational perspective on a potential tradeoff between consumer choices and civic objectives in the market for news. In the context of the expansion of choice in television news, I ask how the media consumption of one generation affects the conditions

[4]This mid-twentieth century period when the press aimed to maintain at least an appearance of balance and objectivity was a historical anomoly. See Chapter 2 of Hamilton (2004) for a discussion of nineteenth century newspapers, just 13 percent of which identified as independent, the rest being explicitly party-affiliated.

[5]See also Mullainathan and Shleifer (2005), Gentzkow and Shapiro (2006), and Gentzkow and Shapiro (2008a).

in which the political preferences of the next generation are formed.[6] The central idea is that partisan news consumption by parents could act as an externality that "pollutes" the formative environment of young people, who may have less exposure to competing ideas and information in the home, and therefore less of an opportunity to form their own views, as their parents sort into partisan news sources. To fix ideas, Figure 2.1 presents a stylized illustration of the shift from the low-choice to high-choice news regimes. In the low-choice example, children of both Democratic and Republican parents are exposed to ABC news; in the high-choice example, by contrast, children of Democratic parents are exposed to MSNBC, children of Republican parents to Fox News.

This paper makes three contributions in exploring the role of cross-generation partisan media influence. First, I field a novel online survey to descriptively assess the importance of parents' news media in children's upbringings. To my knowledge, this is the first descriptive evidence on this subject. The survey was designed to gauge not just levels but also trends in the exposure of young people to partisan news. Two simple features of the survey allow me to measure how young people's self-reported exposure to partisan news has evolved over time. First, I can examine the responses to questions like the following separately by age cohort: "Overall, when you were growing up (ages 0-18), were you ever exposed to partisan news media from your parents or guardians?" Second, I can compare self-reported formative exposure of respondents to partisan news from their parents to how often respondents say their own children are exposed to their partisan news. The pattern that emerges, and the central finding of the survey, is striking. I define a cohort as the year in which a respondent was ten years old. For cohorts through the 1980's, around half of respondents say they were "Frequently" or "Occastionally" to partisan news from their parents; that number shoots up to nearly three-quarters in the 1990's, when Fox News and MSNBC were launched.[7]

The survey also elicits values of respondents to gauge the hypothesized tension between free media choice for adults and a balanced formative environment for young people. 91% of respondents indicated that they believe people ought to be free to choose whatever news media outlets they wish, and 90% also said they agreed that "young people (age 0-18) deserve the opportunity to form their own political beliefs in an environment with exposure to relatively neutral news and different points of view." When then asked if kids in general today do indeed have that opportunity, only 13% agreed (and when asked if *their* kids have that opportunity 31% answered yes).

[6]Throughout, I use "political preferences" interchangeably with ideology or party. Ultimately, my model will yield predictions that I test in the data on the self-reported party affiliation of parents and their children.

[7]While both Fox and MSNBC were launched in 1996, MSNBC did not become the forwardly liberal outlet it is known as today until the mid-2000's.

My next contribution is a stylized overlapping generations model of media choice.[8] The model is intended to be a basic conceptual framework that serves two purposes in evaluating the shift from a low-choice to a high-choice media regime. First, the model generates a prediction that I can bring to the data, which is that expanded media choice for parents will strengthen the transmission of political views from parents to children. Second, the framework allows a light formalization of the welfare-like concept that survey respondents almost universally supported–young people ought to have an environment in which they can make up their own minds. In the context of the model, I call this agency, and I discuss the relationship between agency for young people and the choice environment for adults.

For exposition, Figure 2.2 offers an anology between agency and income mobility. Consider first three concepts more familiar to economists–inequality, mobility, and opportunity. The first two are measurable features of the economy, and the last is a normative value. Inequality is a feature of some static income distribution, and mobility refers to how easily individuals can move from one place in that distribution to another. I argue that we tend to take mobility as an important indicator because it reflects the extent to which an economy (and policy) is upholding the normative value of equal opportunity; low mobility suggests low opportunity, and that it is circumstances of birth more than individual effort or talent that are determining labor market outcomes. Likewise, in the context of political preferences, consider the analogous concepts of polarization, transmission, and agency. Polarization is a feature of a distrubtion of preferences, but this paper is not at heart about polarization. It is about transmission and agency. I argue that high transmission of political views from one generation to the next indicates a society that is not upholding the value of agency. In such a world, ideology is merely inherited, which is at odds with the values expressed by respondents in my survey and the excerpt with which I started the paper from Sunstein (2017): "Freedom requires certain preconditions, ensuring not just respect for choices and the satisfaction of preferences, whatever they happen to be, but also the free formation of desires and beliefs" (p. 159).

My third and final contribution is to test the transmission prediction using the rollout of Fox News. I follow an empirical design pioneered by DellaVigna and Kaplan (2007), who exploited the staggered introduction of Fox News across media markets in the 1990's and 2000's and, using a sample of twenty-eight states, find that Fox increased Republican vote share in 2000 presidential election by 0.4-0.7 percentage (effects that sound modest but are enough to turn tight elections).[9] I exploit that same variation with updated Nielsen data

[8]Stylized in the sense that I do not micro-found behavior in utility maximization, but rather relate the key pieces in a simple statical model.

[9]Another closely related paper is Martin and Yurukoglu (2017), who study Fox News election effects using the channel position as an instrument. It turns out people watch less Fox News when it is higher

to estimate how potential childhood exposure to Fox affects the likelihood that children of Republicans grow up to become Republicans themselves. With a YouGov survey on political views of linked parents and children, I find that in Republican-leaning zipcodes, an additional year of Fox News exposure is associated with around a three percentage point increase in the likelihood that a child of Republican parents identifies as a Republican. I also use the Targetsmart voter file, which contains administrative records on voter registration, and find no evidence that Fox News spurred changes in the transmission of party registration.[10]

The rest of this paper is organized as follows. In Section 2.2, I describe the survey and the descriptive evidence it offers. Section 2.3 lays out the model, showing how media choice relates to cross-generation preference transmission and agency. In Section 2.4, I present the main empirical results on the effects of Fox News, and Section 2.5 concludes.

2.2 Descriptive Evidence

I fielded the survey in July 2020 through Amazon's mTurk, an online platform frequently used for convenience samples by social scientists (e.g. in economics, Kuziemko et al. (2015) use mTurk in their study of preferences for redistribution). This survey data has several strengths and weaknesses. The central limitation is that the sample is non-representative. Below I do compare the demographic and political characteristics of my mTurk sample to the U.S. population to gauge its representativeness, and I also take several steps to ensure data quality. First, I follow the guidance outlined in Kuziemko et al. (2015), including implementing an attention check midway through the survey, taking only individuals with at least a 90% approval rating on previous mTurk tasks, and, to screen out international respondents posing as U.S. residents, restricting responses to U.S. IP addresses and launching the survey in U.S. daytime hours.[11] Following Moss (2018), I administered the survey with CloudResearch (TurkPrime) to block suspicious geocodes that may include bots and to oversample women to reach gender parity. Based on the advice of Lopez and Hillygus (2018), who find that on contentious topics respondents can give deliberatively provocative responses, I included recommended "trolling" checks for sincerity. Finally, I recorded response time and

up in the channel lineup. With this variation, the authors find similarly sized effects to DellaVigna and Kaplan (2007) in 2000 and document that those effects grew in the 2004 and 2008 elections. More recently Jamieson and Albarracin (2020), Bursztyn et al. (2020), and Simonov et al. (2020) study how Fox News has affected misinformation, COVID cases and deaths, and compliance with social distancing guidance during the pandemic.

[10] I discuss below why one might expect to find different effects on party identification and voter behavior. The former is first of all a lower bar to clear with media influence–one could see changes in self-reported party affiliation without seeing changes in voting, particularly in a low-turnout country like the U.S. Moreover, the administrative data have major limitations, notably a weak statistical linkage between parents and children.

[11] See question B23 in appendix Section B.1.2 for the attention check text.

dropped a small number of low outliers (less than five minutes) who clicked randomly through the survey.

The chief advantages of the mTurk survey are twofold. First, it is about 1/30 the cost of traditional surveys; I paid respondents $1.67 to complete the survey, targeting a $10 hourly wage for the ten-minute survey. Second, to my knowledge, this is the first survey evidence on the influence (or lack thereof) of parental partisan media on young people's formative environments. This makes the novel evidence well worth the shortcomings of mTurk, since no superior data exist. Table 2.1 shows how the mTurk sample compares to a representative sample from the American Community Survey. On age, gender, and race, the mTurk sample is reasonably representative. Respondents to the mTurk survey were more educated and somewhat more Republican, however. 68.9% of the mTurk sample reported holding a bachelor's degree compared to just 31.5% of the general population. 41.8% of the mTurk sample identified as Republican, compared to 39% of the population as measured in the 2016 American National Election Study. The table also shows somewhat stronger support for Donald Trump in the mTurk sample than in the general population.

The survey consisted of three main sections. First, Section A collected basic demographic information. Section B covered respondents' upbringings and the role of their parents' or guardians' new media. Lastly, Section C surveyed respondents on a set of values questions, including their views and experiences relating to their own children, before closing with questions on political affiliation. Politics of the respondent were included last both to prevent priming respondents into their party identities, and also to ensure they knew that the survey was not tailored differently to people of different political views, which could risk perceptions of bias in the survey. The final section allowed open responses on the subject and generated a number of insightful (and humorous) comments. These responses are listed in appendix Section B.1.1. The full set of survey instruments and response options is listed in appendix Section B.1.2.[12]

The first purpose of the survey was to guage the levels and trends in exposure of young people to partisan media from their parents. Is this an important force in society? Or a negligible one? I begin with data on the question, "Overall, when you were growing up (ages 0-18), were you ever exposed to partisan news media from your parents or guardians?" Responses options on a four-point Likert Scale included "Yes, frequently," "Yes, occasionally," "Rarely," and "No, never." Figure 2.3 shows the share of respondents answering frequently or occasionally by cohort, defined as the year in which the respondent was ten years old. A stark pattern emerges. Note first that around half of respondents

[12]Readers can take the survey at the following link: `https://umich.qualtrics.com/jfe/form/SV_72hLOEJK7rRYj3f`.

through the 1980's cohorts answered yes. A sharp break occurs in the 1990's, when both Fox News and MSNBC were launched (though as explained above, MSNBC would not take a strong partisan turn until the 2000's). This pattern suggests that indeed the advent of cable news could have played a role in increasing the exposure of young people to partisan press.[13]

The second main purpose of the survey was to assess various values respondents may hold relating to media choice and kids' formative environments. Table 2.2 summarizes answers to four such questions. The first is, "Do you agree or disagree that young people (ages 0-18) deserve the opportunity to form their own political beliefs in an environment with exposure to relatively neutral news and different points of view?" That may seem like it is difficult to disagree with, but, in a way, that is the point–this appears to be a value with near-universal support. Column 1 of Table 2.2 shows that 90% of respondents answered they either "strongly agree" or "agree." Turning to Column 2, however, we see that just 13% of respondents believe that kids have that opportunity (this shows the share answering "mostly neutral" or "completely neutral" to: "Overall, would you say most young people today are raised in a political news media environment that is more one-sided and partisan, or more neutral and balanced?") A somewhat larger share in Column 3 reported that their own kids (for those who have them) have had fair environments, which is unsurprising given the social desirability of such a response. Importantly, Column 4 reports overwhelming agreement in response to the question, "In general, do you agree or disagree that people should be free to consume whatever news media they wish?" The motivation for the model I introduce in the next section–and the motivation for this paper–is in part to explore and understand the tension between the values espoused in Columns 1 and 4.

The survey also included a short battery of questions relating to how young people process information and form their beliefs, which I discuss next in the context of the model.

2.3 A Conceptual Framework

This section lays out a stylized statistical model of cross-generation media influence. The purpose of the model is to establish a simple conceptual framework with which to evaluate how the shift from a low-choice to a high-choice media environment affects (1) the transmission of preferences from one generation to the next and (2) what I call the agency of young people, or the extent to which they determine their own political views. The model generates a testable prediction, which is that more media choice for parents leads to a stronger

[13] At minimum, it is clear that *something* happened in the 1990's, either in the perceptions of the survey respondents in those cohorts or in their experiences.

correlation of political preferences between parents to children. The key assumptions about behavior are twofold. First, I assume selective exposure on behalf of parents, meaning they choose news outlets that most closely match their own political ideologies. For evidence related to selective exposure, see Iyengar and Kyu (2009), Gentzkow and Shapiro (2011), and Western et al. (2006); for models involving selective exposure, see Mullainathan and Shleifer (2005), Gentzkow and Shapiro (2010), and Martin and Yurukoglu (2017).

Second, an implicit assumption in my model is "selection neglect" on behalf of children exposed to partisan news, meaning young people do not take into account that they may be in one-sided information environments in forming their views. Enke (2020), in a paper titled "What You See is All There Is," finds that in a laboratory setting people receiving biased information signals still form beliefs on the basis of what is front of them, even when told that the signals are disproportionately coming from one side of a distribution. To assess the plausibility of this assumption in my setting, I included survey questions asking, "Some kids (age 0-18) are raised in highly partisan environments, with exposure to partisan parents, communities, and news outlets. Would you say these kids are aware that they are only 'hearing one side' of the political story?" and, "Again consider kids in partisan environments. Would you say kids in these environments are at risk of simply inheriting the beliefs they grow up around, or are they capable of adjusting their beliefs to account for the bias around them?" Answers to the first question were split somewhat evenly, with 51.8% of respondents answering that kids are highly or somewhat aware, and 48.1% answering that kids are only slightly or not at all aware. But on the second question, only 8.1% answered that kids can mostly account for biases (which would correspond to a model of rational or Bayesian belief formation); 49.5% answered that kids can somewhat adjust for biases but are still influenced by partisan environments, and 42.4% said that kids cannot see through biases and will mostly adopt views around them. I proceed with the assumption of selection neglect and note that allowing children to partially "de-bias" would temper the transmission prediction of my simple model.

The key equation relating media influence to political preferences is,

$$P_{it} = \alpha_1 P^*_{it} + \underbrace{\alpha_2 \bar{P}_{i,t-1} + \alpha_3 M_{it} + \epsilon_{it}}_{Environment\ E_{it}}, \ \ where \sum \alpha_j = 1, \tag{2.3.1}$$

with the variables defined as follows:

- P_{it}: Observed political preferences of person i in generation t.
- P^*_{it}: Underlying type of person i (determines views under $E_{it} = 0$).
- $\bar{P}_{i,t-1}$: Average of person i's parents' preferences.

- M_{it}: Average slant of media exposure (e.g. Fox News ≈ 1).
- ϵ_{it}: Ideology of outside-the-home community influences.

Equation 2.3.1 says that person i's political views at time t are some weighted average of their own type P^*_{it}, which is intrinsic to them[14], and of the influence of their parents' $\bar{P}_{i,t-1}$ ($t-1$ denotes the previous generation), media exposure M_{it}, plus an error term ϵ_{it} capturing other outside-the-home factors. Together, these influences–parents, media, and community–comprise the environment $E_i t$. Each variable is centered at zero on a left-to-right political scale, as pictured in Figure 2.4, with more negative values corresponding to more liberal, and more positive values to more conservative.

Next, assume party identification proceeds as follows:

1. Individuals draw a type P^*_{it}, parents $\bar{P}_{i,t-1}$, community ϵ_{it} (all $\sim N(0, \sigma_x^2)$).[15]
2. Parents select media M_{it} closest to $\bar{P}_{i,t-1}$.
3. Individuals form preferences P_{it} based on Equation 2.3.1, and base their party (or vote) on a simple cutoff as in Martin and Yurukoglu (2017):

$$P_{it} > 0 \Rightarrow \textit{Republican}, \;\; P_{it} < 0 \Rightarrow \textit{Democrat}$$

With this framework we can evaluate a shift from a low-choice to a high-choice media regime. Assume that in the low-choice world, only one media outlet characterized by 0 on the left-to-right scale is available; this is intended to model the centrist ABC, CBS, and NBC network newscasts. In this environment, $M_{it} = 0$ for all individuals i, and the low-choice environment E^{LC}_{it} reduces to:

$$E^{LC}_{it}(M_{it}) = \alpha_2 \bar{P}_{i,t-1} + \alpha_3 M_{it} + \epsilon_{it} = \alpha_2 \bar{P}_{i,t-1} + \epsilon_{it} \tag{2.3.2}$$

Assume that in the high-choice world, individuals have available a news outlet on the same position on the ideological scale as themselves. Strictly speaking, this means the set of

[14] Alford et al. (2005), based on studies of twins, estimate that around one-third to one-half of political views are genetically based, but that the environment plays a central role as well: "More importantly, let us not forget that a heritable component of 50% for political ideology... still leaves plenty of opportunity for the environment to alter attitudes and behaviors–and even orientation" (p. 165). One particularly thoughtful respondent to my survey relatedly commented, "I also look at my own family. Three boys, raised by the same two parents. My older brother is extremely liberal. I and my younger brother are extremely conservative. We were all exposed to the same types of media and political opinions, had similar childhood and young adult life experiences, similar interests, and yet we came out on opposite ends of the political spectrum. I actually think there is an inherent, I suppose biological, basis for more of our political beliefs than we might instinctively expect."

[15] In reality these correlated, to start I assume they are independent.

outlets available is $\{-\infty, \infty\}$. In Figure 2.4 above, for exposition, I have sketched Fox News at 1 and MSNBC at -1. For suitably chosen variances σ_x^2 of the variables in the model, most individuals' ideologies will lie in that $[-1, 1]$ range, but the scale in the end in unimportant. What's important is that in the high-choice world, parents can find ideologically consonant news sources. I could restrict news outlets to discrete choices like $\{-1, 0, 1\}$, but this would complicate the framework with little conceptual gain.

Under high-choice, and assuming selective exposure, we have $M_{it} = \bar{P}_{i,t-1}$, so that

$$E_{it}^{HC}(M_{it}) = \alpha_2 \bar{P}_{i,t-1} + \alpha_3 M_{it} + \epsilon_{it} = (\alpha_2 + \alpha_3)\bar{P}_{i,t-1} + \epsilon_{it}. \tag{2.3.3}$$

Compare the low-choice and high-choice environments for children. We have $E_{it}^{LC}(M_{it}) = \alpha_2 \bar{P}_{i,t-1} + \epsilon_{it}$, and $E_{it}^{HC}(M_{it}) = (\alpha_2 + \alpha_3)\bar{P}_{i,t-1} + \epsilon_{it}$. Note that the high-choice world *amplifies parental influence* (the coefficient on parents' views $\bar{P}_{i,t-1}$ increases from α_2 to $\alpha_2 + \alpha_3$, where the α's are positive weights). This is the first key insight of the model. When media choice is low, parents have less lattitude to convey their own views to their children through television news. But when media choice expands, parents can amplify their own views, influencing children not only direclty, but also through the news that features in the home.

