Media Persuasion and Consumption: Evidence from the Dave Ramsey Show

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Abstract

Can entertaining mass media programs influence individual consumption and savings decisions? I study this question by examining the impact of the *Dave Ramsey Show*, an iconic US radio talk show which encourages people to spend less and save more. To that end, I combine household-level expenditure records from a large scanner panel with fine-grained information about the geographic coverage of the radio show over time. Exploiting the quasi-natural experiment created by the staggered expansion of the radio show from 2004 to 2019, I find that exposure to the radio show decreases monthly household expenditures. This effect is driven by households with initially high expenditures relative to their income. In a mechanism experiment, I document that listening to the radio show has a persistent effect on people's attitudes towards consumption and debt. This suggests that attitudinal changes are a key mechanism driving behavioral change. My findings highlight the potential of entertaining mass media programs for interventions aimed at changing people's financial decisions.

JEL classification: D14, D91, G51, L82, Z13.

Keywords: Consumption, debt, edutainment, household finance, mass media, persuasion, radio.

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1 Introduction

Low savings rates and rising levels of household debt are a major problem in the US and many other countries.[1] Identifying effective policy responses has proven challenging (Gomes et al., 2021), rendering the question of how to promote responsible financial behaviors important. From a policy perspective, entertaining mass media programs may be particularly promising as they can reach a broad audience with a persuasive message. The private market already offers a variety of mass media content providing advice on how to make financial decisions. A relevant example is the *Dave Ramsey Show*—one of the most successful radio talk shows in the US—which argues that Americans spend too much and save too little. But do people act on what they are told? Are they capable of implementing the advice in practice? Or do they stay only for the entertainment?

In this paper, I examine the impact of the *Dave Ramsey Show* to tackle the following question. How and to what extent can entertaining mass media programs change the economic decision of how much to consume? I provide evidence from a quasi-natural experiment created by the staggered expansion of the *Dave Ramsey Show* over a period of 15 years. Using fine-grained variation in the geographic coverage of the radio show's broadcast over time, I document the radio show's impact on individual consumption levels in a large household scanner panel. Moreover, I combine a variety of tools ranging from text analysis of web-scraped audio records to machine learning methods to supplement my empirical analysis. To shed light on the behavioral mechanism, I conduct a tailored experiment examining the effect of the radio show on people's attitudes towards spending and borrowing money.

The *Dave Ramsey Show* is the second most popular radio program in the US with more than 20 million weekly listeners on over 600 affiliated radio stations. Each weekday, Dave Ramsey talks about personal finance and provides credit counseling for three hours. The radio show provides an attractive setting to examine mass media persuasion in the consumption and savings domain for three reasons. First, it explicitly aims to persuade its audience to change their behavior. For over 25 years, the *Dave Ramsey Show* has consistently broadcasted its key message that Americans spend too much and save too little. Dave Ramsey argues that Americans live beyond their means trying to keep up with the Joneses but fail to realize that "the Joneses are broke."[2] In the radio show, debt is portrayed as a symptom of conspicuous consumption and the negative consequences of debt are regularly highlighted.

[1]For example, high household leverage has been linked to macroeconomic instability (Mian et al., 2017) and can impede people's ability to accumulate sufficient savings for retirement (Lusardi et al., 2020). Low savings rates may not always reflect optimal decisions but can instead result from behavioral barriers such as self-control problems (Laibson, 1997; Karlan et al., 2014).

[2]Dave Ramsey, *The Total Money Makeover*

Second, the staggered expansion of the radio show provides a source of quasi-natural variation in exposure to its message across time and space. Beginning in 1996, the radio show expanded to other media markets by licensing its content to local radio stations, averaging about one new station every other week over the next 25 years.

Third, the radio show has never changed its format and has consistently provided the same advice, which Dave Ramsey confirmed in an interview.[3] The effects of exposure to the radio show are thus comparable over time. To support this, I additionally examine the show's topics by analyzing all episodes uploaded on YouTube from 2013–2021. By using web-scraping to obtain the speech-to-text transcripts of these episodes, I circumvent the challenge that radio programs are not systematically recorded. A topic model estimated on a text corpus equivalent to about 3,000 hours of content suggests that the distribution of topics is stable over time.

My empirical strategy exploits the fact that the radio show was introduced in different media markets at different times to assess its impact on consumption. Specifically, I employ a difference-in-differences approach to estimate the causal impact of the radio show on consumption using variation in household-level expenditures before and after the local introduction of the radio show. The main identification assumption underlying this approach is that the timing of the radio show's introduction is unrelated to other factors driving household consumption. Anecdotal evidence from personal interviews with senior executives of the radio show suggests that the timing of the expansion was not driven by strategic considerations. Indeed, I find that the expansion is uncorrelated with baseline observables. As a more demanding test, I examine whether machine learning methods, which excel at uncovering non-linear statistical relationships, can predict the timing of market entry from observables. In a cross-validation exercise, I find that a random forest regression (Breiman, 2001) fails to predict the timing of the expansion from data about local economic conditions and the socioeconomic composition of the local population. Taken together, this evidence alleviates concerns about strategic entry.

To implement my empirical strategy, I combine comprehensive data on, (i), individual consumption and, (ii), the geographic coverage of the radio show's broadcast over time. In particular, I draw on 2004–2019 household-level scanner data from the Nielsen Homescan panel, which includes detailed information on the monthly grocery purchases of a large, geographically dispersed sample of US households. To determine the availability of the radio show, I collect novel data on the timing, technical specifications and geographic locations of its affiliated radio stations. I account for the influence of the topography and physical obstacles on radio signal strength by using a radio signal propagation model (Olken, 2009). This allows me to observe the staggered expansion of the radio show at the zip code-month level and identify when Nielsen households had

[3] Interview with AllAccess (July 6, 2010)

access to the *Dave Ramsey Show*.