We can now examine how the shift from low-choice to high-choice affects preference transmission across generations. Mostly simply, we can ask, how strongly correlated are preferences across generations?[16] Deriving the correlations $\rho(P_{it}, \bar{P}_{i,t-1})$ between parents' and childrens' views in the both the high-choice and low-choice environments yields:

$$\rho_{HC}(P_{it}, \bar{P}_{i,t-1}) = \frac{(\alpha_2 + \alpha_3)\, \sigma_{\bar{P}_{i,t-1}}}{\sigma_{\alpha_1 P_{it}^* + E_{it}^{HC}}} > \frac{\alpha_2\, \sigma_{\bar{P}_{i,t-1}}}{\sigma_{\alpha_1 P_{it}^* + E_{it}^{LC}}} = \rho_{LC}(P_{it}, \bar{P}_{i,t-1}) \tag{2.3.4}$$

The model predicts that more media choice will lead to a higher correlation in political views across parents and their children. In a paper titled, "The Home as Political Fortress," Iyengar et al. (2018) document a secular increase the correlation between parents' and kids' politics. My contribution is to propose that the news media may be strengthening that fortress. To test this hypothesis, I need variation in access to partisan media among parents, for which I use the rollout of Fox News in Section 2.4 below. Before proceeding to the empirics, I use the model to explore one further issue I call agency.

The central tension in values this paper introduces is that between news choice for parents and the opportunity for young people to form their own views in a balanced environment. If all children of Republicans grew up around Republican news and became

[16] One could also ask, conditional on having Republican (or Democratic) parents, what is the probability that a child becomes a Republican or Democrat?

Republicans themselves, rarely or never having been exposed to competing sets of facts, values, and arguments, it would seem they had relatively little opportunity for deliberation and determination themselves. Likewise with children of Democrats. I now lightly formalize this notion in the context of the model, defining agency A in preference formation as follows. I ask, for a given environment E, what is the probability that your party (or vote) will reflect your type P_{it}^*? Mathematically, this looks like:

$$A(E) := \mathcal{P}(Party\, reflects\, type) \tag{2.3.5}$$

$$= \mathcal{P}(P_{it} > 0 | P_{it}^* > 0) \cdot \mathcal{P}(P_{it}^* > 0) + \mathcal{P}(P_{it} < 0 | P_{it}^* < 0) \cdot \mathcal{P}(P_{it}^* < 0) \tag{2.3.6}$$

$$= 1/2 \left(\mathcal{P}(P_{it} > 0 | P_{it}^* > 0) + \mathcal{P}(P_{it} < 0 | P_{it}^* < 0) \right) \tag{2.3.7}$$

This concept of agency has a Rawlsian flavor, as I'm asking *before you know your type*, what are the odds that in a given environment your ultimate political views will reflect that type? Given the assumptions of normality in the model, agency across exposure environments has the bell-curve shape depicted in Figure 2.5, which captures the intuition that in more extreme environments–more partisan parents, more partisan news, etc.–characteristics of the individual are less likely to determine party. Note that the relationship in Figure 2.5 holds independently of the media environment. So to evaluate the shift from low-choice to high-choice media regimes, we need to ask how choice affects the environment E (or its absolute value, which determines A), which we have done above in equations 2.3.2 and 2.3.3.

Using those results, Figure 2.6 shows two cases of expanded media choice. For the first case, in the plot on the left, consider a household with Republican parents ($\bar{P}_{i,t-1} > 0$) in a Republican community ($\epsilon_{it} > 0$). In this case, we have:

$$|E_{HC}| = |(\alpha_2 + \alpha_3)\bar{P}_{i,t-1} + \epsilon_{it}| > |\alpha_2 \bar{P}_{i,t-1} + \epsilon_{it}| = |E_{LC}| \tag{2.3.8}$$

(e.g. Republicans watching Fox vs. Republicans watching ABC)

Here the high-choice amplification of parental views pushes the environment further to the right, reducing the agency–the likelihood that parties reflect individual types–of the children, as illustrated in the left-hand figure. This is the tradeoff at the heart of this paper. Agency is lower in the high-choice environment, which itself sounds like something of a paradox.[17] But there is a second case to consider.

[17] A paradox arising in part from the assumption that children here are passive agents. Arguably, with the advent of social media and the general expansion and fragmentation of the media market, children too have more choice, which would complicate the analysis. This focus of this paper, and the empirical exercise that is feasible, relates only to the market for television news, where it is less likely that young children or adolescents have directly benefited from the rollout of Fox News, which I study in Section 2.4.

Assume instead we have a household with Republican parents ($\bar{P}_{i,t-1} > 0$) in a very liberal community ($\epsilon_{it} << 0$), such that the cumulative, weighted influence of parents and community makes for a left-of-center environment, $E_{it} < 0$. Now it could be the case that

$$|E_{HC}| = |(\alpha_2 + \alpha_3)\bar{P}_{i,t-1} + \epsilon_{it}| < |\alpha_2 \bar{P}_{i,t-1} + \epsilon_{it}| = |E_{LC}|, \tag{2.3.9}$$

where, as illustrated in the right-hand plot in Figure 2.6, more media choice makes for a higher-agency environment. The intuition here is that in an otherwise highly one-sided environment ($\epsilon_{it} << 0$), the introduction of alternative partisan news programs could actually serve as a counterweight. Interestingly, this is precisely how Fox News asks us to understand itself–as a conservative voice in an otherwise liberal-leaning public dialogue. The cumulative effect on agency will therefore depend on the distribution of these cases in the population. In a highly geographically sorted setting, which arguably holds today (see e.g. Bishop (2009) on "The Big Sort"), the former case will predominate.

2.4 Empirical Tests

2.4.1 Data and Design

I follow the design developed in DellaVigna and Kaplan (2007) to estimate how Fox News affected the transmission of political views from parents to children. Specifically, DellaVigna and Kaplan (2007) study a twenty-eight state sample and exploit the fact that Fox News rolled out across different media markets at different times. As shown in Figure 2.7, Fox was launched in 1996 and reached saturation in the U.S. around 2004. The authors digitized publicly available data from the *Television and Cable Factbook* to determine when Fox arrived in each local area. Martin and Yurukoglu (2017), however, later showed that the *Factbook* data contained significant measurement error, and that proprietary data from Nielsen was a more accurate measure Fox's staggered arrival.[18] I therefore use the DellaVigna and Kaplan (2007) design and the Nielsen FOCUS data set from Martin and Yurukoglu (2017). My outcome data is a 2015 YouGov survey that appears in Iyengar et al. (2018). The key features of the data for my purposes are that it features parent-child pairs with zipcode location identifiers; its chief disadvantage is that it is a small sample, with around 1,000 total respondents, or 500 parent-child pairs. The average age of the child in the 2015 survey is 25.12, meaning the typical child was about 10 years old in 2000, the year by which I divide the treatment and control groups.

[18]From the Martin and Yurukoglu (2017) online appendix: "About 40 percent of the control group in DVK is mis-classified as not having cable access to Fox News. About 25 percent already had access in 1998 and hadn't been updated for at least two years in the Factbook" (p. 61); see Table A2 in that appendix.

As in DellaVigna and Kaplan (2007), I divide locations–zipcodes, in my case–into those that did and did not have access to Fox News in 2000, and I wish to test whether children of Republican parents in the earlier arrival Fox zipcodes are more likely to have become Republicans themselves.[19] First, the validity of the design depends on ruling out selection–one might expect that Fox selected first into disproportionately Republican areas. DellaVigna and Kaplan (2007) show that with sufficient controls, this actually was not the case. One intution for that finding is that while a rural area like Idaho may on average be more Republican, and Manhattan more Democratic, it may still have been more profitable to enter the larger market first. Moreover, Fox's arrival depended on negotiations with numerous local cable providers, which also produces some idiosyncratic variation.

Table 2.3 shows results of selection tests for the sample of YouGov zipcodes. Specifically, Panel A regresses an indicator variable for Fox's availability in 2000 on the share of Republican contributions in a zipcode in the 1992 and 1996 presidential elections. These are zipcode-level FEC political contributions data, based on OpenSecrets Bulk Data, as in Martin and Yurukoglu (2017). There is no strong relationship between political contributions and Fox's entry in this sample. The coefficients are small and statistically insignificant. Panel B instead uses Republican vote share, where again no clear selection pattern emerges. Table 2.3 is consistent with the finding in DellaVigna and Kaplan (2007) that Fox did not arrive disproportionately early in disproportionately Republican places.

2.4.2 Results

The main specification I estimate to test for transmission effects is

$$ChildRep = \beta_1 ParRep \; + \; \beta_2 ParRep \, x \, Fox \; + \; \beta_3 Fox \; + \; Controls \; + \; \epsilon, \qquad (2.4.1)$$

where β_2 is the coefficient of interest. Here "ChildRep" is an indicator for whether the child identifies as a Republican, and "ParRep" similarly is an indicator for whether the parent is a Republican. The "Fox" variable equals one if the zipcode of the respondents had Fox News available in 2000 and zero otherwise. The controls include various demographics of the parents and children from YouGov and zipcode characteristics as described in Table 2.4. Panel A of the table shows in the first row that there is a strong relationship between the party of the parent and the party of the child. Having a Republican parent increases the probability that a child becomes a Republican by about 45 percentage points.

[19] Another limitation of the YouGov data is that I do not know the zipcode in which the child grew up; reported is the zipcode of the parent in 2015, which I use as an imperfect proxy to determine potential Fox exposure in the 1990's and early 2000's.

The second row tests whether having had a Republican parent *and* early access to Fox News further increases the likelihood that the child is a Republican. These coefficients do have a positive sign, consistent with the predictions of my model, but they are not statistically significant. One major limitation of using such a small sample is the imprecision of the estimates. For interpretation of the magnitudes, note that there is an average difference in years of potential Fox exposure between the treatment and control groups of about four years. Thus, dividing a coefficient of 0.081 (in the third and fourth columns) by four shows that an additional year of potential Fox exposure is associated with about a two percentage point increase in the likelihood that the child becomes a Republican. Panel B runs the same tests, but within Republican-leaning zipcodes (defined as those with Republican contribution shares exceeding half). Here there are somewhat larger and more precisely estimated β_2 coefficients, translating to an additional year of potential Fox exposure being associated with about a 3.3 percentage point increase the probability that a child of Republicans becomes a Republican.

Lastly, I use the 2013 TargetSmart voter file to test whether these same effects appear in voting behavior (or, more accurately, voter registration behavior). The voter file contains individual-level admininstrative data on voter registration from state databases. Its advantages are that it provides a larger sample, and it is a measure of behavior rather than self-reported beliefs. In thirty-one states, registering to vote includes a prompt for party registration, which is often a requirement to participate in primaries (the conditions vary by state and party). The data do have major limitations for my study, however. Importantly, the voter file does not contain parent and child links. It does contain last names and addresses, so I follow the procedure in Iyengar et al. (2018) to construct predicted parent-child pairs, which are individuals living at the same address, with the same last name, and whose age difference exceeds 18.[20] Clearly this will not catch most parent-child pairs, as those who are not living at the same address or whose last names differ cannot be matched (and some with the same last name at the same address will not in fact be parent-child pairs). Nonetheless, I follow the Iyengar et al. (2018) method to provide a novel test of Fox News' influence on the transmission of party registration across generations.

Table 2.5 reports estimates of Equation 2.4.1 using a fourteen state sample of the 2013 TargetSmart voter file and the Iyengar et al. (2018) matching procedure.[21] Row 1 shows coefficients on the parent's party of comparable size to the Table 2.4 YouGov regression results. In row 2, there is no evidence that more Fox News exposure increased the likelihood

[20]Thanks to Tobias Konitzer for generously sharing code and data.

[21]These are the states for which party registration is available and which also appear in the DellaVigna and Kaplan (2007) sample of twenty-eight states, for comparison, which satisfied the computational constraints of the platform provided to me for analysis of the proprietary TargetSmart data.

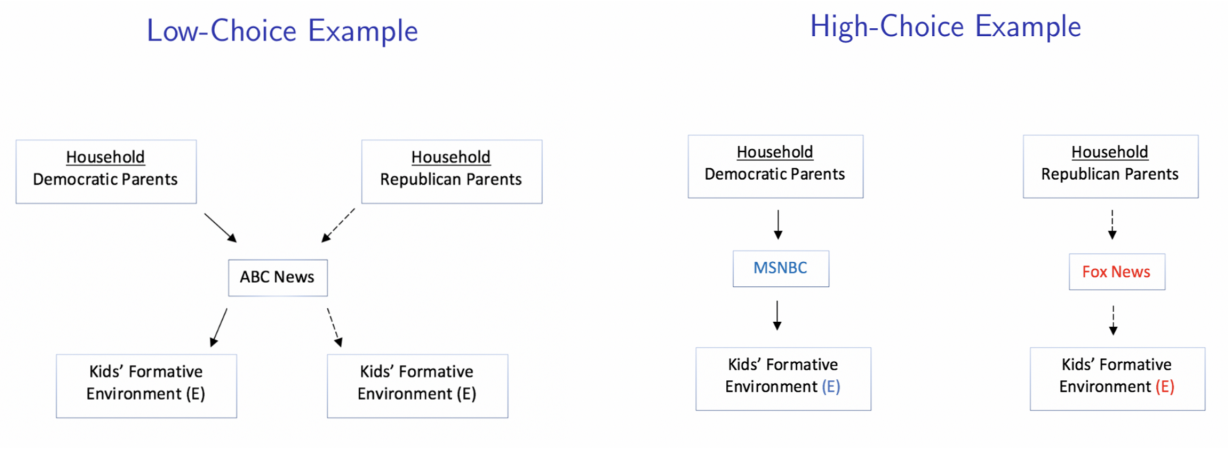
that children of registered Republican parents registered as Republicans themselves (see the Table 2.4 notes for details on controls, which mirror those in DellaVigna and Kaplan (2007)). As discussed briefly above, there are numerous reasons the YouGov survey findings and the TargetSmart registration results could differ, including that one could self-identify as a Republican without actually registering or voting as such, and that the Targetsmart data are limited to predicted parent-child pairs where the child is living at the same address as the parent and thus is not representative. In sum, I take the results presented in this section as offering suggestive evidence of a Fox News leading to a stronger association between self-reported Republican beliefs between parents and their children. But given the data and design limitations, further work would be necessary to confidently conclude that Fox caused Republican parents to more strongly transmit their political views.

2.5 Conclusion

This paper has explored the idea that as media choice expands and people sort into ideologically consonant news sources, children may as a consequence be raised in more limited, partisan information environments. More choice may sound on the surface as though it would expand freedom. But this may not be the case for the generation that succeeds that expansion in choice and is raised in "epistemic bubbles." I have presented three main sets of arguments and evidence, focusing on the shift in television news from network to cable. First, I gathered what is to my knowledge the first survey evidence on young people's exposure to their parents' partisan news. This shows both a sharp increase in that exposure in the 1990's, around the time that Fox News was launched, and also an apparent tension in values–most people say they support free choice in news, while also believing that kids "deserve the opportunity to form their own political beliefs in an environment with exposure to relatively neutral news and different points of view," and that very few young people today in fact have that opportunity. I next developed a model to formalize that opportunity for belief formation as a concept I call agency. The model also generates a testable prediction, which is that more news media choice will strengthen the transmission of political beliefs from parents to children. Testing that prediction with the rollout of Fox News in the 1990's and early 2000's, I find suggestive evidence of a stronger correlation between Republican parents' and children's party in places where Fox arrived earlier, but no such evidence in data on voter registration. I conclude that while the data and empirical design have limitations, and ought therefore to be interpreted with caution, the survey and model offer evidence and ideas worthy of further study as our media and politics appear only to be heading towards further division. One cannot claim that such division is what citizens and consumers have

chosen, if young people were merely conscripted into the fight without the full opportunity to deliberate and determine their beliefs for themselves.

Figure 2.1: Low-Choice vs. High-Choice Media Environments



Notes: The figures are stylized illustrations of the central mechanism of interest in this paper, which is that parents' media choices can influence children's formative environments, and those environments can become more polarized as the menu of media choices expands. The environment is denoted E in the model below.

Figure 2.2: Agency by Analogy to Mobility

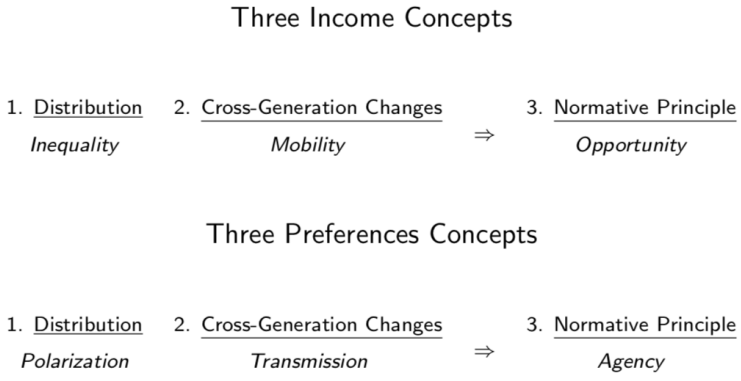
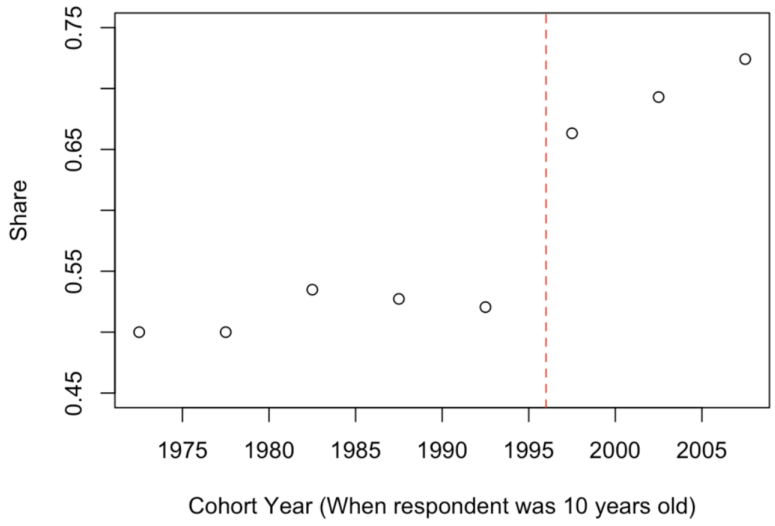


Figure 2.3: Exposure to Parents' Partisan Media Over Time



Notes: The figure shows the share of respondents answering "frequently" or "occasionally" to the question, "Overall, when you were growing up (ages 0-18), were you ever exposed to partisan news media from your parents or guardians?" Responses are shown separately by cohort, defined here as the year in which the respondent was ten years old. The vertical red dashed line indicates the year in which Fox News was launched.