I present three main findings. First, my main result is that exposure to the radio show decreases monthly household expenditures. The intent-to-treat effect on households living in areas that receive access to the radio show is a 1.3% decrease in expenditures. An event-study approach examining household expenditures up to twelve months before and after market entry confirms these findings and documents the absence of differences in pre-trends in expenditures, which supports the key identification assumption. Moreover, the event study shows that the impact of the radio show is stable and does not dissipate over the next twelve months. As individual exposure to the radio show is unobserved, I conduct a bounding exercise to better interpret the magnitude of the intent-to-treat effect. This exercise suggests that exposure to the radio show decreases household expenditures by at least 5.4%. From a policy perspective, not only the average effect of the radio show matters but also whether it persuades the intended target population. Examining heterogeneity in effects, I find that the decrease in expenditures is driven by households with initially high pre-exposure expenditures relative to their income, i.e., those who might benefit more from curbing their spending. In contrast, household income alone does not moderate the magnitude of the effect.

Second, I examine *how* households manage to decrease their expenditures. The answer to this question is not obvious because the radio show provides only limited guidance on this topic above and beyond its main advice to rigorously track and budget all household expenditures. In principle, households could choose to purchase less or try to pay less for their current basket of goods. I provide evidence that households decrease their expenditures primarily by decreasing the total number of products purchased. In contrast, I find economically insignificant effects on measures of frugal shopping behavior, such as purchasing products with a large package size or on-sale products that come at a discount.

Third, I study *why* households decrease their expenditures. A large part of the radio show is explicitly aimed at changing people's attitudes towards consumption and debt. A change in fundamental attitudes would explain the stability of the radio show's impact on behavior. I therefore investigate whether the radio show changes people's attitudes. As the observational data is limited to expenditure records, I conduct a pre-registered experiment with a representative sample of 1,500 Americans to address this question. In the main experiment, respondents are randomly assigned to a treatment group that listens to the *Dave Ramsey Show* and a control group that listens to a neutral audio recording. After respondents finish a module designed to obfuscate the study's purpose, I use items from validated scales to measure attitudes towards consumption (Richins and Dawson, 1992) and debt (Davies and Lea, 1995). I find that listening to the *Dave Ramsey Show* for a mere five minutes causes treated respondents to adopt more negative attitudes towards consumption and debt. For example, treated respondents have 24% of a standard deviation more negative attitudes towards conspicuous consumption. A robustness treatment shows that the effects are not driven by the choice of the audio recording used in the control group. Despite the minimalist nature of the intervention, the treatment effects persist for at least a week as confirmed by an obfuscated follow-up survey, thereby allowing me to rule out experimenter demand effects (Haaland et al., forthcoming). A back-of-the-envelope calculation suggests that the change in attitudes may be sufficiently large to explain the magnitude of the decrease in expenditures documented in the scanner data.

My findings also hold under a series of additional robustness checks. For example, I replicate the decrease in expenditures using a more demanding empirical specification that only exploits residual variation in radio signal strength that can be attributed to the influence of physical obstacles on radio signals (Olken, 2009; Armand et al., 2020). I implement this approach by controlling for the hypothetical signal strength that would be achieved in the absence of topographic obstructions in my main specification. Moreover, to alleviate concerns about biases in two-way fixed effects models caused by heterogeneous treatment effects over time (Goodman-Bacon, 2019; Callaway and Sant'Anna, 2020; de Chaisemartin and D'Haultfœuille, 2020), I replicate the event study approach using the imputation estimator proposed by Borusyak et al. (2021).

This paper makes several contributions to the literature. First, I contribute to the household finance literature by demonstrating the potential of mass media programs for behavioral interventions aimed at changing individual financial decisions. Specifically, my evidence from the *Dave Ramsey Show* suggests that repeated messages from mass media channels about the value of savings and the cost of debt can encourage people to decrease their consumption.[4] This suggests that delivering carefully designed messages through mass media could be an attractive complement to other behavioral interventions, such as providing financial education in order to raise financial literacy (Lusardi and Mitchell, 2007; Hastings et al., 2013; Fernandes et al., 2014).[5] This resonates with the findings from a nascent literature studying the effectiveness of edutainment interventions, i.e., a combination of education and entertainment, in developing countries (Ferrara et al., 2012; Coville et al., 2019; Bjorvatn et al., 2020; Banerjee et al., 2020). For example, Berg and Zia (2017) find that financial messages embedded in a South African soap opera encouraged people to borrow from formal banks rather than

[4]My evidence also relates to research on relative consumption motives (Frank, 1985; Abel, 1990; Falk and Knell, 2004; Charles et al., 2009; Heffetz, 2011; Bursztyn et al., 2018). I show that exposure to public messages criticizing the desire to "keep up with the Joneses" can make people less willing to spend.

[5]Alternative approaches to encourage savings include, among others, changes in the choice architecture (Madrian and Shea, 2001; Carroll et al., 2009), peer influence (Beshears et al., 2015), or classical tax incentives (Chetty et al., 2014). See Beshears et al. (2018) for a comprehensive review.

informal sources of credit.[6]

Second, more generally, this paper presents the first causal evidence that mass media programs can affect individual consumption *levels*. I thus contribute to the growing literature studying the social and economic impact of mass media by providing evidence of mass media persuasion in the core economic domain of consumption and savings decisions. Consumption and savings decisions differ conceptually from other domains where media persuasion has previously been documented, such as political behavior (Gentzkow, 2006; Della Vigna and Kaplan, 2007; Enikolopov et al., 2011; Durante and Knight, 2012; Adena et al., 2015; Durante et al., 2019; Wang, 2021), violence and conflict (Dahl and DellaVigna, 2009; Della Vigna et al., 2014; Yanagizawa-Drott, 2014; Armand et al., 2020), or gender norms (Jensen and Oster, 2009; Ferrara et al., 2012; Okuyama, 2019). Moreover, my findings suggest that mass media programs can affect people's materialistic orientation, consistent with the sociological perspective on mass media as a cultural agent of change (Hjarvard, 2008, 2013). While scholars have explored the relationship between mass media and *what* people consume, it has proven challenging to identify a causal effect of mass media messages on *how much* people consume. For example, in a related paper, Bursztyn and Cantoni (2016) carefully evaluate the impact of pre-reunification exposure to Western television on consumption choices in former East Germany. Interestingly, they find that advertisements in Western television affected what consumers purchased, but they find no effect on total expenditures. I thus shed light on the long-suggested influence of mass media on consumption levels (Belk and Pollay, 1985; Richins, 1987).

Third, I provide causal evidence that *non*-advertisement mass media content can influence people's consumption choices. The persuasive influence of mass media on consumer behavior has traditionally been the subject of research in the marketing sciences (see Bagwell, 2007, for a review). However, empirical research on advertisement mainly focuses on the effect on the sales of individual brands and firms rather than total household expenditures, with recent (meta-)studies suggesting that television (Lodish et al., 1995; DellaVigna and Gentzkow, 2010; Shapiro et al., 2021) and digital advertising (Blake et al., 2015; Lewis and Nguyen, 2015) are largely ineffective. Indeed, a key question since Marshall (1919) is whether advertisement is "combative", resembling a tug-of-war between advertisers without affecting total expenditures (Chen et al., 2009). My findings suggest that persuasive communication can, in principle, change total expenditures by shaping people's attitudes towards consumption.

More broadly, this paper relates to the literature studying the impact of charismatic

[6]An important difference to edutainment interventions is that Dave Ramsey *explicitly* encourages people to change their behavior. In contrast, edutainment interventions rely on *implicit* persuasion in the sense that messages aimed at behavioral change are subtly embedded in the respective movie or soap opera, which has been theorized to lower barriers to behavioral change (Banerjee et al., 2020).

individuals (Antonakis et al., 2004; Jones and Olken, 2005; Bassi and Rasul, 2017; Bursztyn et al., 2020; Müller and Schwarz, 2020; Wang, 2021) and recent work on narratives in economics (Akerlof and Snower, 2016; Bénabou et al., 2020; Eliaz and Spiegler, 2020; Shiller, 2020; Schwartzstein and Sunderam, 2021). Dave Ramsey employs narratives of frugality and restraint (Shiller, 2020) and argues against what he perceives as a "consumerist culture." My evidence thus suggests that charismatic media personalities can use stories and narratives to change people's attitudes and behaviors.

2 Background

2.1 The Dave Ramsey Show

The *Dave Ramsey Show*, featuring its host Dave Ramsey, is one of the most successful US radio shows of the past decades and was ranked second place after Sean Hannity on *Talkers Magazine's* list of top radio talk shows in 2021.[7] About 20 million Americans tune in every week and as of 2021, 49% of Americans had heard of the radio show (YouGov, 2021b). Broadcasted from its studio in Nashville, Tennessee, the talk show airs Monday through Friday from 2–5 pm Eastern Time, which is the time of the day when radio consumption peaks.

Message The *Dave Ramsey Show* talks about money, debt, and personal finance, with a focus on helping people to "get out of debt". This distinguishes it from other radio talk shows that—with the exception of only two other major consumer finance shows—exclusively discuss politics, culture, and sports.[8] The radio show has a distinct and consistent message about consumption and debt: Americans live beyond their means trying to keep up with the Joneses, but fail to realize that the Joneses are "broke and living in debt, too." Given this diagnosis, the radio show aims to persuade Americans to reduce their consumption:

> "Financial peace isn't the acquisition of stuff. It's learning to live on less than

[7]Appendix Figure B.2 shows consistently more Google searches for the radio show than for *Hannity*.

[8]In 2020, there were only two other consumer finance radio talk shows among *Talker's Magazine* list of top 100 radio talk shows. The *Ric Edelman Show* provides investment advice and guidance on estate planning. In 2020, the radio show aired on 62 radio stations for two hours each Sunday. The *Clark Howard Show*, which stopped broadcasting in 2020, talks about consumer finances and provides advice on how to "spend less and save more", in particular by avoiding "scams and rip-offs." This radio show mainly provides tips on how to save money by making use of special deals, coupons, or one-off promotions, thus appealing to people who enjoy being frugal. However, it is less geared towards persuading people to change their behavior. Its audience size of 3.5 million weekly listeners is small compared to the *Dave Ramsey Show*, and only 29% of Americans had heard of the radio show in 2021 (YouGov, 2021a). Consumer finance programs on national television, such as *Making Money with Charley Payne*, mostly feature news about the stock market, discuss individual stocks and provide investment advice.

> you make, so you can give money back and have money to invest."
>
> – Dave Ramsey

Debt is consistently portrayed as a symptom of immature behavior, a failure of self-control, and a desire to impress others through conspicuous consumption:

> "It is human nature to want it and want it now; it is also a sign of immaturity. Being willing to delay pleasure for a greater result is a sign of maturity. However, our culture teaches us to live for the now. 'I want it' we scream, and we can get it if we are willing to go into debt. Debt is a means to obtain the 'I want its' before we can afford them."
>
> – Dave Ramsey, *The Total Money Makeover*

The radio show thus uses an economic narrative based on Protestant values of frugality and restraint (Shiller, 2020). Appendix Section E.1 provides further qualitative evidence documenting this narrative and an analysis of Dave Ramsey's rhetoric can be found in Dori-Hacohen (2019).