Figure 2.4: Left-to-Right Scale for Modeled Variables

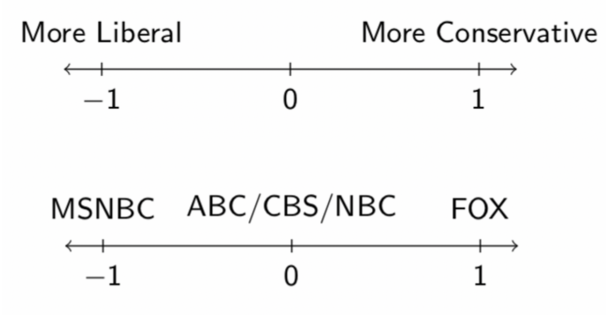


Figure 2.5: Agency Across Environments

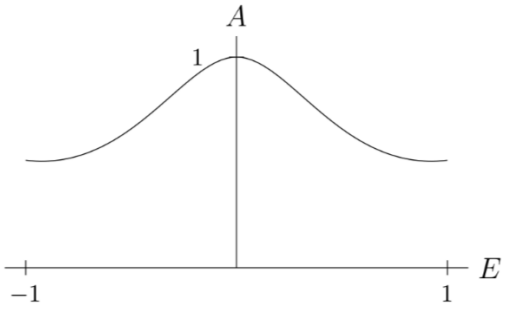
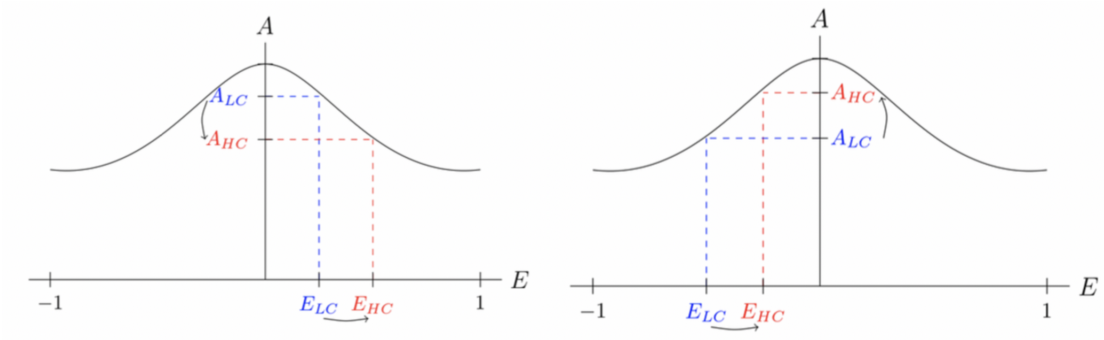


Figure 2.6: Two Cases of Agency and Media Choice



Notes: As described in the text, the figures illustrate two cases of how media choice can affect agency. On the left is a case of Republican parents in a Republican community, and on the right a case of Republican parents in a more liberal community. The latter case sees agency for young people increase as a consequence of increased media choice for parents.

Figure 2.7: Fox Rollout Timeline

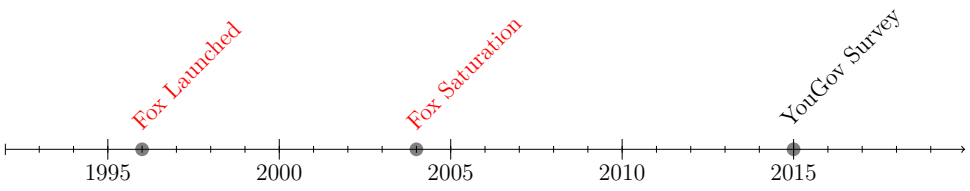


Table 2.1: Survey Summary Statistics

	mTurk Sample	2018 ACS*
Age	35.0	37.9
Male	0.487	0.492
White	0.751	0.755
Black	0.125	0.140
Bachelor's degree	0.689	0.315
	-	-
Republican	0.418	0.390
Trump voter	0.472	0.416
N	519	-

Notes: Ages are medians. *Party benchmark is from the 2016 ANES; "Trump voter" refers to 2020 voting intentions, data from RealClear Politics July 2020 average.

Table 2.2: Share Who Agree with Each Statement

Kids deserve balanced media environment	Kids have that environment	My kids had that environment	People free to choose media
0.900	0.133	0.306	0.906

Notes: The table shows the share of respondents who answered they either "strongly agree" or "agree" to the questions in columns 1 and 4. Columns 2 and 3 report the share who responded "mostly neutral" or "completely neutral" to questions about the political environments in which most kids and their own kids are raised today. See text for additional question details. The sample in Column 3 includes only respondents with children.

Table 2.3: Fox Availability and Pre-Period Political Conditions

	Panel A: Pre-Period Contributions Dep. Variable: Fox Available in 2000			
	(1)	(2)	(3)	(4)
1996 Republican Contribution Share	-0.008	0.029	-0.019	-0.035
	(0.095)	(0.099)	(0.106)	(0.107)
1992 Republican Contribution Share			-0.015	
			(0.014)	
1996-1992 Difference				0.015
				(0.014)
Census Demographics		X	X	X
N (Zip codes)	423	417	390	390
	Panel B: Pre-Period Vote Shares Dep. Variable: Fox Available in 2000			
	(1)	(2)	(3)	(4)
1996 Republican Vote Share	-0.083	-0.067	-0.355	0.020
	(0.199)	(0.230)	(0.548)	(0.276)
1992 Republican Vote Share			0.375	
			(0.650)	
1996-1992 Difference				-0.375
				(0.650)
Census Demographics		X	X	X
N (Zip codes)	456	441	441	441

Notes: The tables show tests for correlations of Fox News entry with political conditions prior to its 1996 launch. These are coefficients from regressions of whether Fox News was available in 2000 in YouGov zipcodes on the listed variables and Census measures of population, income, education, and employment. * sig. at 10 percent, ** sig. at 5 percent.

Table 2.4: Effects of Fox News on Transmission of Political Preferences

	Panel A: Full Sample Outcome: Child Republican			
	(1)	(2)	(3)	(4)
Parent Republican	0.477**	0.445**	0.454**	0.450**
	(0.047)	(0.046)	(0.048)	(0.048)
Parent Republican × Fox 2000	0.108	0.093	0.081	0.081
	(0.070)	(0.069)	(0.070)	(0.070)
Fox News 2000	-0.005	0.002	0.017	0.017
	(0.044)	(0.042)	(0.043)	(0.043)
YouGov Individual Controls		X	X	X
Census Demographic Controls			X	X
1996 Republican Vote Share				X
N (Parent-Child Pairs)	456	456	441	441
	Panel B: Republican Zip Codes Outcome: Child Republican			
	(1)	(2)	(3)	(4)
Parent Republican	0.464**	0.439**	0.461**	0.458**
	(0.053)	(0.052)	(0.054)	(0.054)
Parent Republican × Fox 2000	0.163**	0.143*	0.135*	0.135*
	(0.079)	(0.078)	(0.079)	(0.079)
Fox News 2000	-0.008	0.007	0.019	0.018
	(0.049)	(0.049)	(0.049)	(0.050)
YouGov Individual Controls		X	X	X
Census Demographic Controls			X	X
1996 Republican Vote Share				X
N (Parent-Child Pairs)	361	361	348	348

Notes: The tables shows estimates of Equation 2.4.1 using the YouGov data. Panel A shows full-sample results, Panel B only Republican-leaning zipcodes. Census demographics are zip-code level population, income, education, and employment, and the YouGov controls are similar variables at the individual level. * sig. at 10 percent, ** sig. at 5 percent.

Table 2.5: Effects of Fox News on Transmission of Voter Behavior

	Dep. Var.: Child Registered Republican			
	(1)	(2)	(3)	(4)
Parent Republican	0.513**	0.486**	0.435**	0.431**
	(0.006)	(0.005)	(0.005)	(0.004)
Par. Republican × Fox News 2000	-0.012	-0.014*	0.004	0.005
	(0.010)	(0.008)	(0.007)	(0.007)
Fox News 2000	-0.000	-0.002	-0.012*	-0.011*
	(0.021)	(0.018)	(0.006)	(0.006)
Census and Cable Controls		X	X	X
Census/Cable Controls, House FE			X	X
1996 Republican Vote Share				X
N (Parent-Child Pairs)	661,800	661,770	661,635	661,084

Notes: * sig. at 10 percent, ** sig. at 5 percent; standard errors clustered by local cable company, which is the level of treatment. Cable controls are measures of the numbers of subscribers to each local cable company, and are described in detail in DellaVigna and Kaplan (2007); "House FE" refers to House District fixed effects, which also appear in the DellaVigna and Kaplan (2007) regressions.

CHAPTER III

TV and Labor Supply: Evidence from the U.S. Rollout

> *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]

3.1 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

[1]See Putnam (1998), p. 221.
[2]See Gentzkow (2006), p. 970.

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 3.1 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 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

[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.

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-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

[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.

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

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 3.1), 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 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

[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 (2008b); Baker and George (2010); Campante and Hojman (2013); Thomas (2019); Kim (2020); and Angelucci et al. (2020).

[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.

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 (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.

3.2 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

[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)).

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 function $U(c, \xi(a)l)$. 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:

$$max\ U(m, \xi(a)l) \tag{3.2.1}$$

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

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 3.2 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 $\tilde{a}$:

$$\xi(\tilde{a})w^{1+\epsilon} + \epsilon\xi(\tilde{a})^{-1/\epsilon} - (1 + \epsilon)\left[w - x\right] = 0 \tag{3.2.2}$$

[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.

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 3.2.2 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 3.2 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.

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

[11] There is a knife edge case.

of the parametric assumptions about the utility function.

3.3 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.

3.3.1 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]

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

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

[13] In appendix section C.1.2, we revisit the results in Gentzkow (2006) and Gentzkow and Shapiro (2008b) using the new ITM data.

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 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*

[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..

[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.

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.

Figure 3.3 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.

3.3.2 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

[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.

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 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 C.2 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 C.2 therefore constructs weights and estimates effects on a representative U.S. population. The results are similar, with slightly bigger effects.

[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 C.2.

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 C.2.3.4 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.

3.4 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:

$$E_{aigt} = \gamma_{gt} + \delta_i + \beta_g \cdot TV_{at} + \pi \cdot X_{aigt} + \epsilon_{aigt}, \tag{3.4.1}$$

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.

3.4.1 Results: Social Security Records

Table 3.1 shows estimates of the differences-in-differences specification in equation 3.4.1 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

[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.

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 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 C.2.3.4 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 3.1). 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 C.2.3.1. 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 3.4.1:

$$\Delta E_{iagt} = \alpha_{gt} + \underbrace{\sum_{j=0}^{a} \beta_{g,j}\ \Delta\text{Channels}_{a,t-j}}_{\text{Lagged Stations}} + \underbrace{\sum_{k=1}^{a} \beta_{g,k}\ \Delta\text{Channels}_{a,t+k}}_{\text{Future Stations}} + \Pi X_{iagt} + \Delta\epsilon_{iagt} \quad (3.4.2)$$

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 3.4. 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.

3.4.2 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 3.2, 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 television 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

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

[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.

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.

3.4.3 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 3.3). 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 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.

3.4.4 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.

Television sharply changed the work habits among workers near or above retirement age.

To illustrate this fact, we first allow for treatment effects to differ by age groups. Figure 3.5 splits the sample into ten year age bins and plots the treatment effects by age group bin. The most striking impact is on the oldest workers in the sample. Effects increase sharply once workers get close to retirement age and we do see significant effects on workers over 55 and even starker effect on workers over the retirement age. By contrast, the effect on prime aged workers is markedly smaller and mostly insignificant.

To evaluate the retirement hypothesis more directly, Figure 3.5 disaggregates the overall effects into three possible transition rates. 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 3.5). These retirement effects are also substantially larger than the effects on other labor market flows. Figure 3.5 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.

3.5 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 3.6 shows that the negative employment effect increases as we take the first few stations

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

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 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

[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.

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 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 decline in employment rates among young men. More recent data, however, suggests that screen time among the young is now reaching

[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)

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.

3.6 Conclusion

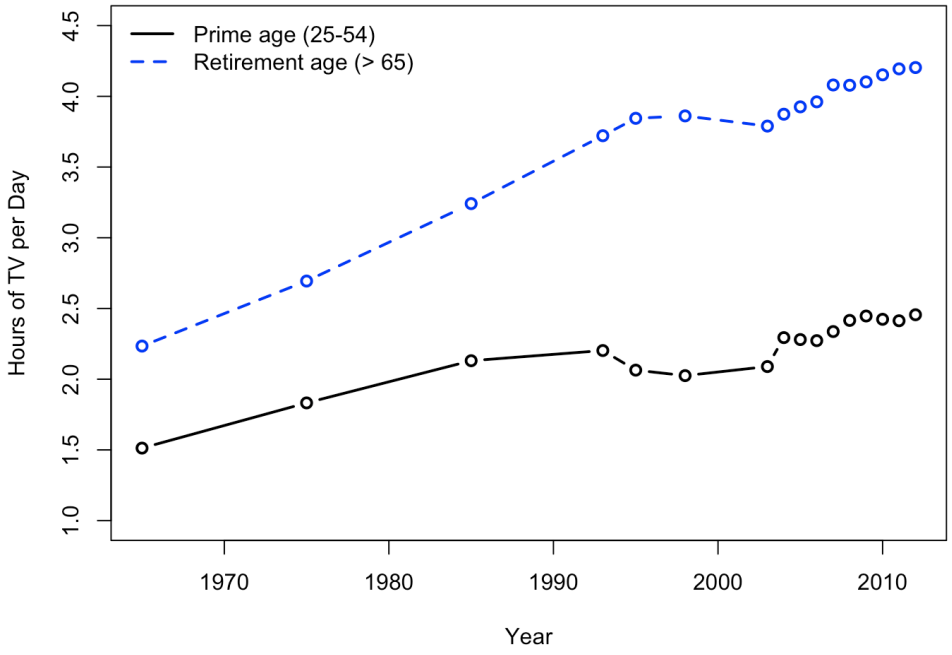
Economists have recently taken an interest in the possibility that entertainment technology may affect work behavior, a hypothesis explored in the context of contemporary video games by Aguiar et al. (2021). All else equal, one would expect an increase in the utility derived from leisure time through 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

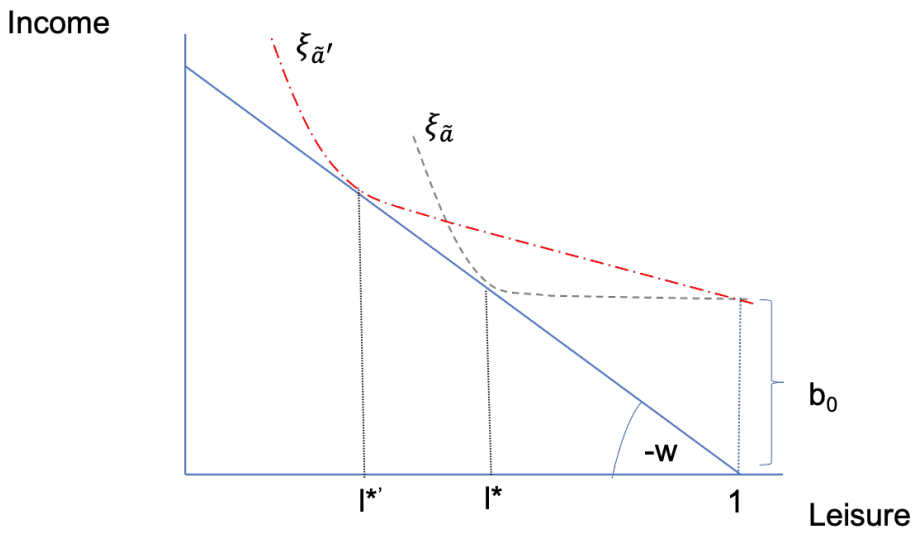
at home? TV improved the outside option for people on the margins of the labor force as it rolled out in the 1940's and 1950's. Given that our findings are from this historical period, one might also ask how relevant they are to understanding labor markets today. The proliferation of ever more compelling 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.

Figure 3.1: 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."

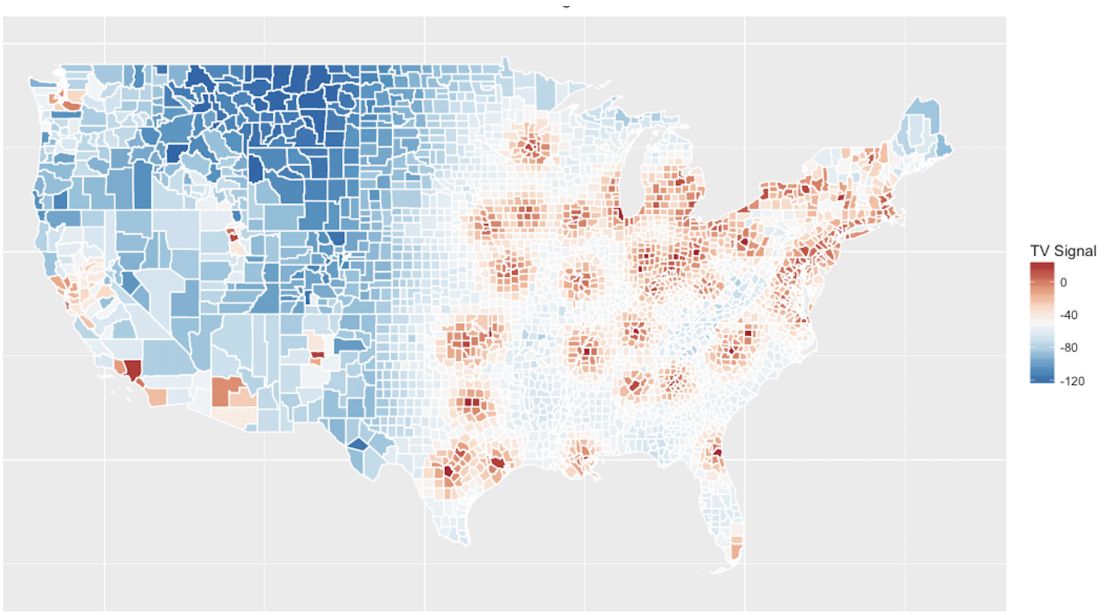
Figure 3.2: Marginal Retiree



Curve.png

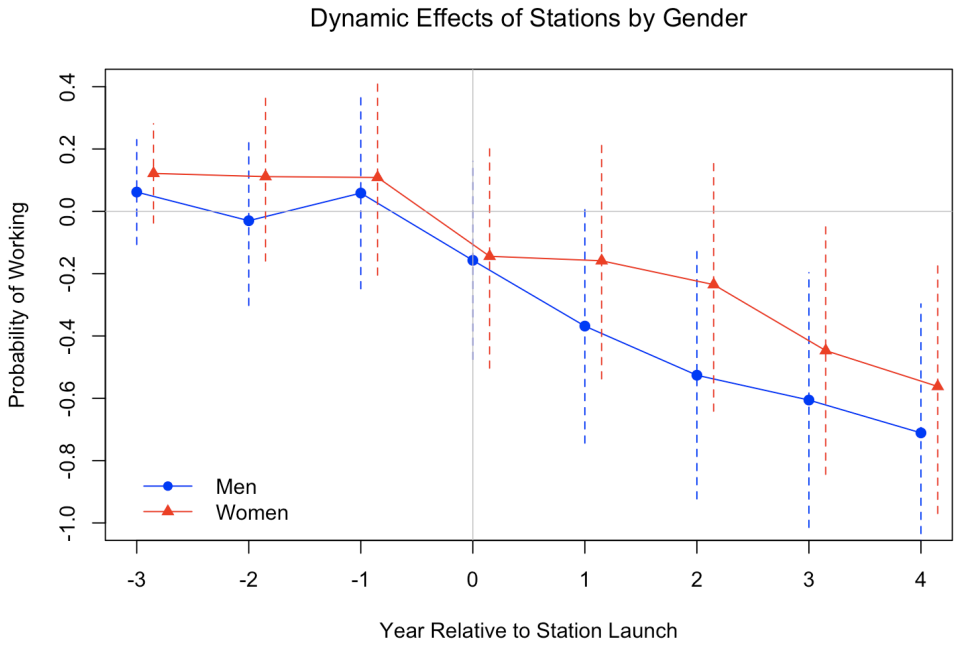
Notes: The figure shows the indifference curve of the marginal retiree, a person who is just indifferent between working and not. The age of the marginal retiree is indicated by $\tilde{a}$. The dashed line is a case with low β_0 and the dash-dot line is a case with higher β_0.

Figure 3.3: ITM-Measured Signal Strength in 1950



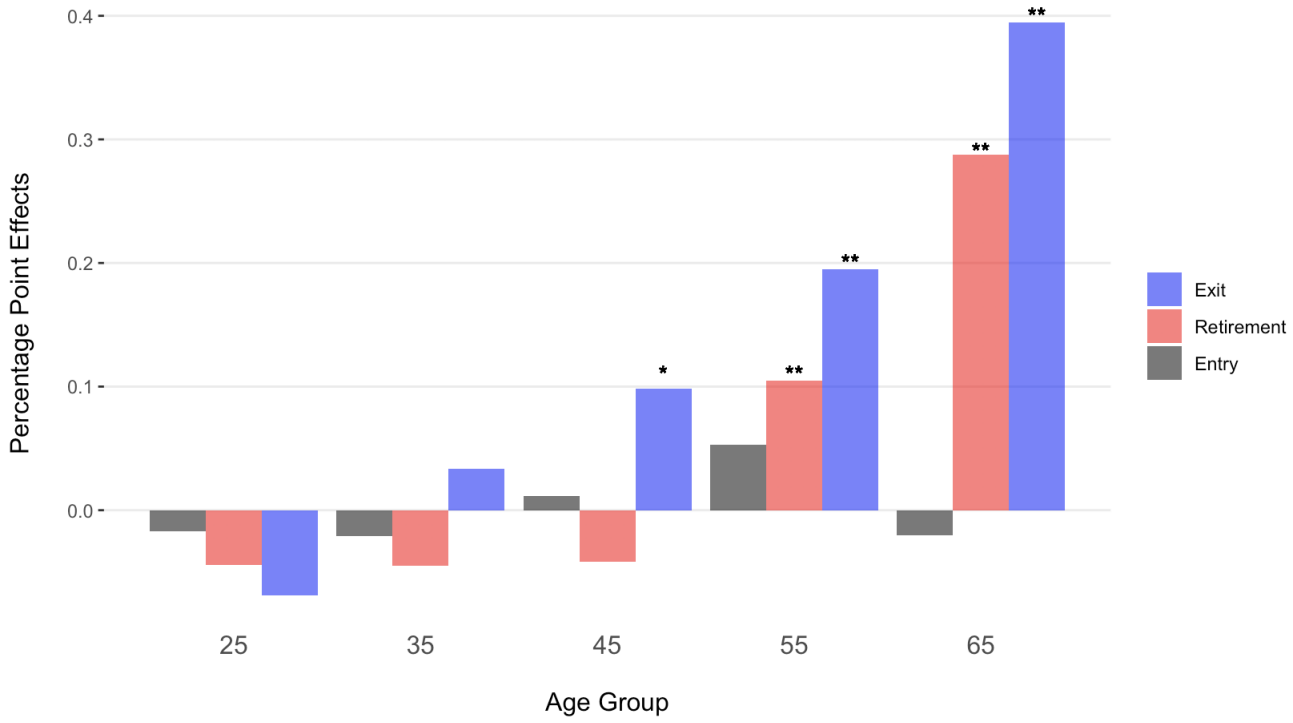
Notes: The figure shows the signal level, in decibels, of the strongest station in each county in 1950, as computed with the ITM. Broadly, counties shaded red had TV access, while counties shaded blue did not; signals whose strength was less than -50 decibels, where the map turns from red to blue, were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

Figure 3.4: Dynamic Effects of Station Launches



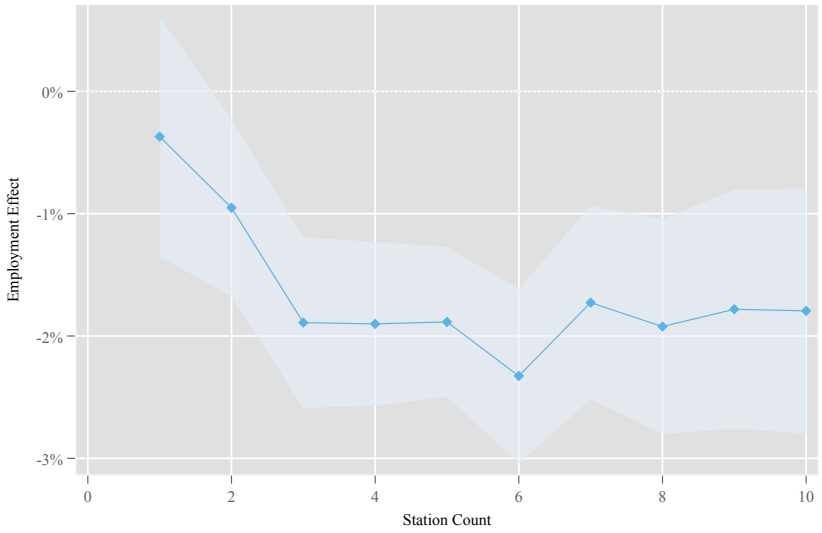
Notes: The figure shows dynamic effects of the launch of a TV station, separately for men and women. Specifically, these are estimated coefficients from individual level regression of equation 3.4.2 and 95 percent confidence intervals. See text for further details.

Figure 3.5: 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 3.1.

Figure 3.6: 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 3.1: 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 3.2: Effects of TV on Employment Using Variation from Regulator Shutdown

	(1)	(2)	(3)	(4)	(5)	(6)
			Interruption Experiment			
	Horse Race		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 3.1. See Table notes for additional details $***p < 0.01, **p < 0.05, *p < 0.1$

Table 3.3: 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. $***p < 0.01, **p < 0.05, *p < 0.1$

APPENDICES

APPENDIX A

Appendix to Chapter 1

A.1 Supplementary tables and figures

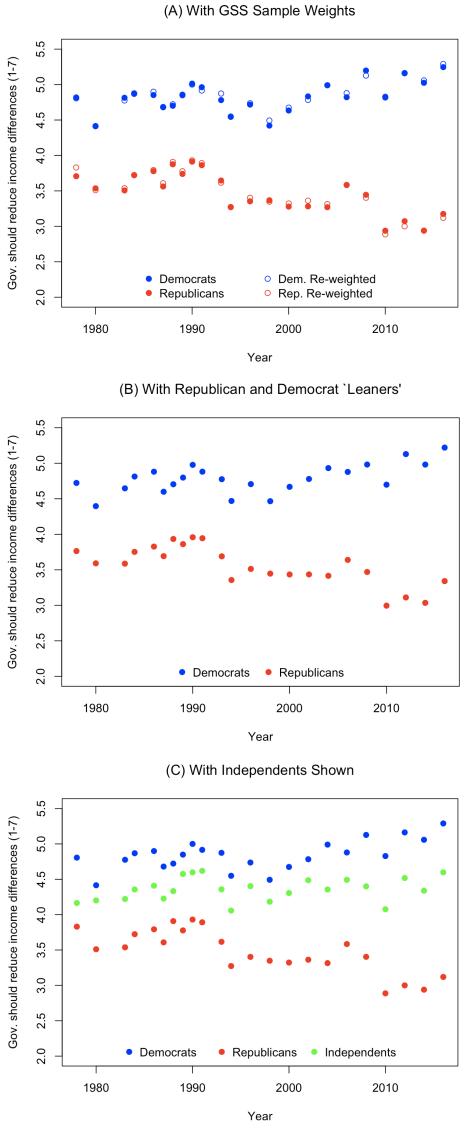
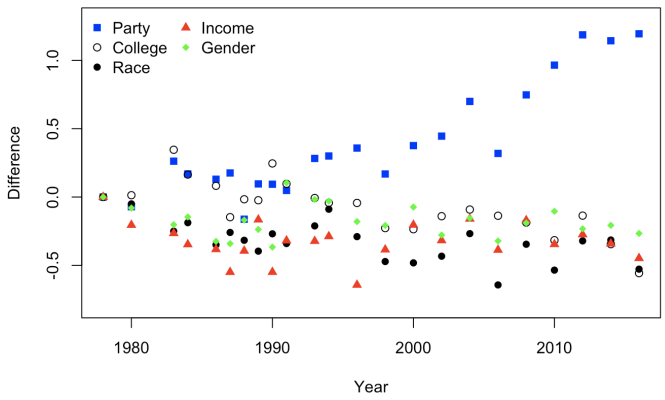


Figure A.1: U.S. Preferences for Redistribution, 1978-2016.

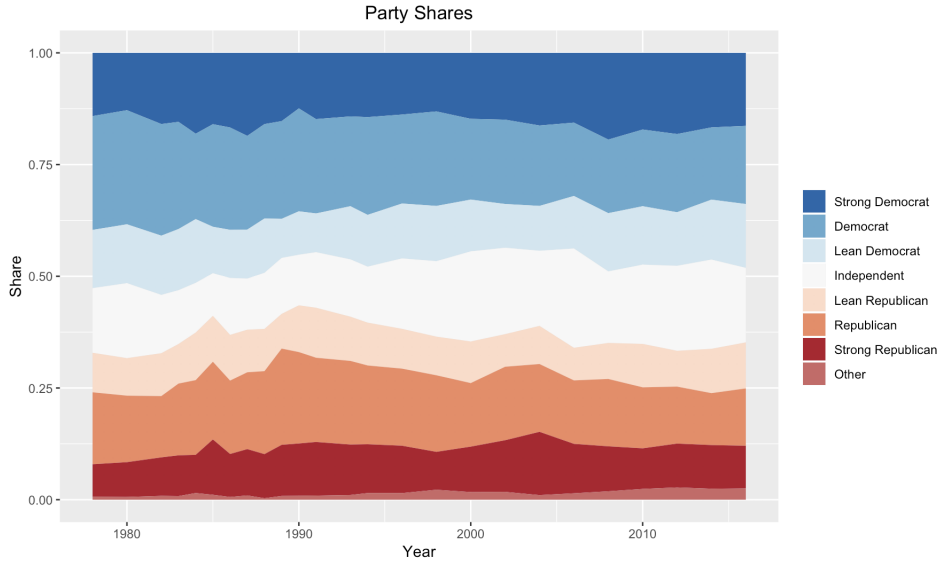
Notes: Each plot is a variant on the Figure 1.1 trends in preferences for redistribution in the GSS. Panel A applies the GSS sample weights *wtssall*. Panel B includes in the categories "Democrat" and "Republican" respondents who said that they leaned towards one of those parties but still considered themselves independents. This compresses the divergence somewhat, relative to Figure 1.1. Panel C includes independents.

Figure A.2: Preference Differences Across Demographic Groups



Notes: This figure shows differences across the indicated groups in GSS redistributive preferences over time. "Party" refers to Democrats versus Republicans; "College" to those with and without a four-year college degree; "Race" to white and non-white; "Income" to below and above average income; and "Gender" to men and women. Differences are normalized to zero in 1978.

Figure A.3: GSS Party Identification Over Time



Notes: The figure shows party identification of GSS respondents from 1978 to 2016 and follows the Gentzkow (2016) figures that show the same stable trends with ANES data on party and ideology.

Table A.1: Party, Income, and Preferences for Redistribution (With Dollar Income)

	Panel A: First and Last Years of Sample				Panel B: First and Second Halves of Sample			
Outcome	Support Redistribution		Republican Party = 1		Support Redistribution		Republican Party = 1	
	1978	2016	1978	2016	1978-1996	1998-2016	1978-1996	1998-2016
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Republican	-0.047	-0.273	-	-	-0.108	-0.176	-	-
	(0.050)	(0.030)			(0.010)	(0.011)		
Democrat	0.088	0.200	-	-	0.081	0.135	-	-
	(0.043)	(0.026)			(0.009)	(0.010)		
Income ($10,000)	-0.025	-0.007	0.007	0.012	-0.022	-0.015	0.016	0.015
	(0.007)	(0.004)	(0.006)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
College Degree	-0.057	0.025	-0.025	-0.018	-0.056	0.008	0.054	0.001
	(0.050)	(0.026)	(0.042)	(0.022)	(0.010)	(0.010)	(0.010)	(0.008)
White	-0.208	0.013	0.153	0.143	-0.111	-0.016	0.201	0.185
	(0.057)	(0.027)	(0.048)	(0.023)	(0.011)	(0.010)	(0.010)	(0.009)
Male	-0.048	-0.038	-0.019	0.013	-0.048	-0.031	-0.002	0.020
	(0.038)	(0.023)	(0.033)	(0.019)	(0.008)	(0.008)	(0.007)	(0.007)
Age	0.001	-0.003	0.003	0.002	-0.002	-0.002	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Independent Mean	0.448	0.519	-	-	0.475	0.473	-	-
Partisan Gap	0.135	0.474	-	-	0.189	0.311	-	-
Observations	696	1,735	696	1,735	15,214	13,315	15,214	13,315

Notes: Data are from the GSS. All regressions are linear probability models. Columns 1 and 2 regress an indicator for support for redistribution (from the *eqwlth* variable) on party affiliation and demographics, with independents as the excluded party category. Columns 3 and 4 regress an indicator equal to one for Republicans on demographics. Columns 5-8 show estimates cutting the sample by the first and second halves rather than the first and last years. Income is in tens of thousands of dollars. Independent mean refers to the average of the outcome variable among independents, and the partisan gap is the difference between the Republican and Democrat coefficients. See text for further details.

Table A.2: Policy Experiment Effects, by Preferred Presidential Candidate

	Top rate	$1M tax	Estate	Min. Wage	Food St.	Scope	Aid	Housing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full sample	2.023	0.0311	-0.0217	0.323	0.313	0.0569	0.133	0.176
	(1.209)	(0.0240)	(0.0258)	(0.0949)	(0.0970)	(0.0613)	(0.0638)	(0.0636)
Control mean	33.79	0.713	0.269	2.546	1.832	3.097	2.559	2.539
Scaled effect	0.140	0.0536	0.0642	0.310	0.153	0.0362	0.0811	0.129
Obs.	1111	1110	1110	806	806	1111	806	806
Obama	0.990	0.00440	-0.0148	0.330	0.226	-0.0112	0.00395	0.117
	(1.570)	(0.0276)	(0.0369)	(0.125)	(0.126)	(0.0753)	(0.0792)	(0.0802)
Control mean	37.31	0.864	0.343	2.716	2.320	3.532	2.960	2.868
Scaled effect	0.0687	0.00760	0.0436	0.316	0.111	0.00711	0.00240	0.0859
Obs.	716	716	716	526	526	716	526	526
Romney	4.186	0.121	-0.0329	0.501	0.772	0.257	0.691	0.441
	(2.742)	(0.0712)	(0.0331)	(0.225)	(0.275)	(0.153)	(0.194)	(0.202)
Control mean	25.39	0.299	0.0556	2.090	0.744	2.181	1.564	1.795
Scaled effect	0.290	0.208	0.0972	0.480	0.378	0.164	0.421	0.323
Obs.	226	226	225	154	154	226	154	154
Other	7.717	-0.0363	-0.0569	0.886	0.725	0.326	0.517	0.472
	(3.748)	(0.0818)	(0.0751)	(0.307)	(0.341)	(0.232)	(0.253)	(0.247)
Control mean	30.80	0.663	0.256	2.433	1.217	2.547	2.183	2.133
Scaled effect	0.536	0.0627	0.168	0.849	0.355	0.208	0.315	0.346
Obs.	169	168	169	126	126	169	126	126

Notes: The table shows results from separate regressions by preferred presidential candidate for the policy experiment in Kuziemko et al. (2015). See Table 1.2 for details on the outcomes.