The radio show additionally makes use of both positive and negative role models to support its main narrative. First, the radio show celebrates people who paid off their debt by having them explain how they achieved this goal before exclaiming: "I'm debt-free!" This ritual, called the debt-free scream, reinforces the idea that having zero debt is socially desirable.[9] Second, the radio show uses negative examples to explain its financial advice on how to cope with debt. Specifically, the main part of the radio show consists of live conversations between Dave Ramsey and people who called the studio line. After describing their financial situation and how debt has negatively affected their relationships or mental health, callers ask Dave Ramsey for advice. These calls reinforce the radio show's philosophy that debt is harmful.[10]

Financial advice The radio show promotes rules of thumb that foster habit formation and focuses less on teaching intricate financial concepts:

> "Winning at money is 80 percent behavior and 20 percent head knowledge. What to do isn't the problem; doing it is. Most of us know what to do, but we just don't do it." – Dave Ramsey, *The Total Money Makeover*

For instance, the radio show recommends the "snowball" method of paying off debt, which involves paying the balances off in order of the smallest to the largest balance.

[9] An example of a debt-free scream can be found here.

[10] It is not unusual for Dave to be angry at the callers, call their behavior "stupid", and provoke them: "When are you going to quit freaking spending money that you don't have?"

While not minimizing total interest paid, immediate successes boost people's motivation (Brown and Lahey, 2015; Kettle et al., 2016). Indeed, past research has shown that simple rules can often be more effective in promoting better financial outcomes (Drexler et al., 2014). Similarly, the radio show advises people to set explicit budgets and plan all of their expenses ahead of each month to preempt overspending. People should then use one paper envelope per budget category and fill them with the corresponding cash amount. In order to become debt-free, the radio show recommends its step-by-step method called the "7 Baby Steps", which starts by saving $1,000 for an emergency fund to pay for unforeseen expenses. People should then apply the debt snowball to their non-mortgage debt before proceeding with the next steps. The show frequently discusses how to implement these steps in practice.

2.2 Program consistency

The radio show has made no major changes to the structure of its daily program, retaining a caller-driven format based on live conversations between Dave Ramsey and callers seeking advice. A key advantage of this setting is that the radio show provides consistent advice over time, which makes the experience of listening to the radio show in different time periods comparable:

> "My advice never changes. My plan works in a good economy and a bad economy because it's all about getting control of your money."
>
> – Dave Ramsey in an interview with AllAccess.com (July 6, 2010)

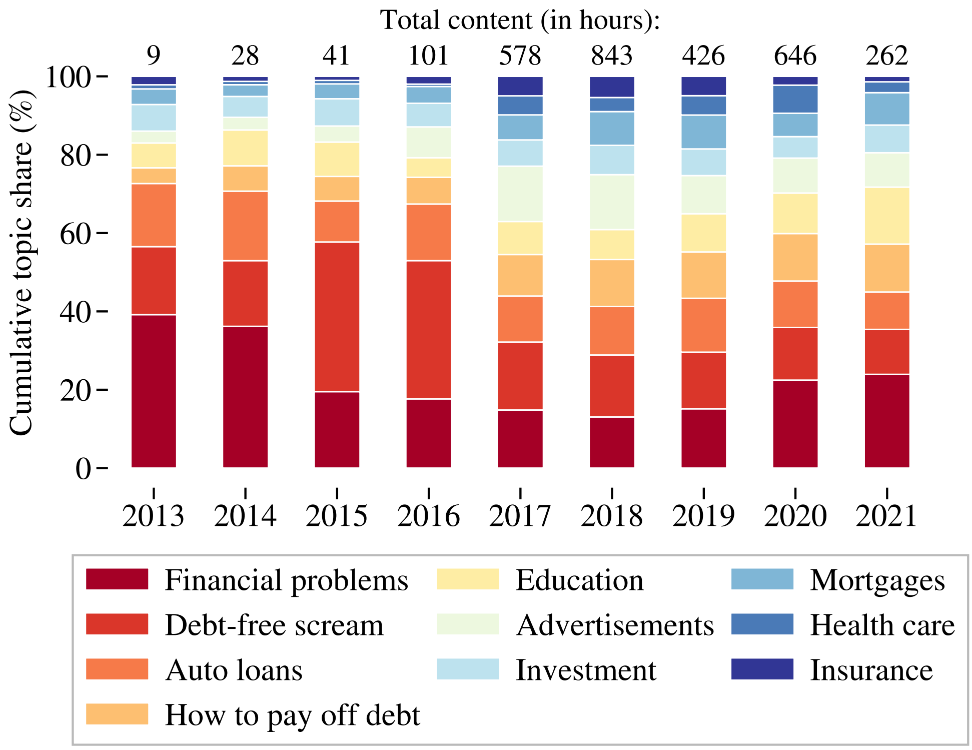
In this section, I provide additional suggestive evidence that the topics of these conversations remained similar over time. As radio talk shows are not systematically recorded (Sweeting, 2015), I obtain content data from YouTube via web-scraping (Kerkhof, 2020). Specifically, I use a Python script to obtain the speech-to-text transcripts and metadata of the 5,587 YouTube videos uploaded by the *Dave Ramsey Show* between August 13, 2013, and May 31, 2021. In total, these videos generated 647 million views and their transcripts capture around 3,000 hours of radio content.[11] As the radio show gradually started to use YouTube more over time, 94% of the data is from 2017 or later.