Table A.3: Table 4 (Omnibus Experiment) from Kuziemko et al. (2015), by Policy Views

	Ineq. v. serious		Ineq. increased		Rich deserving	
	(1)	(2)	(3)	(4)	(5)	(6)
Full sample	0.102	0.104	0.119	0.120	-0.0500	-0.0526
	(0.0154)	(0.0144)	(0.0130)	(0.0128)	(0.0119)	(0.0114)
Control mean	0.285	0.285	0.738	0.738	0.180	0.180
Scaled effect	0.357	0.365	0.539	0.540	0.173	0.182
Covariates?	No	Yes	No	Yes	No	Yes
Observations	3703	3703	3704	3704	3690	3690
Liberals	0.147	0.150	0.0870	0.0829	-0.0454	-0.0486
	(0.0253)	(0.0257)	(0.0166)	(0.0170)	(0.0135)	(0.0137)
Control mean	0.414	0.414	0.836	0.836	0.0964	0.0964
Scaled effect	0.516	0.528	0.392	0.374	0.157	0.168
Covariates?	No	Yes	No	Yes	No	Yes
Observations	1536	1536	1536	1536	1529	1529
Conservatives	0.0399	0.0325	0.151	0.148	-0.0808	-0.0784
	(0.0239)	(0.0243)	(0.0305)	(0.0315)	(0.0323)	(0.0327)
Control mean	0.129	0.129	0.615	0.615	0.386	0.386
Scaled effect	0.140	0.114	0.680	0.668	0.279	0.271
Covariates?	No	Yes	No	Yes	No	Yes
Observations	877	877	878	878	875	875
Moderates	0.0913	0.0885	0.137	0.131	-0.0332	-0.0323
	(0.0249)	(0.0253)	(0.0230)	(0.0235)	(0.0182)	(0.0185)
Control mean	0.238	0.238	0.706	0.706	0.137	0.137
Scaled effect	0.320	0.310	0.617	0.589	0.115	0.111
Covariates?	No	Yes	No	Yes	No	Yes
Observations	1290	1290	1290	1290	1286	1286

Notes: Table 4 from Kuziemko et al. (2015), separate regressions by variable *policyview*.

Table A.4: Table 4 (Omnibus Experiment) from Kuziemko et al. (2015), by Party

	Ineq. v. serious		Ineq. increased		Rich deserving	
	(1)	(2)	(3)	(4)	(5)	(6)
Full sample	0.102	0.104	0.119	0.120	-0.0500	-0.0526
	(0.0154)	(0.0144)	(0.0130)	(0.0128)	(0.0119)	(0.0114)
Control mean	0.285	0.285	0.738	0.738	0.180	0.180
Scaled effect	0.357	0.365	0.539	0.540	0.173	0.182
Covariates?	No	Yes	No	Yes	No	Yes
Observations	3703	3703	3704	3704	3690	3690
Democrat	0.133	0.137	0.0938	0.0938	-0.0539	-0.0588
	(0.0221)	(0.0221)	(0.0155)	(0.0156)	(0.0133)	(0.0134)
Control mean	0.376	0.376	0.812	0.812	0.124	0.124
Scaled effect	0.465	0.480	0.423	0.423	0.186	0.203
Covariates?	No	Yes	No	Yes	No	Yes
Observations	1991	1990	1991	1990	1982	1981
Republican	0.0400	0.0396	0.144	0.144	-0.100	-0.0936
	(0.0213)	(0.0217)	(0.0328)	(0.0335)	(0.0337)	(0.0339)
Control mean	0.0841	0.0841	0.591	0.591	0.395	0.395
Scaled effect	0.140	0.139	0.650	0.648	0.347	0.324
Covariates?	No	Yes	No	Yes	No	Yes
Obsservations	803	801	803	801	800	798
Other	0.0755	0.0905	0.148	0.143	0.0131	0.0112
	(0.0305)	(0.0307)	(0.0268)	(0.0269)	(0.0210)	(0.0216)
Control mean	0.268	0.268	0.712	0.712	0.109	0.109
Scaled effect	0.265	0.317	0.667	0.646	0.0451	0.0386
Covariates?	No	Yes	No	Yes	No	Yes
Observations	914	912	915	913	913	911

Notes: Table 4 from Kuziemko et al. (2015), separate regressions by party.

Table A.5: Table 4 (Omnibus Experiment) from Kuziemko et al. (2015), by Preferred Presidential Candidate

	Ineq. v. serious		Ineq. increased		Rich deserving	
	(1)	(2)	(3)	(4)	(5)	(6)
Full sample	0.102	0.104	0.119	0.120	-0.0500	-0.0526
	(0.0154)	(0.0144)	(0.0130)	(0.0128)	(0.0119)	(0.0114)
Control mean	0.285	0.285	0.738	0.738	0.180	0.180
Scaled effect	0.357	0.365	0.539	0.540	0.173	0.182
Covariates?	No	Yes	No	Yes	No	Yes
Observations	3703	3703	3704	3704	3690	3690
Obama	0.123	0.125	0.0967	0.0963	-0.0505	-0.0524
	(0.0196)	(0.0195)	(0.0145)	(0.0145)	(0.0122)	(0.0122)
Control mean	0.353	0.353	0.793	0.793	0.127	0.127
Scaled effect	0.431	0.437	0.436	0.435	0.175	0.181
Covariates?	No	Yes	No	Yes	No	Yes
Observations	2504	2502	2505	2503	2496	2494
McCain	0.0670	0.0675	0.183	0.190	-0.0934	-0.0916
	(0.0227)	(0.0235)	(0.0314)	(0.0324)	(0.0321)	(0.0328)
Control mean	0.0909	0.0909	0.591	0.591	0.355	0.355
Scaled effect	0.235	0.237	0.825	0.857	0.323	0.317
Covariates?	No	Yes	No	Yes	No	Yes
Observations	830	828	830	828	826	824
Other	0.0707	0.119	0.150	0.172	0.0165	-0.0195
	(0.0468)	(0.0496)	(0.0438)	(0.0454)	(0.0394)	(0.0413)
Control mean	0.245	0.245	0.690	0.690	0.163	0.163
Scaled effect	0.248	0.416	0.675	0.776	0.0569	0.0675
Covariates?	No	Yes	No	Yes	No	Yes
Observations	374	373	374	373	373	372

Notes: Table 4 (Omnibus Experiment) from Kuziemko et al. (2015), separate regressions by preferred presidential candidate.

Table A.6: Table 5 (Omnibus Experiment) from Kuziemko et al. (2015), by Policy Views

	Top rate	$1M tax	Estate	Petition	Min. wage	Trust	Scope	Dem 2012
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full sample	0.931	0.0502	0.357	0.0648	0.0325	-0.0292	0.132	0.0152
	(0.549)	(0.0126)	(0.0140)	(0.0156)	(0.0141)	(0.0115)	(0.0339)	(0.0125)
Control mean	30.21	0.740	0.171	0.234	0.690	0.158	3.076	0.529
Scaled effect	0.0914	0.111	2.043	0.394	0.0995	1.250	0.110	0.0246
Observations	3741	3704	3673	3060	3690	3702	3704	3703
Liberals	0.162	0.0290	0.439	0.0654	0.0417	-0.0447	0.140	0.0375
	(0.859)	(0.0144)	(0.0232)	(0.0272)	(0.0190)	(0.0188)	(0.0478)	(0.0180)
Control mean	34.18	0.904	0.254	0.305	0.822	0.171	3.552	0.800
Scaled effect	0.0159	0.0641	2.515	0.398	0.128	1.917	0.117	0.0607
Observations	1547	1536	1523	1254	1529	1535	1536	1536
Conservatives	1.206	0.0726	0.241	0.0405	-0.00428	-0.0380	0.0830	0.0177
	(1.083)	(0.0331)	(0.0255)	(0.0273)	(0.0330)	(0.0221)	(0.0754)	(0.0223)
Control mean	24.00	0.452	0.0797	0.141	0.496	0.148	2.349	0.182
Scaled effect	0.118	0.161	1.378	0.246	0.0131	1.629	0.0690	0.0286
Observations	888	878	872	700	875	877	878	878
Moderates	1.280	0.0549	0.340	0.0896	0.0350	-0.00786	0.112	-0.0192
	(0.983)	(0.0231)	(0.0242)	(0.0263)	(0.0257)	(0.0199)	(0.0602)	(0.0245)
Control mean	29.74	0.743	0.134	0.215	0.666	0.149	3.007	0.442
Scaled effect	0.126	0.121	1.945	0.545	0.107	0.337	0.0934	0.0310
Observations	1306	1290	1278	1106	1286	1290	1290	1289

Notes: Table 5 (Omnibus Experiment) from Kuziemko et al. (2015), separate regressions by variable *policyview*.

Table A.7: Table 5 (Omnibus Experiment) from Kuziemko et al. (2015), by Party

	Top rate (1)	$1M tax (2)	Estate (3)	Petition (4)	Min. wage (5)	Trust (6)	Scope (7)	Dem 2012 (8)
Full sample	0.931	0.0502	0.357	0.0648	0.0325	-0.0292	0.132	0.0152
	(0.549)	(0.0126)	(0.0140)	(0.0156)	(0.0141)	(0.0115)	(0.0339)	(0.0125)
Control mean	30.21	0.740	0.171	0.234	0.690	0.158	3.076	0.529
Scaled effect	0.0914	0.111	2.043	0.394	0.0995	1.250	0.110	0.0246
Observations	3741	3704	3673	3060	3690	3702	3704	3703
Democrats	1.023	0.0411	0.422	0.0599	0.0527	-0.0457	0.0677	-
	(0.739)	(0.0132)	(0.0203)	(0.0237)	(0.0177)	(0.0175)	(0.0429)	
Control mean	32.87	0.886	0.226	0.314	0.791	0.203	3.548	-
Scaled effect	0.100	0.0909	2.418	0.364	0.161	1.961	0.0563	-
Observations	1990	1990	1973	1632	1981	1989	1990	1990
Republicans	1.561	0.0717	0.196	0.0495	-0.00544	-0.00478	0.180	-
	(1.163)	(0.0356)	(0.0253)	(0.0282)	(0.0352)	(0.0233)	(0.0811)	
Control mean	24.49	0.416	0.0612	0.136	0.473	0.136	2.297	-
Scaled effect	0.153	0.158	1.120	0.301	0.0167	0.205	0.150	-
Observations	801	801	795	664	798	801	801	801
Other	0.424	0.0325	0.331	0.0863	-0.00531	-0.0177	0.207	-
	(1.221)	(0.0297)	(0.0296)	(0.0296)	(0.0310)	(0.0175)	(0.0810)	
Control mean	29.62	0.718	0.153	0.150	0.668	0.0840	2.765	-
Scaled effect	0.0416	0.0718	1.895	0.525	0.0163	0.759	0.172	-
Observations	950	913	905	764	911	912	913	912

Notes: Table 5 from Kuziemko et al. (2015), separate regressions by party.

Table A.8: Table 5 (Omnibus Experiment) from Kuziemko et al. (2015), by Preferred Presidential Candidate

	Top rate (1)	$1M tax (2)	Estate (3)	Petition (4)	Min. wage (5)	Trust (6)	Scope (7)	Dem 2012 (8)
Full sample	0.931	0.0502	0.357	0.0648	0.0325	-0.0292	0.132	0.0152
	(0.549)	(0.0126)	(0.0140)	(0.0156)	(0.0141)	(0.0115)	(0.0339)	(0.0125)
Control mean	30.21	0.740	0.171	0.234	0.690	0.158	3.076	0.529
Scaled effect	0.0914	0.111	2.043	0.394	0.0995	1.250	0.110	0.0246
Observations	3741	3704	3673	3060	3690	3702	3704	3703
Obama	0.862	0.0464	0.397	0.0612	0.0291	-0.0458	0.0996	0.0102
	(0.680)	(0.0135)	(0.0183)	(0.0205)	(0.0165)	(0.0152)	(0.0404)	(0.0174)
Control mean	32.58	0.847	0.222	0.284	0.769	0.193	3.425	0.746
Scaled effect	0.0846	0.103	2.275	0.372	0.0891	1.963	0.0828	0.0166
Observations	2527	2503	2481	2061	2494	2502	2503	2502
McCain	2.093	0.115	0.278	0.0743	0.0462	0.00882	0.256	0.00775
	(1.085)	(0.0347)	(0.0255)	(0.0282)	(0.0350)	(0.0208)	(0.0793)	(0.0199)
Control mean	23.63	0.426	0.0455	0.122	0.478	0.0887	2.199	0.0766
Scaled effect	0.205	0.254	1.593	0.452	0.142	0.378	0.213	0.0125
Observations	836	828	824	690	824	827	828	828
Other	1.936	-0.00719	0.319	0.0815	0.0838	-0.0314	0.155	0.0520
	(2.005)	(0.0519)	(0.0449)	(0.0478)	(0.0526)	(0.0279)	(0.143)	(0.0352)
Control mean	28.54	0.699	0.109	0.150	0.619	0.0787	2.653	0.0880
Scaled effect	0.190	0.0159	1.828	0.495	0.257	1.347	0.129	0.0840
Observations	378	373	368	309	372	373	373	373

Notes: Table 5 from Kuziemko et al. (2015), separate regressions by preferred presidential candidate.

Table A.9: Table 10 (Emotional Experiment) from Kuziemko et al. (2015), by Policy Views

	Ineq. v. serious (1)	Ineq. increased (2)	Poverty v. serious (3)	Min. Wage (4)	Aid poor (5)	Food stamps (6)	Housing (7)	Trust gov. (8)
Full sample	0.0783	0.0410	0.0885	0.0469	0.117	0.177	0.0397	-0.00979
	(0.0292)	(0.0258)	(0.0313)	(0.0989)	(0.0665)	(0.101)	(0.0670)	(0.0211)
Control mean	0.337	0.775	0.296	2.546	2.559	1.832	2.539	0.124
Scaled effect	0.221	0.225	0.257	0.0449	0.0714	0.0866	0.0291	0.0931
Observations	1002	1001	799	799	799	799	799	1002
Liberals	0.112	0.0897	0.132	0.133	0.185	0.504	0.171	-0.0237
	(0.0485)	(0.0332)	(0.0556)	(0.163)	(0.0950)	(0.160)	(0.104)	(0.0334)
Control mean	0.484	0.840	0.423	2.966	3.137	2.491	3.017	0.142
Scaled effect	0.316	0.491	0.384	0.128	0.113	0.247	0.125	0.225
Observations	456	456	349	349	349	349	349	456
Conservatives	0.0144	0.0320	0.0777	0.469	0.289	0.336	0.111	0.00363
	(0.0488)	(0.0689)	(0.0506)	(0.217)	(0.177)	(0.222)	(0.165)	(0.0335)
Control mean	0.130	0.657	0.0787	1.921	1.494	0.449	1.652	0.0370
Scaled effect	0.0405	0.175	0.226	0.450	0.176	0.164	0.0810	0.0345
Observations	230	230	190	190	190	190	190	230
Moderates	0.104	-0.0209	0.0771	0.0284	-0.0787	-0.135	-0.0674	-0.0325
	(0.0566)	(0.0508)	(0.0605)	(0.194)	(0.141)	(0.206)	(0.135)	(0.0461)
Control mean	0.268	0.763	0.274	2.403	2.508	1.895	2.500	0.159
Scaled effect	0.293	0.114	0.224	0.0272	0.0479	0.0659	0.0493	0.309
Observations	316	315	260	260	260	260	260	316

Notes: Table 10 from Kuziemko et al. (2015), separate regressions by variable *policyview*.

Table A.10: Table 10 (Emotional Experiment) from Kuziemko et al. (2015), by Preferred Presidential Candidate

	Ineq. v. serious (1)	Ineq. increased (2)	Poverty v. serious (3)	Min. Wage (4)	Aid poor (5)	Food stamps (6)	Housing (7)	Trust gov. (8)
Full sample	0.0783	0.0410	0.0885	0.0469	0.117	0.177	0.0397	-0.00979
	(0.0292)	(0.0258)	(0.0313)	(0.0989)	(0.0665)	(0.101)	(0.0670)	(0.0211)
Control mean	0.337	0.775	0.296	2.546	2.559	1.832	2.539	0.124
Scaled effect	0.221	0.225	0.257	0.0449	0.0714	0.0866	0.0291	0.0931
Observations	1002	1001	799	799	799	799	799	1002
Obama	0.0733	0.0514	0.0988	0.105	0.0573	0.162	0.0590	-0.0281
	(0.0412)	(0.0300)	(0.0444)	(0.134)	(0.0792)	(0.130)	(0.0841)	(0.0305)
Control mean	0.410	0.816	0.352	2.716	2.960	2.320	2.868	0.171
Scaled effect	0.207	0.281	0.287	0.101	0.0349	0.0792	0.0432	0.268
Observations	643	643	507	507	507	507	507	643
Romney	0.173	0.0604	0.214	0.402	0.268	0.133	-0.0800	0.0262
	(0.0508)	(0.0761)	(0.0645)	(0.238)	(0.210)	(0.294)	(0.208)	(0.0408)
Control mean	0.0430	0.624	0.0897	2.090	1.564	0.744	1.795	0.0538
Scaled effect	0.487	0.331	0.622	0.385	0.163	0.0650	0.0586	0.249
Observations	199	199	169	169	169	169	169	199
Other vote	0.122	-0.0611	0.0780	0.0585	0.0806	-0.0285	-0.0880	0.0298
	(0.0956)	(0.0814)	(0.111)	(0.354)	(0.304)	(0.393)	(0.307)	(0.0386)
Control mean	0.390	0.790	0.333	2.433	2.183	1.217	2.133	0.0244
Scaled effect	0.345	0.334	0.227	0.0560	0.0491	0.0139	0.0644	0.283
Observations	160	159	123	123	123	123	123	160

Notes: Table 10 from Kuziemko et al. (2015), separate regressions by preferred presidential candidate.

Table A.11: Table 11 (Policy Experiment) from Kuziemko et al. (2015), by Policy Views

	Ineq. v. serious (1)	Poverty v. serious (2)	Min. Wage (3)	Aid poor (4)	Food stamps (5)	Housing (6)	Private charity (7)	Trust gov. (8)
Full sample	0.0405	-0.00637	0.323	0.133	0.313	0.176	-0.137	-0.0325
	(0.0279)	(0.0272)	(0.0949)	(0.0638)	(0.0970)	(0.0636)	(0.0709)	(0.0207)
Control mean	0.343	0.326	2.546	2.559	1.832	2.539	2.025	0.149
Scaled Policy Effect	0.108	0.0196	0.310	0.0811	0.153	0.129	0.0740	0.654
Obs.	1111	1111	806	806	806	806	1068	1111
Liberals	0.00352	-0.0216	0.232	-0.0409	0.233	0.124	0.0697	-0.0375
	(0.0489)	(0.0479)	(0.147)	(0.0927)	(0.152)	(0.0957)	(0.0800)	(0.0330)
Control mean	0.504	0.447	2.966	3.137	2.491	3.017	1.364	0.165
Scaled Policy Effect	0.00940	0.0664	0.222	0.0249	0.114	0.0911	0.0377	0.754
Observations	509	509	383	383	383	383	483	509
Conservatives	0.0166	0.0233	0.461	0.608	0.881	0.556	-0.636	0.0169
	(0.0438)	(0.0403)	(0.188)	(0.173)	(0.230)	(0.165)	(0.196)	(0.0416)
Control mean	0.129	0.122	1.921	1.494	0.449	1.652	3.210	0.116
Scaled Policy Effect	0.0444	0.0716	0.441	0.370	0.432	0.407	0.344	0.339
Observations	280	280	188	188	188	188	270	280
Moderates	0.0823	-0.0318	0.464	0.134	0.350	0.195	-0.174	-0.0275
	(0.0571)	(0.0552)	(0.207)	(0.143)	(0.199)	(0.129)	(0.150)	(0.0419)
Control mean	0.278	0.312	2.403	2.508	1.895	2.500	2	0.153
Scaled Policy Effect	0.220	0.0978	0.444	0.0813	0.171	0.143	0.0944	0.553
Observations	322	322	235	235	235	235	315	322

Notes: Table 11 from Kuziemko et al. (2015), separate regressions by variable *policyview*.

Table A.12: Table 11 (Policy Experiment) from Kuziemko et al. (2015), by Preferred Presidential Candidate

	Ineq. v. serious (1)	Poverty v. serious (2)	Min. Wage (3)	Aid poor (4)	Food stamps (5)	Housing (6)	Private charity (7)	Trust gov. (8)
Full sample	0.0405	-0.00637	0.323	0.133	0.313	0.176	-0.137	-0.0325
	(0.0279)	(0.0272)	(0.0949)	(0.0638)	(0.0970)	(0.0636)	(0.0709)	(0.0207)
Control mean	0.343	0.326	2.546	2.559	1.832	2.539	2.025	0.149
Scaled effect	0.108	0.0196	0.310	0.0811	0.153	0.129	0.0740	0.654
Observations	1111	1111	806	806	806	806	1068	1111
Obama	0.0437	0.00178	0.330	0.00395	0.226	0.117	-0.0410	-0.0624
	(0.0393)	(0.0387)	(0.125)	(0.0792)	(0.126)	(0.0802)	(0.0737)	(0.0286)
Control mean	0.428	0.391	2.716	2.960	2.320	2.868	1.528	0.189
Scaled effect	0.117	0.00548	0.316	0.00240	0.111	0.0859	0.0222	1.253
Observations	716	716	526	526	526	526	685	716
Romney	0.0376	-0.0468	0.501	0.691	0.772	0.441	-0.483	0.000702
	(0.0365)	(0.0377)	(0.225)	(0.194)	(0.275)	(0.202)	(0.222)	(0.0446)
Control mean	0.0551	0.110	2.090	1.564	0.744	1.795	3.210	0.102
Scaled effect	0.100	0.144	0.480	0.421	0.378	0.323	0.262	0.0141
Observations	226	226	154	154	154	154	219	226
Moderates	0.0823	-0.0318	0.464	0.134	0.350	0.195	-0.174	-0.0275
	(0.0571)	(0.0552)	(0.207)	(0.143)	(0.199)	(0.129)	(0.150)	(0.0419)
Control mean	0.278	0.312	2.403	2.508	1.895	2.500	2	0.153
Scaled effect	0.220	0.0978	0.444	0.0813	0.171	0.143	0.0944	0.553
Observations	322	322	235	235	235	235	315	322

Notes: Table 11 from Kuziemko et al. (2015), separate regressions by preferred presidential candidate.

APPENDIX B

Appendix to Chapter 2

B.1 Survey Appendix

B.1.1 Open Responses

At the end of the survey, respondents received the following open-response prompt: "Do you have any other thoughts or experiences about the news media and young people to share?" Below is the full set of substantive answers.

1	I challenge your interpretation of a fairness doctrine as the government controlling the media. The media would still be allowed to say whatever they like, no censorship. They would just have to allow opposing views equal time. Since even cable and online news sources are somewhat financed by the public, this is a fair exchange, and would lead to much less echo chamber nonsense.
2	I just wish the news was more balanced and less partisan. It results in brainwashing of young minds. Thanks for the HIT! I did my best and hope the data is useful!
3	I think the amount of bias and skewed reporting is extremely dangerous for the younger population that may not have the experience to separate information from propaganda.
4	I think the current state of the "news" in our world is just a sad excuse for propaganda.
5	Seems like a totally "liberal" view in the "mainstream" media and if you disagree, you are labeled racist, or anti-whatever.
6	think and gather information beforeyou decide
7	In my experience I grew up in a republican supporting family. I myself became a republican supporter.

8	It's obviously a little troubling that news today is just shaped by what you want to hear. Algorithms are destroying objectivity.
9	the news lie and the young people that don't know better accept it like sheep.
10	I believe that, too often, children are needlessly influenced by the politics or religion of their parents. To such a degree, that these ideals often become the children's own without any substantive introspection. This almost occurred to me, but fortunately, as I passed through high school, I was finally provided contrasting opinions and perspectives, which allowed be to gradually and completely alter my views in so many integral ways.
11	90 percent of the media is thoroughly orchestrated lies
12	it will be fine to report genuine news
13	All mainstream media in the US is biased. I found that out about 6 years ago, there is a ton of propaganda which is sad because people stay ignorant when they think they understand what's actually going on in the world. I only get news from leftist sites with a history of being objective and providing facts.
14	Only that young people should not feel restricted to their parent's political views.
15	I do want to say that I grew up in the 60's, so we didn't have access to the media like we do now. Also, growing up, my parents were definitely Democrats, but they totally came to their senses later on and switched to Republican. I followed them. :) That's why I said they would have voted for Trump if they were still alive.
16	i like to say watch news media to the young people
17	There are some age and gender differences in the topics teens share on social media. ... posting selfies, videos or other updates of their lives to social media. ... A central conversation surrounding social media and young people is the ... feelings that they have people who will support them through .
18	Back when I was growing up it wasn't at all like it is today, a divided mess.
19	From my own experience, I didn't really question the political beliefs that I heard in my home as a child. I basically just accepted them and "by default" supported the Republican party. It wasn't until I got to college and was out on my own more that I started actually questioning my own beliefs and exploring how I actually felt about issues.
20	0-18 is a really wide range when it comes to awareness of their parents political bias
21	lately they been very once side and not telling the news truthfully. get your news from many sources
22	the media is often toxic, especially right leaning, and families can often instill their same twisted values into their children
23	Young people must be exposed to news media and they must analyze the situation and decide the best for the future of their country

24	I think it's up to each individual to form their own opinion. I do want to stress that I'm fine with bias, but I'm not fine with spreading news that is provably non-factual. My parents, kids and wife can believe whatever they want... They can believe whatever they want, just don't post on Facebook that one of the candidates is secretly a lizard man wearing a human suit. Partisan bias is fine. Spreading imaginary bull#### is not.
25	I would only add that I think it is important you expose children to different views, and allow them to make up their minds. For instance, I have let my daughter watch Fox News and MSNBC so she can see the two sides of partisan belief.
26	TEENAGERS SHOULD USE THE MEDIA WISELY AND CORRECTLY
27	The news media can and has influenced young people. Facebook is full of political jargon. I have a daughter who should be out carrying a "Vote for Trump" sign. She's obsessed.
28	I think young people are exposed to such loaded news that it brainwashes young people into becoming loyalists to an ideology or party, without seeing the other side of the argument.
29	Young people are in future of USA.
30	I do not care about politics, nor do I care what happens in the world around me. Let everyone else deal with that.
31	Because of the internet kids are exposed to all sorts of different viewpoints regardless of their physical social environment.
32	news media keep people update and bring people together with common topic.
33	biased mstly fake
34	I think young people are bombarded with liberal media and education so it can be difficult for them to see what's on the other side. I know this because I think it was like that for me growing up and I didn't change my view on things until I started looking into the right more on my own.
35	media is very important of this situation. media gives lot of information about current and previous situation
36	The young people should aware of political news , which helps them to become a leader or else to select a good leader
37	BE SURE FOR NEW MEDIA AND BE AWARE FOR YOUNG PEOPLE
38	it is important for a person to be a part of a political view
39	I became a republican on the day Ronald Reagan was shot. I was eight years old. Several students in my class cheered when we got the news. I decided that I did not want to be like them.
40	I grew up with my dad wanting to lay down a very fundamentalist Christian foundation for my life; as such we'd listen to Christian radio all the time, which often included conservative political messages, but never actually outright said they were political. At the age of 16 I began learning how to think for myself and I broke out of my Christianity and began viewing all types of media. I was a traditional "Liberal" for a while and now I consider myself a Democratic Socialist.

41	I feel the news media is out of control because I feel they don't report accurate news anymore. I feel this shouldn't be aloud. They should only report the facts and leave their opinion out of it.
42	It is a much different news climate now than when I was younger.
43	it is more like a business today
44	Just because your family grew up a certain way does not mean you have to agree with it.
45	I mostly worry about the racist elements that come into play with certain conservative politics that kids are exposed to. It's kind of terrifying to see whole generations of people pretty much indoctrinated into a very concrete and negative way of thinking.
46	There isn't news today it is all one sided
47	daily news are very important every people
48	I wish the media was more biased but they have become very one-sided and I think they will lie in order to get a "good story". The media no longer has integrity.
49	I don't really know what party I stand for. I don't let my son watch the news because it's to violent and he is 7.
50	the news media is so incredibly biased towards the democratic party and liberal idealism that the republican party will probably be dead by the time the next generation grows up unless an effort is made to continue to spread the beliefs our country was built upon
51	yeah, i have thought that the media was so powerful weapon in the world. it can be change the past and the future
52	I think it's important for children to have their questions answered with responses from multiple different perspectives and this can be tough to do with media in it's current state.
53	i think young people may not be as interested as older people.
54	It is very difficult for young people right now since so much information is overwhelming and it is difficult to know the truth.
55	in this day and age it's not really "there's good people on both sides" - the right wing is flat out dangerous so i don't think these questions really apply negatively if the kid grows up in a moderate/left-leaning household
56	More experience to see the news media, to gain knowledge.
57	Usually it was only during disasters (like 9/11, plane crashes, etc) where the television was on a news media station when I was younger. That was when I was exposed to news more.
58	please analyse the news right or wrong
59	My children often inform me what in going on in the news because they get so much of it in social media. I talk with them about being careful of getting facts not falsity
60	Everything these days is one sided.
61	Unfortunately it is very difficult to find a neutral source of news in today's environment. The local news probably comes the closest to filling this role.
62	everytime share sports news and political news

63	Most of the news media on TV is liberal so of course if that's all they are exposed to, it will influence them.
64	young people are pre-occupied with other things now...
65	Today it seems that young people have a huge desire to "fit in" and go with the majority opinion of their group.
66	The news media is always on-sided.
67	propaganda works and is highly effective and what the msm is spewing today is pure leftist propaganda
68	I think all youngsters should see news and know what happening in the country
69	They watch and make their own opinions based on their experience
70	I do think the news today is a lot more biased/partisan than when I was growing up so that it has a stronger affect on influencing young peoples opinions and beliefs.
71	The family joke while growing up is that my parents would cancel out one another's vote. My sons are much more conservative than I am. In fact, they are closer to being libertarians. Go figure.
72	There is so much content out there that it has to be hard to escape an echo chamber of news and opinions.
73	I don't really know how early young people tend to start becoming interested in politics. I grew up with parents who were news junkies and differed in their political opinions. That got me interested early on and gave me different points of view. It seems to me that most kids grow up in a more partisan environment, but I see them frequently switching their views once they get into college or move out on their own.
74	I think this is an important topic for discussion and it's one that does concern me, especially as a conservative. However, despite everyone's intuitive belief that the media has this incredible influence on people's beliefs, I'm not sure reality bears that out. Look at the 2016 election. If you used all media as a barometer for who should win–I'm not talking about predictions, but rather the tenor of reporting on each candidate–Clinton should have won easily. The mainstream media is clearly left-leaning. And yet we haven't had a 50-year run of Democratic domination in the White House or Congress. (Well, for a while Congress was like that.) I also look at my own family. Three boys, raised by the same two parents. My older brother is extremely liberal. I and my younger brother are extremely conservative. We were all exposed to the same types of media and political opinions, had similar childhood and young adult life experiences, similar interests, and yet we came out on opposite ends of the political spectrum. I actually think there is an inherent, I suppose biological, basis for more of our political beliefs than we might instinctively expect.
75	Young people need to experience things for themselves before forming political views. They have tendency to adapt to the views of those around them from my observations, and with the way cable news and the internet work, tehy just seek out supporting information to correlate with how they feel.
76	News media is very important to develop our social knowledge.

77	My mother was a very liberal democrat, but when she turned about 75, she went to the extreme opposite and became a very conservative republican. I've been very struck and curious about that. She passed away at 82.
78	The rely too much on reading news on social media.
79	The world I grew up in is vastly different from today's world. Cable TV was in its infancy and my parents did not have it in our home. There was no such thing as social media. The only daily news we got was the newspaper and 30 minuts of national news and 30 minutes of local news. And although my parents were both registered Democrats their ideology was closer to that of Republicans; they were very conservative in their beliefs. I was in my late adolescence and early teens during the Civil Rights movement which seemed to cause the paradigm shift in parties. At 18 I registered to vote as a Democrat but by then I was very liberal in my thinking and this caused tension and arguments with my parents when we watched the nightly news together and saw the Vietnam Protests and Civil Rights protests. I supported the protesters and the efforts of Martin Luther King and others while my parents saw it as trouble makiers and instigators who should be prevented from overturning the apple cart and destroying the status quo. The only time we agreed politically was when we all voted for Jimmy Carter for President.
80	Young people today have a lot more opportunity to find their own news sources online and do not have to rely on what their parents bring into the house.
81	news papers and media was very interesting
82	when i was a child there were only I believe two news channels and they were not on all day, probably only once or twice
83	young people are more matured now which is good for the society
84	Other than parents, from my experience, my friends, work colleagues and some celebrities had also play some parts in shaping my political beliefs.
85	I think televised news only provides one side to a story, and the children watching don't understand that. They also don't know how or don't care to seek out the other side, so they get convinced at a very early age as to what their beliefs should be.
86	I think that there should be a news outlet that in completely neutral and that kids need to research all political parties and what they stand for in order to make an informed decision themselves.
87	We need a strong news media, especially in these times. Good luck with your research.
88	There is no longer any journalistic integrity in most of the news media, which is a shame, and they should be ashamed of themselves. It seems like more so than ever integrity is low on the priority list, and the focus is on getting viewers to tune in or click on their site etc.
89	I think kids absorb and internalize their parents' opinion. Especially if those opinions are strongly held.
90	This should get exposed to both sides.

91	My parent primarily focused on the local news, didn't show interest or focus on politics. Most of the political discussions we had were based on decision that affected us based on race.
92	I was born in 1954, so there was no internet. What first got me interested in the news was Woodstock, when I was 13. From then on, I was pretty curious about what was going on, and they became difficult times by the time I was a teen. (Kent State, Vietnam, etc.)
93	The media has never in my lifetime been so openly biased and partisan. If it doesn't change, it will contribute to a civil war, with each side believing in their absolute truth and the absolute stupidity and evil of the other side.
94	I think young people should strive towards forming their own opinions
95	A central conversation surrounding social media and young people
96	News media are very helpful to learn some social things to make children to learn very easyly and its very nice
97	I think it depends on what types of media they see and how old they are when they see it and agree with the issues that they start to form their own opinion.
98	Both views should be presented (non-partisan) so young people can make a True Informed choice.
99	The media wasn't overtly biased or divurgent when I was a kid. People had to be deeply partisan to even find the heavily partisan things like mother jones or national review, and now those sources are relatively moderate compared to Breitbart and Common Dreams
100	I just think its best to ease into news and politics at your own pace.
101	The news media is very biased and dishonest. You have to sift through what they are trying to tell you and figure out the truths for yourself first. Once you do that you have to make up your own mind on what your unique opinions are regarding different issues.
102	The experience of news media is good
103	I remember growing up, my parents didn't talk a lot about politics and as a result I didn't know much. However, my friends did have those parents and they always had their parents political views. I think when someone grows up, what determines what views they take is based on whether they think rationally or not.
104	Sharing the news in media helps to gain knowledge.
105	each and every child should be toiught to read newspaper
106	I think for the most parts kids take in the influences from their environments.
107	I find a lot of news sources very biased, it's hard to know who to believe.
108	I believe that every young person must form his own position on politics as soon as possible.
109	Social Media is used a lot by the younger generation.
110	We are in a time of crisis with our news media and our country.
111	I grew up in basically a cultlike environment and have gotten free but am still impacted by the experience to this day

112	Mom was involved in politics when I was young to help the schools get better funding. It was good for me to learn from that.
113	I feel it's both easier & harder to get both sides or opinions on a subject. News is much more entertainment driven/hit driven & seems less concerned with neutrally presenting facts. Everything feels much more op-ed but presented as fact & you are painted negatively if you choose to present a different side/opinion. Kids want to be liked & want to belong so it is easier just to believe what is the popular view

B.1.2 Survey Instruments

The survey instruments and response options are listed below (no responses listed indicates an open response question). Survey logic is not shown here; e.g. a respondent with no children would not see questions asking about children. After Section A on demographics, a brief section surveyed respondents on their family structure when growing up, which Section B then reflected. For instance, a respondent who said they grew up with a father and mother would be asked about both, a respondent who grew up with only a father (or other male guardian) would be asked only about the father, and so on. Language allowed for same-sex parents as well. For brevity, Section B below lists only the questions as asked regarding fathers. Questions for mothers or female guardians were otherwise identical. The survey is available here: `https://umich.qualtrics.com/jfe/form/SV_72hLOEJK7rRYj3f`.

Section A: Demographics

A1: What is your age?

A2: Please indicate your marital status. [Single (1), Married (2), Other (3)]

A3: What is your gender? [Male (1), Female (2)]

A4: How would you describe your ethnicity/race? [European American/White (1), African American/Black (2), Hispanic/Latino (3), Asian/Asian American (4), Other (5)]

A5: Do you have children? [Yes (1), No (2)]

A6: In which zip code do you currently live?

A7: Which category best describes your highest level of education? [Eighth grade or less (1), Some high school (2), High school degree / GED (3), Some college (4), 2-year college

degree (5), 4-year college degree (6), Master's degree (7), Doctoral degree (8), Professional degree (JD, MD, MBA) (9)]

A8: Did you grow up in the United States? Specifically, is the U.S. where you spent the most time from ages 0-18? [Yes, I grew up in the U.S. (1), No, I grew up outside the U.S. (2)]

A9: In which zipcode did you grow up? (Where did you spend the most time from ages 0-18?)