To shed light on the evolution of the topic distribution of the radio show over time, I use *Latent Dirichlet Allocation* (LDA, see Blei et al., 2003), which is a commonly used technique for topic analysis that aims to extract a fixed number of latent topics from unstructured text data (Gentzkow et al., 2019). To apply this method, I partition the video transcripts into text documents containing the equivalent of five contiguous minutes of speech. I then train a LDA model with ten latent topics on this text corpus. The

[11] I apply a series of common processing steps to the raw text data, such as removing stop words and stemming words, which I discuss in more detail in Appendix Section E.2.

trained model then assigns to each document a probability distribution over topics.[12] Figure 1 displays topic shares from 2013–2021, obtained by averaging the predicted topic probabilities across documents. Reassuringly, the most common topics identified by the model capture central themes of the radio show: The largest topic is "financial problems" (22%), which refers to segments where callers describe their personal finances, how much debt they owe, and how debt has negatively affected their well-being and personal relationships. This topic thus reinforces the radio show's message that debt is harmful. This is followed by a topic capturing the celebration of people who paid off their debt by decreasing their consumption and standards of living during the so-called debt-free scream (20%). The least common topics are "Insurance" (3%) and "Health care" (3%).

Figure 1: Topic distribution



Notes: This figure uses the text-to-speech transcripts of all videos uploaded on the *Dave Ramsey Show*'s YouTube channel between 2013 and 2021. The figure displays the distribution of topics across years. Topic shares are obtained from *Latent Dirichlet Allocation* by calculating the average probability of each topic across documents, where documents consist of 5-minutes of contiguous speech. For each year, the total content (in hours) uploaded on the radio show's YouTube channel is indicated above each bar. In total, excluding duplicate uploads, there are 2,934 hours of content.

[12]Appendix Section E.2 provides more details about the implementation. Appendix Figure E.1 displays the topic-specific word distribution of the trained LDA model. Appendix Section E.2.3 provides additional descriptive evidence from word frequencies and word co-occurrence rates

Figure 1 provides suggestive evidence that there are no major trends in the topic composition over time. While topic shares can fluctuate across years, none of the topics is on a clear upward or downward trajectory. Moreover, in the 2017–2021 period which accounts for 94% of all uploaded content, the topic distribution is remarkably stable. This is not surprising given the industry wisdom that radio listeners expect program consistency (Perebinossoff et al., 2005), which the vice president of Ramsey Media confirmed:

> "Consistency in messaging is paramount. You must give the audience what they want and expect on a consistent basis."
> – Brian Mayfield in an interview with *Inside Radio* (January 25, 2019)

These findings should, however, be taken with a grain of salt as material uploaded on YouTube is likely to be carefully selected to appeal to YouTube users, and may only offer a partial glimpse of what is discussed on-air. In particular, only a quarter of the text data comes from full-length episodes, which the radio show began to upload in 2019, while the remainder of the data comes from videos edited down to "highlights" of an episode. Restricting the topic analysis to full-length episodes—where scope for selection is more limited—reveals that the topic distribution is stable across years (as shown in Appendix Figure E.3). Moreover, despite the COVID-19 shock, Figure E.3 shows that the distribution of topics within full-length episodes changes little from 2019 to 2021.

3 Data

To study the impact of the *Dave Ramsey Show* on consumption, it is necessary to combine two types of data. First, one needs fine-grained information about the radio coverage, and hence availability, of the *Dave Ramsey Show* across space and over time. Second, this information has to be linked with comprehensive, household-level expenditure records. This section describes the data and methods used to satisfy these requirements.

3.1 Radio coverage

As individual exposure to mass media programs is unobserved, I exploit variation in the *availability* of the *Dave Ramsey Show* across space and over time. To determine the availability of the *Dave Ramsey Show* at a fine-grained geographic level, I utilize a unique data set including information about the radio stations that broadcast the radio show at each point in time. I then determine the geographic coverage of these radio stations using an engineer-developed radio propagation model.

Ramsey Media provided a list of 493 radio stations that broadcast the *Dave Ramsey Show*, including their call sign, broadcasting frequency, and, crucially, the exact date they started carrying the radio show. As many radio stations build secondary transmitters to increase their service area, I manually match all listed radio stations with license and construction records from the Federal Communications Commission (FCC), which yields 176 additional secondary transmitters. Figure B.3 displays the location of all 670 transmitters. For each transmitter, I collect technical specifications from the FCC's engineering records, such as the transmitter's effectively radiated power, height, broadcast frequency and geographic location, which I use to calculate the predicted receiver signal strength across zip codes.

The transmission of radio signals between a transmitter and a receiver location is governed by the laws of electromagnetic propagation. In free space, i.e., in the absence of topographic factors, radio signal strength depends on the frequency and power of the transmitter, and attenuates proportionally to the square of the distance from the transmitter. In practice, however, physical objects such as large buildings and topographic features such as mountains, forests and hills interfere with signal propagation, causing complex patterns of reflection, diffraction, and refraction (Cavell et al., 2017).

I therefore calculate the predicted radio signal strength corrected for topography using the Longley-Rice/Irregular Terrain Model (ITM).[13] Developed by the US government, the ITM is used by radio engineers and by economists, starting with Olken (2009), to predict the coverage area of radio transmitters. The high predictive accuracy of the model has been validated empirically in the field (Kasampalis et al., 2013). Specifically, I calculate the path loss (in dB) between the transmitter location and the centroid of US zip codes. I then obtain the receiver signal strength by subtracting the path loss from the signal strength of the transmitter. Next, I use the maximum receiver signal strength across transmitters in a zip code to determine radio coverage (Durante et al., 2019). Finally, I combine the time-invariant geographic coverage of each transmitter with data on when these transmitters started to broadcast the radio show. The result is a monthly panel of the predicted receiver signal strength across zip codes between 1994 and 2019.

As radio coverage requires a sufficiently strong signal, I binarize the radio signal strength based on a threshold of 50 dBμV/m (Cavallo, 2017). This allows me to distinguish between zip codes with and without radio coverage in my analysis. In a validation exercise, I show that the results are robust to using thresholds between 40 and 50 dBμV/m (as shown in Appendix Figure C.7), which have been used in prior work on the impact of radio broadcasts (Yanagizawa-Drott, 2014; Blouin and Mukand, 2019).

[13] I thank Benjamin Olken for kindly sharing the ITM code.

3.2 Nielsen Homescan

To measure people's consumption, I draw on expenditure records from the Nielsen Homescan panel. A crucial advantage of this data compared to other household surveys is that the location of residency of each participating household is observed down to the 5-digit zip code level, which allows me to exploit fine-grained variation in radio coverage in my empirical analysis. The data set includes detailed information on the food and non-food product purchases of over 100,000 US households from 2004–2019. Households use an optical scanner at home to record information about their product purchases from grocery stores, drug stores, liquor stores and other retailers. The information includes the price, quantity, date of purchase, store identifiers, deals, and product characteristics at the Universal Product Code (UPC) level. For each shopping trip, households record the date and the store location before scanning the UPC bar codes of purchased items and entering prices and quantities. If the retailer exchanges point-of-sale data with Nielsen, the weighted average retailer-week price of each item is automatically recorded. Otherwise, households manually enter prices from their receipt and any deals involved in purchasing the item.[14] Nielsen imposes an undisclosed annual expenditure threshold that the value of all recorded purchases must exceed for a household to be included in the data set. Comparisons with the Consumer Expenditure Survey suggest that recorded purchases in the Nielsen panel account for a quarter of average annual household expenditures (Dubé et al., 2018). Nielsen also collects a broad set of self-reported demographic information, such as household income and household composition, age, gender, race, employment status and education of the household heads. Importantly, households also report the 5-digit zip code of their location of residency.

When recruiting panelists, Nielsen employs a stratified sampling approach to ensure that the sample is broadly representative of the general population in terms of nine demographic characteristics.[15] Moreover, the Nielsen Homescan sample is highly geographically dispersed. Figure B.4 displays the distribution of Nielsen households across the 210 Designated Market Areas (DMAs), where a single DMA comprises several counties. These DMAs are used in the media industry to define media markets.

My primary outcome is the log of monthly household expenditures, which I obtain by aggregating total food and non-food expenditures before coupon use across all shopping

[14]A potential concern is that households record product purchases with errors. Einav et al. (2010) study the quality of the data by comparing scanner data from a large retailer with self-reported product purchases and find that the reporting error is comparable in magnitude to other commonly used economic data sets.

[15]The demographic variables are household size, income, age of head of household, race, Hispanic origin, education of male and female household heads, occupation of head of household, presence of children, and county size. Lusk and Brooks (2011) study selection into household scanning panels such as Nielsen Homescan. They find that panelists tend to be older, more educated, more female and more price sensitive compared to a probability-based sample.

trips within a calendar month. In Section 5.7, I verify that my results are robust to using alternative definitions of household expenditures. Appendix Figure B.5 provides an overview of the geographic variation in average monthly household expenditures, which range from $357 up to $530 per month.

In my empirical analysis, I apply three exclusion criteria. First, I drop households that join the Nielsen Homescan panel after the *Dave Ramsey Show* became available in their location of residence. As my empirical strategy identifies the impact of the radio show from within-household changes, these "always treated" households do not contribute any identifying variation. On the contrary, recent progress on the econometrics of two-way fixed effects models shows that the presence of always treated units can actually bias estimates (see, for instance, Goodman-Bacon, 2019; Borusyak et al., 2021). Second, I focus on households that participate in the Nielsen panel for at least two years. For households that experience a change in radio coverage, I require that households are observed at least one year before and after they receive access to the radio show to ensure a sufficient observation period. Finally, I drop households that move across zip codes to address concerns about changes in purchase behavior in the years around the move (Bronnenberg et al., 2012; Allcott et al., 2019). This additionally addresses concerns about selective migration of households into regions with access to the *Dave Ramsey Show*. The final panel of 3,744,078 household-months comprises 39,016 households in 11,219 zip codes across 202 DMAs.

3.3 Additional data

I supplement my analyses with additional data from various sources, including information on monthly house prices at the zip code level (from the Zillow Group), the county-level monthly unemployment rate and annual per-capita income (from the Bureau of Labor Statistics), the share of population in urban areas, racial composition and age groups (county-level; US Census and American Community Survey), and information about the Christian population. Moreover, I obtain county-level data on voter turnout and party vote shares for the 2000-2016 Presidential elections from the MIT Election Data and Science Lab (2018). Appendix Section A provides an overview of all data sources.

4 Empirical strategy

4.1 National expansion

My empirical analysis exploits the staggered, national expansion of the *Dave Ramsey Show* across the US between 2004 and 2019. The radio show started in 1992 on 99.7

WWTN in Nashville, Tennessee, and began expanding to other markets in 1996. As a self-syndicated radio show, Ramsey Media neither owns nor operates radio stations, but rather engages in so-called affiliate relations with independent radio stations and networks. Affiliates receive locally exclusive access in exchange for advertisement minutes, a common practice in the radio industry that enables talk shows to realize economies of scale and radio stations to outsource the risk inherent in content production. As of 2019, the radio show is broadcasted by over 600 radio stations covering 208 out of 210 DMAs (see Appendix Figure B.3). Figure 2 provides an overview of the staggered national expansion by indicating the biannual availability of the *Dave Ramsey Show* as well as changes in its coverage area. The expansion of the radio show into geographically distant media markets occurs early on and the sequence of the expansion does not appear to be driven by geographic considerations. Moreover, with about 40 new affiliates per year, the expansion was generally uniform over time (see Figure B.1).