A10: What was your TOTAL household income, before taxes, last year? [$0 - $9,999 (1), $10,000 - $ 14,999 (2), $15,000 - $19,999 (3), $20,000 - $29,999 (4), $30,000 - $39,999 (5), $40,000 - $49,999 (6), $50,000 - $69,999 (7), $70,000 - $89,999 (8), $90,000 - $109,999 (9), $110,000 - $149,999 (10), $150,000 - $199,999 (11), $200,000 or more (12)]

A11: What is your current employment status? [Full-time employee (1), Part-time employee (2), Self-employed or small business owner (3), Unemployed and looking for work (4), Student (5), Not in labor force (not working and not looking for work, including retired, full-time parent, etc.) (6)]

A12: Has the COVID-19 pandemic affected your employment status? [Yes, my hours were reduced because of the pandemic. (1), Yes, I was laid off because of the pandemic. (2), Minimally or not at all. (3)]

Section B: Role of Respondents' Parents or Guardians

Prompt: We would like to learn about the main household adult influences in your childhood (up through age 18). Whether these adults were biological parents, step parents, or other guardians is not important for our study, and we will refer to primary male guardians as "fathers" and female guardians as "mothers" in the next few questions.

B1: When you were growing up, how interested was your father (or main male guardian) in information about what was going on in government and politics? [Extremely interested (1) Very interested (2), Moderately interested (3), Slightly interested (4), Not interested at all (5)]

B2: About how often did your father watch, read, or listen to news about what was going in government and politics? [Daily (1), A few times a week (2), A few times a month (3), Almost never (4), Never (5)]

B3: In general, which types of news media did your father consume? Select all that apply. [Broadcast television (like ABC, CBS, etc.) (1), Cable television (like MSNBC, Fox News, CNN, etc.) (2), Radio (like NPR or talk radio) (3), Newspapers or magazines (like the Wall Street Journal or Newsweek) (4), Internet and social media (like Twitter or Facebook) (5), Podcasts (like the Daily) (6), Other (please describe): (7), None (8)]

B4: Which of the following specific news outlets do you recall your father consuming when you were growing up? Select all that apply. [ABC, CBS, or NBC national newscasts (1), Local TV newscasts (2), CNN (3), MSNBC (4), Fox News (5), NPR or other public radio (6), Conservative talk radio (7), The New York Times (8), The Wall Street Journal (9), Local newspapers (Like the Detroit Free Press or Seattle Times) (10), Facebook or Twitter (11), Other (please describe): (12), None (13)]

B5: How important do you believe your father's news media was in influencing his political beliefs? [Extremely important (1), Very important (2), Moderately important (3), Slightly important (4), Not at all important (5)]

B6: Growing up, about how often did you see, read, or hear your father's news media? Examples include seeing some of a news media program your father was watching on TV, or hearing a radio show he was listening to in the car. [Daily (1), A few times a week (2), A few times a month (3), Almost never (4), Never (5)]

B7: How important do you believe your father's news media was in influencing your political beliefs? [Extremely important (1), Very important (2), Moderately important (3), Slightly important (4), Not at all important (5)]

B8: Some people find it important to pass on their political beliefs to their children, while others do not. How important do you believe it was to your father for you to hold similar political beliefs to him? [Extremely important (1), Very important (2), Moderately

important (3), Slightly important (4), Not at all important (5)]

B9: When you were growing up (ages 0-18), did your father usually think of himself as a Republican, a Democrat, an Independent, or what? [Republican (1), Democrat (2), Independent or Other (3)]

B10: Would he call himself a strong Democrat or a not very strong Democrat? [Strong (1), Not very strong (2)]

B11: Would he call himself a strong Republican or a not very strong Republican? [Strong (1), Not very strong (2)]

B12: Would he think of himself as closer to, or leaning towards, the Republican or Democratic party? [Republican (1), Democratic (2), Neither (3)]

B13: Where did he see himself on the liberal-conservative spectrum? [Very liberal (1), Liberal (2), Moderate (3), Conservative (4), Very Conservative (5)]

B14: Which candidate did he support in the 2016 presidential election? If he could not or did not vote, pick the person you believe he wanted to win at the time. [Hillary Clinton (1), Donald Trump (2), Neither (3)]

B15: Overall, when you were growing up (ages 0-18), were you ever exposed to partisan news media from your parents or guardians? [Yes, frequently (1), Yes, occasionally (2), Rarely (3), No, never (4)]

B16: Were you ever exposed specifically to cable news (like CNN, MSNBC, and Fox News) that your parents or guardians watched at home? [Yes, frequently (1), Yes, occasionally (2), Rarely (3), No, never (4)]

B17: Overall, when you were growing up, how important was your parents' or guardians' news media in shaping the types of political ideas and information you were exposed to? [Extremely important (1), Very important (2), Moderately important (3), Slightly important (4), Not at all important (5)]

B18: How important do you believe your parents' news media was in influencing your own political beliefs? [Extremely important (1), Very important (2), Moderately important

(3), Slightly important (4), Not at all important (5)]

B19: When you were growing up, would you say your parents' or guardians' news media had a partisan perspective, or was closer to being neutral or balanced? [Extremely partisan (1), Very partisan (2), Somewhat partisan (3), Mostly neutral (4), Completely neutral (5)]

B20: How important would you say your parents' political beliefs themselves were in influencing your views? [Extremely important (1), Very important (2), Moderately important (3), Slightly important (4), Not at all important (5)]

B21: Some young people feel a need to "rebel" against their parents' politics, others do not. Was it important to you to rebel against or reject your parents' views? [Extremely important (1), Very important (2), Moderately important (3), Slightly important (4), Not at all important (5)]

B22: When you were growing up, how frequently was politics discussed in your home? [Daily (1), A few times a week (2), A few times a month (3), Almost never (4), Never (5)]

B23: Before proceeding to the next and final set of questions, we want to ask for your feedback about the responses you provided so far. It is vital to our study that we only include responses from people who devoted their full attention to this study. This will not affect in any way the payment you will receive for taking this survey. In your honest opinion, should we use your responses, or should we discard your responses since you did not devote your full attention to the questions so far? [Yes, I have devoted full attention to the questions so far, and I think you should use my responses for your study. (1), No, I have not devoted full attention to the questions so far, and I think you should not use my responses for your study. (2)]

Section C: Respondents' Views and Experiences on Media and Young People Today

C1: Do you agree or disagree that young people (age 0-18) deserve the opportunity to form their own political beliefs in an environment with exposure to relatively neutral news

and different points of view? [Strongly agree (1), Agree (2), Undecided (3), Disagree (4), Strongly disagree (5)]

C2: Overall, would you say most young people today are raised in a political news media environment that is more one-sided and partisan, or more neutral and balanced? [Extremely partisan (1), Very partisan (2), Somewhat partisan (3), Mostly neutral (4), Completely neutral (5)]

C3: In general, do you agree or disagree that people should be free to consume whatever news media they wish? [Strongly agree (1), Agree (2), Undecided (3), Disagree (4), Strongly disagree (5)]

C4: As a response to polarized news media, some have proposed government policies such as a "fairness doctrine" that would require television shows, for example, to air contrasting opinions on controversial issues. Do you agree or disagree that the government should regulate the content of news media programs? [Strongly agree (1), Agree (2), Undecided (3), Disagree (4), Strongly disagree (5)]

C5: About how often do/did your children see, read, or hear your news media? Examples include seeing some of a news media program you were watching on TV, or hearing a radio show you were listening to in the car. [Daily (1), A few times a week (2), A few times a month (3), Almost never (4), Never (5)]

C6: Some people find it important to pass on their political beliefs to their children, while others do not. How important is it to you for your children to hold similar political beliefs to you? [Extremely important (1), Very important (2), Moderately important (3), Slightly important (4), Not at all important (5)]

C7: Overall, how important would you say your news media is/was in shaping the types of political ideas and information your kids are/were exposed to? [Extremely important (1), Very important (2), Moderately important (3), Slightly important (4), Not at all important (5)]

C8: Overall, how important do you believe your news media is/was in influencing your

kids' political beliefs? [Extremely important (1), Very important (2), Moderately important (3), Slightly important (4), Not at all important (5)]

C9: Are your kids mostly closer to being Republicans, Democrats, or what? [Republican (1), Democrat (2), Some of both (3), Neither (4), My kids are too young to identify with a party (5)]

C10: Overall, would you say your children are/were raised in a political news media environment that is/was more one-sided and partisan, or more neutral and balanced? [Extremely partisan (1), Very partisan (2), Somewhat partisan (3), Mostly neutral (4), Completely neutral (5)]

C11: Around what age would say your kids started to make their own choices about news media to consume? (If your children haven't yet, please enter the rough age you believe they will.)

C12: Some young people feel a need to "rebel" against their parents' politics, others do not. Do you sense that it is/was important for your kids to rebel against or reject your views? (If they are too young, please answer for your perception of kids generally today.) [Extremely important (1), Very important (2), Moderately important (3), Somewhat important (4), Not at all important (5)]

C13: Overall, when your kids were growing up (age 0-18), were they ever exposed to partisan news media from you? [Yes, frequently (1), Yes, occasionally (2), Rarely (3), No, never (4)]

C14: Were your kids ever exposed specifically to cable news (like CNN, MSNBC, and Fox News) that you watched at home? [Yes, frequently (1), Yes, occasionally (2), Rarely (3), No, never (4)]

C15: How interested are you in information about what is going on in government and politics? [Extremely interested (1), Very interested (2), Moderately interested (3), Slightly interested (4), Not interested at all (5)]

C16: About how often do you watch, read, or listen to news about what is going in

government and politics? [Daily (1), A few times a week (2), A few times a month (3), Almost never (4), Never (5)]

C17: In general, which types of news media do you consume? Select all that apply. [Broadcast television (like ABC, CBS, etc.) (1), Cable television (like MSNBC, Fox News, CNN, etc.) (2), Radio (like NPR or talk radio) (3), Newspapers or magazines (like the Wall Street Journal or Newsweek) (4), Internet and social media (like Twitter or Facebook) (5), Podcasts (like the Daily) (6), Other (please describe): (7), None (8)]

C18: Which of the following specific news outlets do you consume? Select all that apply. [ABC, CBS, or NBC national newscasts (1), Local TV newscasts (2), CNN (3), MSNBC (4), Fox News (5), NPR or other public radio (6), Conservative talk radio (7), The New York Times (8), The Wall Street Journal (9), Local newspapers (like the Detroit Free Press or Seattle Times) (10), Facebook or Twitter (11), Other (please describe): (12), None (13)]

C19: How important do you believe your news media is in influencing your political beliefs? [Extremely important (1), Very important (2), Moderately important (3), Slightly important (4), Not at all important (5)]

C20: Some kids (age 0-18) are raised in highly partisan environments, with exposure to partisan parents, communities, and news outlets. Would you say these kids are aware that they are only "hearing one side" of the political story? [Highly aware (1), Somewhat aware (2), Slightly aware (3), Not at all aware (5)]

C21: Again consider kids in partisan environments. Would you say kids in these environments are at risk of simply inheriting the beliefs they grow up around, or are they capable of adjusting their beliefs to account for the bias around them? [Kids are not yet able to see through bias, and will mostly adopt views around them. (1), Kids can somewhat account for bias, but will still be influenced by it. (2), Kids can mostly account for biased upbringings in forming their own beliefs. (3)]

C22: Do you usually think of yourself as a Republican, a Democrat, an Independent, or what? [Republican (1), Democrat (2), Independent or Other (3)]

C23: Would you call yourself a strong Democrat or a not very strong Democrat? [Strong (1), Not very strong (2)]

C24: Would you call yourself a strong Republican or a not very strong Republican? [Strong (1), Not very strong (2)]

C25: Do you think of yourself as closer to, or leaning towards, the Republican or Democratic party? [Republican (1), Democratic (2), Neither (3)]

C26: Where do you see yourself on the liberal-conservative spectrum? [Very liberal (1), Liberal (2), Moderate (3), Conservative (4), Very Conservative (5)]

C27: Which candidate did you support in the 2016 presidential election? If you could not or did not vote, pick the person you wanted to win at the time. [Hillary Clinton (1), Donald Trump (2), Neither (3)]

C28: Which candidate will you support in the 2020 presidential election? If you cannot or will not vote, pick the person you want to win. [Joe Biden (1), Donald Trump (2), Neither (3)]

C29: Imagine you had been raised in an environment where your political influences–your parents, community, news media, etc.–had been the opposite views of those you actually experienced. Do you agree or disagree that you would likely have different political beliefs today? [Strongly agree (1), Agree (2), Undecided (3), Disagree (4), Strongly disagree (5)]

C30: Have you changed the political party you support over your life, or have you mostly supported the same party? [I have changed the party I support. (1), Mostly I have supported the same party. (2)]

C31: Around what age would you say you first identified with or supported a political party or candidate?

C32: Around what age would you say you first started to consume news media yourself? This could mean watching TV news, reading a newspaper, etc.

C33: Do you have any other thoughts or experiences about the news media and young people to share?

C34: These are the last two questions and will not affect your payment. We find some people don't take surveys seriously, instead providing insincere or humorous responses. How often do you do this? [Always (1), Often (2), Occasionally (3), Rarely (4), Never (5)]

C35: Have you provided honest and thoughtful responses to this survey? [Yes, completely (1), Yes, mostly (2), Partly (3), No (4)]

APPENDIX C

Appendix to Chapter 3

C.1 Appendix B: Measuring TV Access

C.1.1 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.

C.1.1.1 An Example of DMA Underestimation (A type II error)

Proximal cities confound the DMA approximation of TV access. For example, panel (A) of figure C.5 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.[1]

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 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 (2008b) 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.

C.1.1.2 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 C.5 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

[1]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.

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.

C.1.1.3 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[2]. 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.[3]

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 C.7, 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 average aurul power increased from 11 to 87 kilowatts.[4] The fixed DMAs do not capture shifts in broadcast areas that followed changes

[2]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.

[3]Table C.5 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.

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

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 C.5 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 3.3.1 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.

C.1.2 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 (2008b) 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 effects are about twice as large with the new data.[5]

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:

operate.

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

$$Y_{it} = \alpha_i + \delta_{rt} + \gamma TV_{it} + \beta X_{it} + \epsilon_{it} \tag{C.1}$$

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 C.6 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 (2008b) 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 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:

$$y_{gc} = \beta TV_{gc} + \phi_g W_c + \delta_c + \gamma_g + \epsilon_{gc} \tag{C.2}$$

$$TV_{gc} = \beta_g^0 ADOPT_c + \phi_g^0 W_c + \delta_c^0 + \gamma_g^0 + \epsilon_{gc}^0 \tag{C.3}$$

The main outcome y_{gc} in equation C.2 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 (2008b) instrument for TV_{gc} in equation C.3 with a variable $ADOPT_c$ for the time at which location c adopted TV broadcasts, as measured using the DMA approximation.

Table C.7 reports the main findings from the paper, as well as the first-stage F-statistic

from equation C.3. 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.

C.2 Appendix B: Empirical Appendix

C.2.1 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.[6] Since

[6]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,

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.[7]

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, 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.

C.2.2 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 C.1. 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

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.

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

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 C.3 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

C.2.2.1 Retirement Trends

Retirement rates grew sharply in the 1950's. Figure C.1 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 C.1 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.

C.2.2.2 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.

C.2.3 Robustness Checks

C.2.3.1 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 3.4.1 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.[8] This eases the interpretation of the results, as coefficients then capture the deviation in employment relative to the t-1 period. Table C.2 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

[8]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.

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.

C.2.3.2 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 C.3 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 3.1). If we use weights for the steady-state estimates, we again find consistent results (see Figure C.4). The impact of television increases with the first few stations and then steadies out. The point estimate is a 3% 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.

C.2.3.3 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 C.4, 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.

C.2.3.4 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: $ATT = ITT/\sigma$.

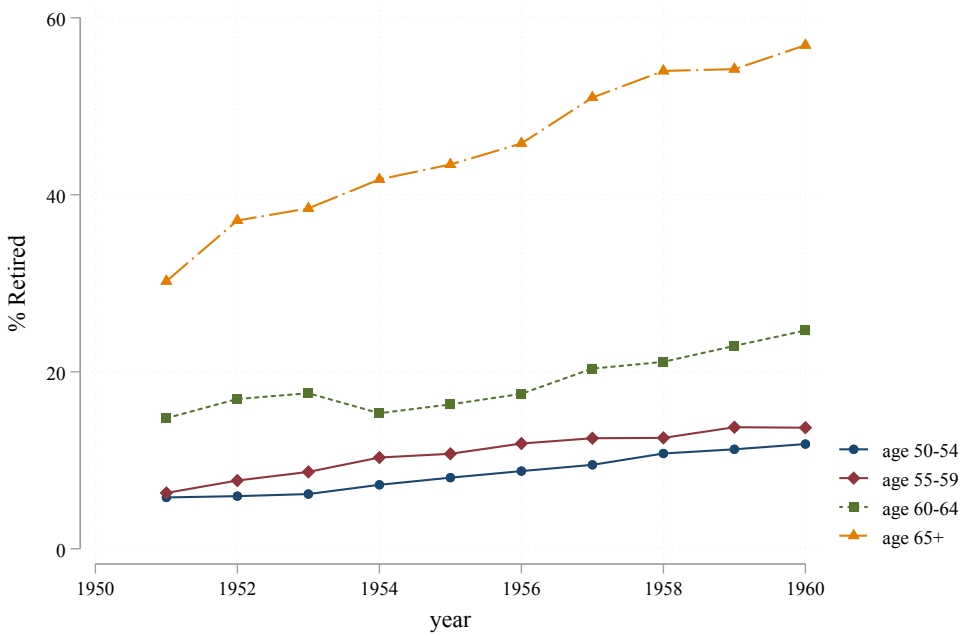
We first calculate the approximate level of non-compliance in our sample (σ). This requires data on migration patterns. The CPS-SSA linked data only includes imperfect information on these rates and we use the matched 1978 CPS migration supplement to estimate migration rates. Many people move every year, 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 C.4 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.