In line with these patterns, the radio show's expansion was not driven by strategic decisions. Qualitative evidence based on personal interviews with senior managers responsible for the expansion of the radio show's affiliate network suggests that the radio show did not prioritize media markets based on socioeconomic characteristics, trends in local economic outcomes, or consumer preferences. Instead, it focused on simply increasing the number of its affiliated stations:

> "The main determining factor for choosing a market to enter is whether or not we are already on in that market. [...] We are either on or we're not on. And so even if we are adding a station and the listenership numbers are minimal, it's still better than zero and still better than not being on."
>
> – Personal interview with a senior manager

The primary reason for this is that radio stations evaluate prospective talk shows based on their past performance in other markets. Indeed, these interviews reveal that radio stations often require evidence of successes in other markets before becoming an affiliate.[16] Moreover, it was important to document a growing number of affiliated radio stations in different regions, as some stations were concerned that the radio show might only find regional success. Thus, the radio show faced strong incentives to expand its network of affiliated stations in a variety of locations.[17]

As a result, the timing of market entry was mainly driven by idiosyncratic demand for a non-political, general interest radio show that allows radio stations to diversify their program of predominantly political talk shows. After describing my empirical strategy,

[16] Dave Ramsey in an interview with AllAccess.com (July 6, 2010).

[17] It is a common practice in the radio industry to promote talk shows to hundreds of radio stations (Hendricks and Mims, 2018), which makes a targeted approach based on in-depth market research economically infeasible as establishing relationships takes time and is a labor-intensive process.

Section 4.3 will present additional, statistical tests suggesting that the timing of market entry was driven by idiosyncratic factors.

4.2 Econometric model

To estimate the effect of the *Dave Ramsey Show* on household expenditures, I employ a difference-in-differences strategy leveraging the radio show's staggered market entry across US zip codes from 2004–2019. Specifically, I estimate the following equation on a monthly panel of households:

$$\text{Outcome}_{itz} = \beta \text{Coverage}_{zt} + \phi_i + \psi_t + X'_{itz}\lambda + \varepsilon_{itz} \tag{1}$$

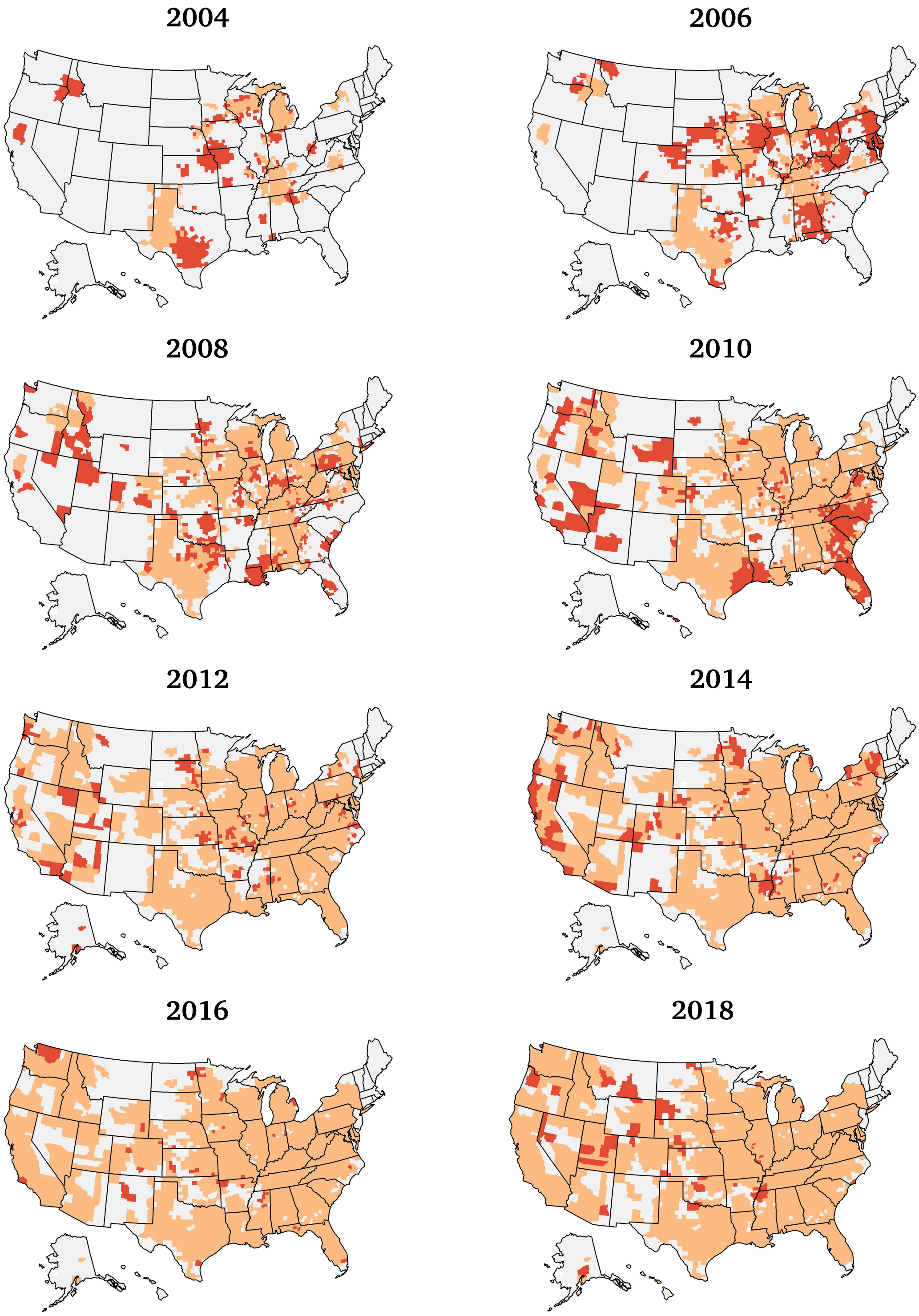
In the primary analysis, Outcome_{itz} is the log monthly expenditures of household i, residing in zip code z, at time t. Coverage_{zt} is a binary indicator variable taking value one if the *Dave Ramsey Show* is available in zip code z at time t and zero otherwise. In all specifications, I include household fixed effects, ϕ_i, and year-month fixed effects, ψ_t. The vector X_{itz} includes time-varying covariates that account for changes in the household's economic situation and local economic shocks. Household-level controls include log household income, household size, marriage status, employment status, and age indicators. Local economic conditions are proxied by zip code-level house prices and the local unemployment rate. In additional specifications, I further include state×year-month fixed effects or DMA×year-month fixed effects, which effectively restricts comparisons to within the same state or Nielsen media market. For inference, I use robust standard errors clustered at the zip code level at which the radio coverage indicator varies. The results are robust to using alternative clustering of standard errors.[18]