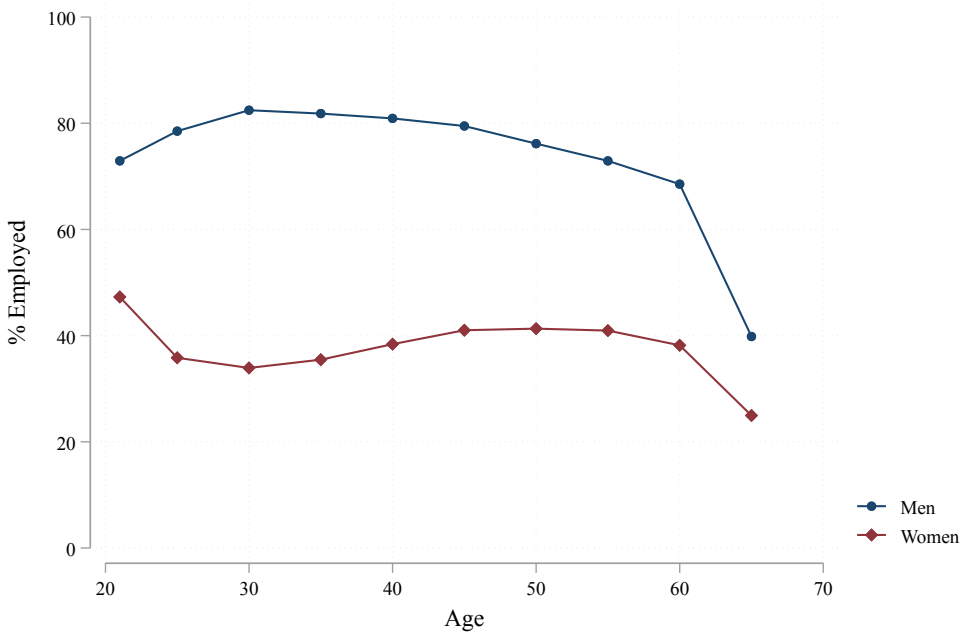
C.3 Appendix Figures and Tables

Figure C.1: Retirement Rates



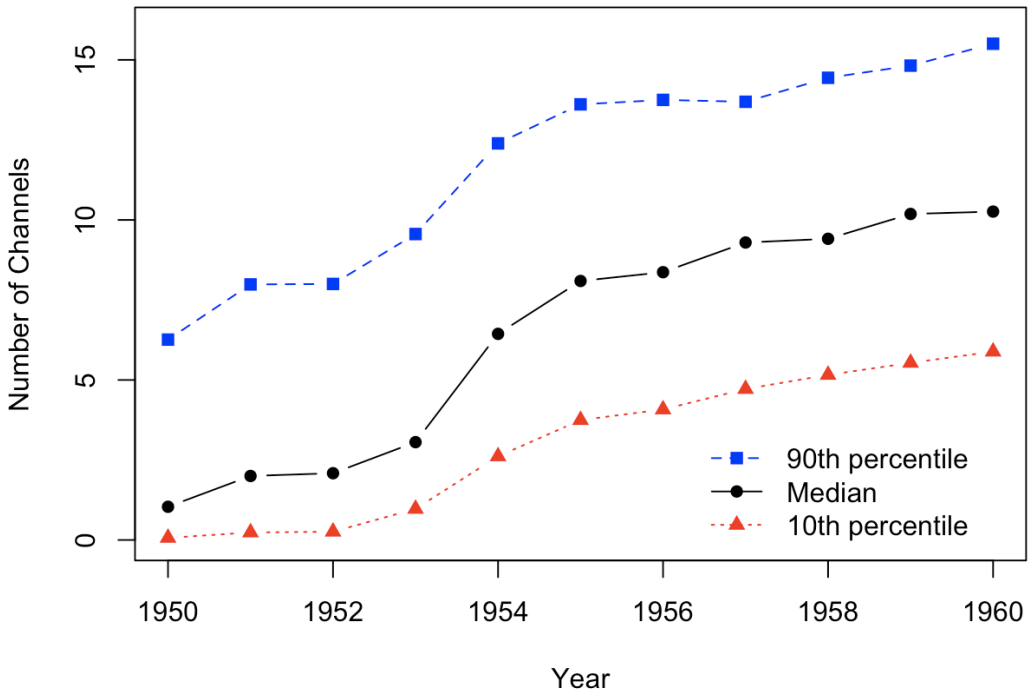
Notes: The figure shows retirement rates among older workers during the 1950's. Retirement is defined as no observed employment in the Social Security records until the end of our sample (1978). Source: linked SSA-CPS data.

Figure C.2: Employment-to-Population Rates over the Life-Cycle



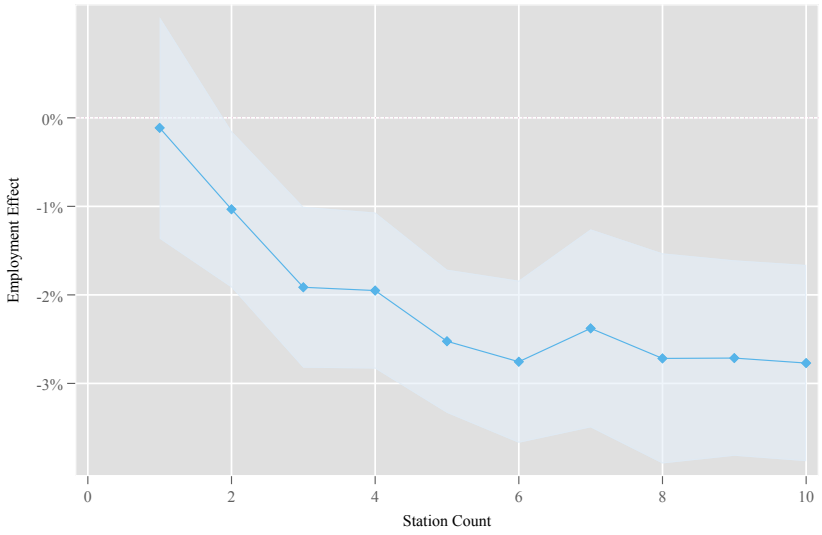
Notes: The figure shows employment rates by age and gender. Each dot shows the average for a five year age window, averaging employment rates over the full sample period. The first and last bins respectively show averages for the age groups 21-24 years and 65+. Source: linked SSA-CPS data.

Figure C.3: Number of Stations Available Over Time



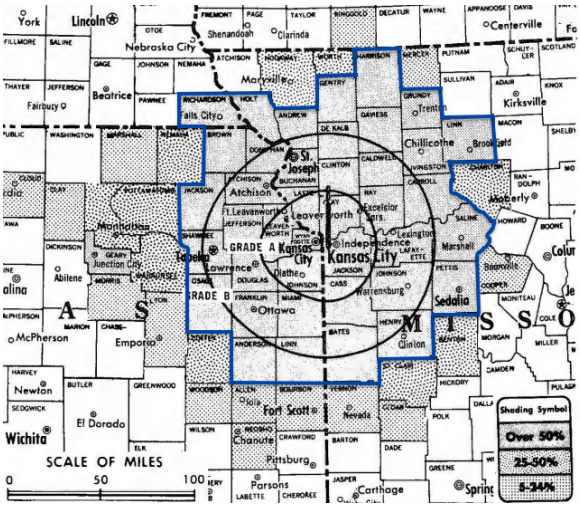
Notes: The figure shows the number of television stations in the U.S. between 1950 and 1960. It shows this for a median person, as well as at the 90th and 10th percentile of the distribution.

Figure C.4: Steady State Effect of TV Accounting for Additional Stations - Weighted Sample

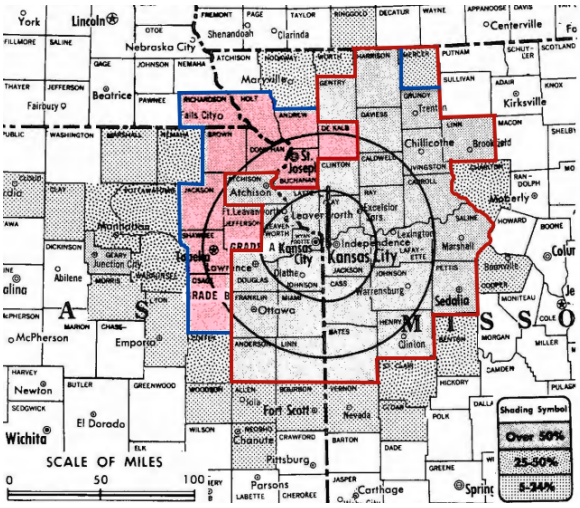


Notes: The figure replicates Figure 3.6 while using sample weights. Weights are constructed to make the sample representative of local population demographics at the annual level.

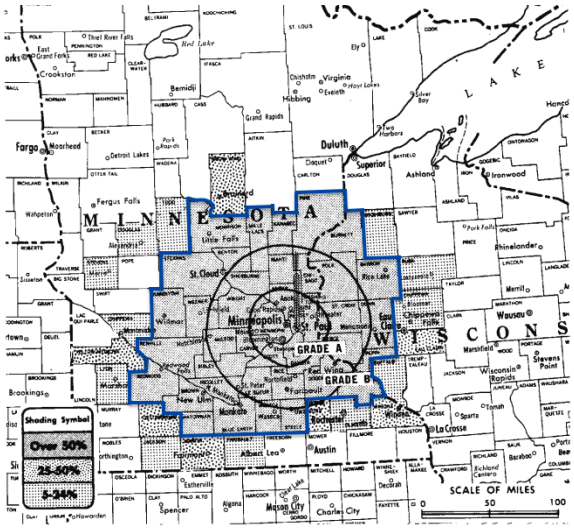
Figure C.5: Coverage Maps and Designated Market Areas



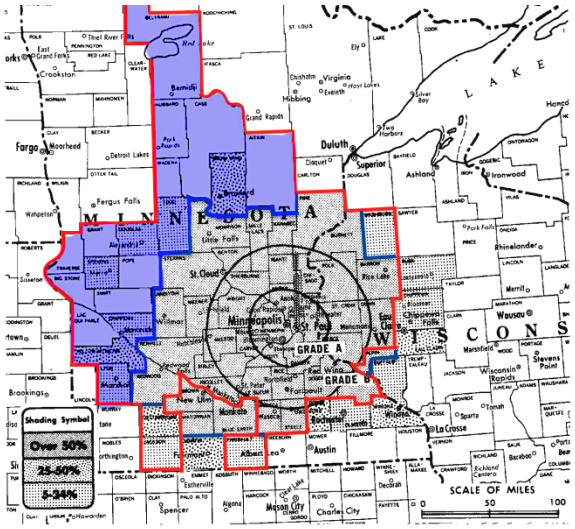
(A) Kansas City coverage map ring (in blue)



(B) Kansas City DMA ring (in red)

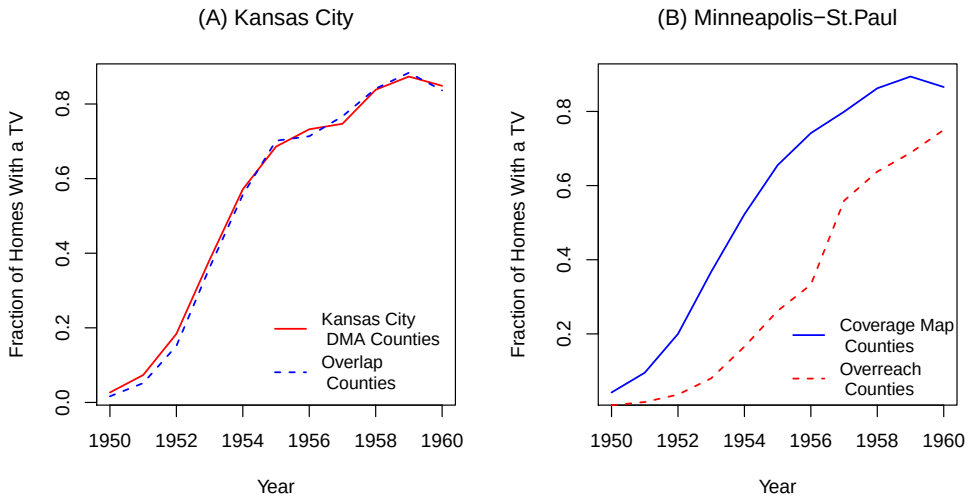


(C) Minneapolis coverage map ring (in blue)



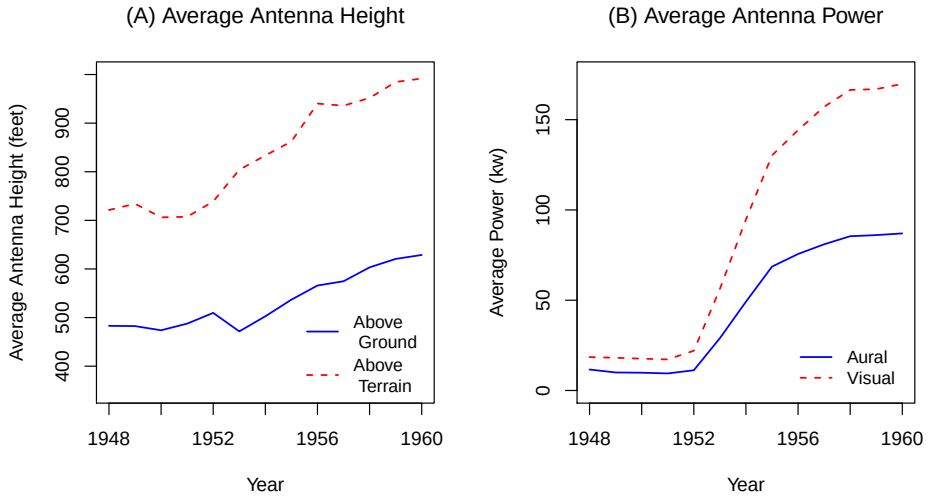
(D) Minneapolis DMA ring (in red)

Figure C.6: 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 C.7: 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 C.1: 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 C.2: 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 × Sex FE	Yes	Yes	Yes	Yes	Region × 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 3.1 for variable definitions and additional specification details. $***p < 0.01, **p < 0.05, *p < 0.1$

Table C.**3:** Individual-level Effects of TV on Employment - Weighted Sample

	(1)	(2)	(3)	(4)	(5)	(6)
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)
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 3.1 and additionally uses sample weights. $***p < 0.01, **p < 0.05, *p < 0.1$

Table C.**4:** 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 × 𝟙(Mobile person)	0.0878			
	(0.141)			
Stations × 𝟙(Age 60+)		-0.584***		
		(0.133)		
Stations × 𝟙(High school dropout)			0.0874*	
			(0.0500)	
Stations × 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 3.1. Mobile: person moved MSA between 1975 and 1976. Robust standard errors in parentheses. $***p < 0.01, **p < 0.05, *p < 0.1$

Table C.5: 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 C.6: 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 C.8 for a plot of the DMA and ITM_{50} coefficients and 90 percent confidence intervals.

Figure C.8: Revisiting TV's Effects on Voter Turnout (Gentzkow, 2006)

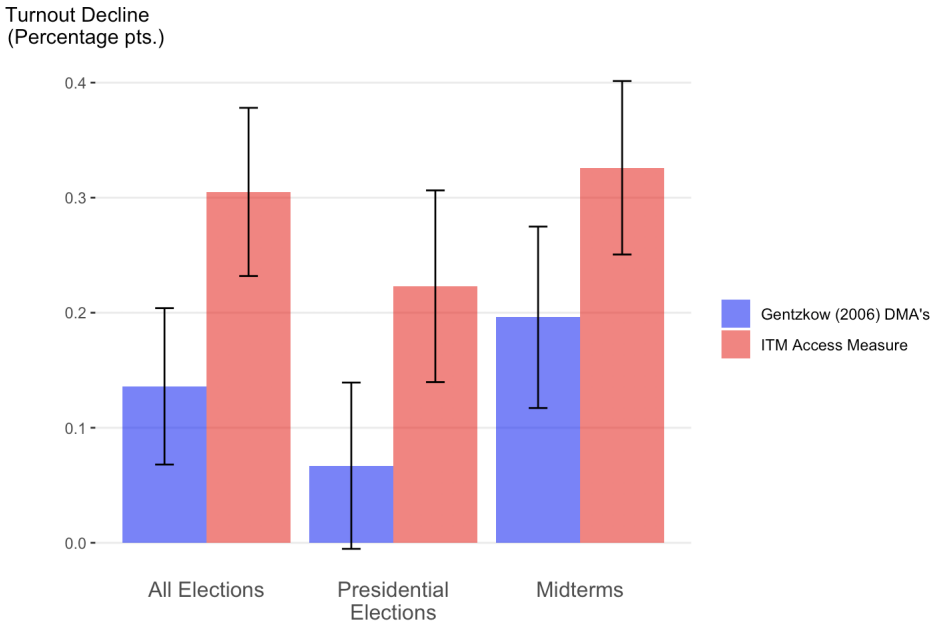
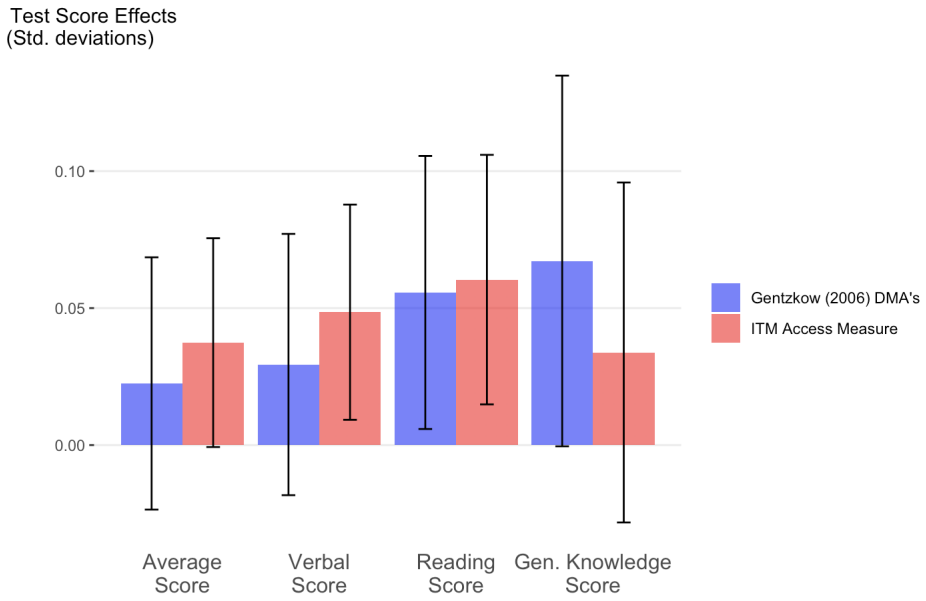


Table C.7: Revisiting TV's Effects on Children's Test Scores (Gentzkow and Shapiro, 2008b)

	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 (2008b) 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 C.9 for a plot of the DMA and ITM_{50} coefficients and 90 percent confidence intervals.

Figure C.9: Revisiting TV's Effects on Children's Test Scores (Gentzkow and Shapiro, 2008b)



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