Equation (1) estimates the impact of the *Dave Ramsey Show* under the assumption that the timing of market entry is conditionally uncorrelated with pre-existing trends in household expenditures. Under this assumption, we can use changes in household expenditures in markets without radio coverage as a counterfactual for the evolution of expenditures in regions that receive access to the radio show.

To empirically evaluate the plausibility of this identification assumption, I present estimates from an event-study approach, which allows me to inspect the dynamics of short-term effects before and after market entry of the radio show. Specifically, I replace

[18]While Nielsen provides post-stratification weights, I do not weight households in my analyses because the set of households that experience a local market entry of the radio show is not nationally representative. For completeness, I show that the results are robust to using the Nielsen weights in Section 5.7.

Figure 2: Radio coverage of the *Dave Ramsey Show*



Notes: This map shows the coverage of the *Dave Ramsey Show* from 2004–2018. Counties with coverage are shown in orange, while those without are indicated in grey. Areas that received coverage within the last two years are indicated in dark red. A county is defined as having coverage in this figure if at least 50% of the population has access to the radio show. The Longley-Rice/Irregular Terrain Model is used to estimate radio coverage at the zip code level, which I aggregate to the county level using population weights. Section 3 describes the data and procedure in more detail.

the binary coverage indicator in Equation (1) with a set of event-time indicators:

$$\text{Outcome}_{itz} = \sum_{\tau=-12}^{12} \beta_\tau \text{Coverage}_{zt\tau} + \phi_i + \psi_t + X'_{itz}\lambda + \varepsilon_{itz} \tag{2}$$

The binary event-time indicator $\text{Coverage}_{zt\tau}$ takes value one if $\tau = t - \tau^*_z$, where τ^*_z is the first time that the *Dave Ramsey Show* was available in zip code z, and zero otherwise. I further include binned indicator variables for event-times more than 12 months before and after market entry. After normalizing β_{-1} to zero, the coefficients β_τ capture the impact of the radio show τ months after market entry relative to the last month in the pre-exposure period. Given recent work on potential biases in two-way fixed effects models arising from dynamic treatment effects, I present complementary event-study estimates using the imputation estimator proposed by Borusyak et al. (2021) as an additional robustness check in Section 5.7.

4.3 Identification assumption

This section provides further evidence supporting the plausibility of the identification assumption that the timing of market entry was conditionally exogenous.

4.3.1 Determinants of market entry

First, I examine the association between the first time the radio show became available in a given area and different standardized baseline covariates from the year 2000. As shown in Figure 3, these associations are all economically small and statistically insignificant. Specifically, a one standard deviation change in any baseline characteristic is associated with a change in the timing of market entry by no more than 3 months—a negligible association compared to the 39 months standard deviation of market entry. This complements the qualitative evidence that market entry was a non-strategic decision.

4.3.2 Machine learning

Second, I conduct a falsification test assessing whether one can predict the timing of market entry from sociodemographic factors. If observables do not improve the predictive accuracy, we should be less concerned about endogeneity of the staggered expansion. To provide a demanding test, I use supervised machine learning and cross-validation to assess the predictability of market entry. A key advantage of machine learning is that it can explore more general relationships and leverage higher-order interactions without imposing functional form assumptions such as linearity. In practice, I repeatedly train

Figure 3: Determinants of the timing of market entry

Notes: This figure plots the coefficients from univariate regression of the first year-month of radio coverage on different baseline characteristics from the year 2000. The unit of observation are zip codes or counties depending on the level of aggregation at which baseline characteristics are measured. All baseline characteristics are standardized to have mean zero and standard deviation one to facilitate comparisons. The standard deviation of the timing of market entry is 39 months, or 3.25 years. 95% confidence intervals are constructed from robust standard errors clustered at the DMA level.

different models to predict the timing of market entry across zip codes with at least one Nielsen household between 2004 and 2018. I use the root mean squared prediction error (RMSE) on a hold-out sample to assess the model fit. The test-train sample splits are obtained from an implementation of a spatial leave-p-groups-out cross-validation approach to prevent "data leakage" from spatial autocorrelation (Le Rest et al., 2014; Roberts et al., 2017).[19] I then compare the distribution of the RMSE of each model to the distribution of the RMSE obtained from randomly assigning counterfactual entry dates.

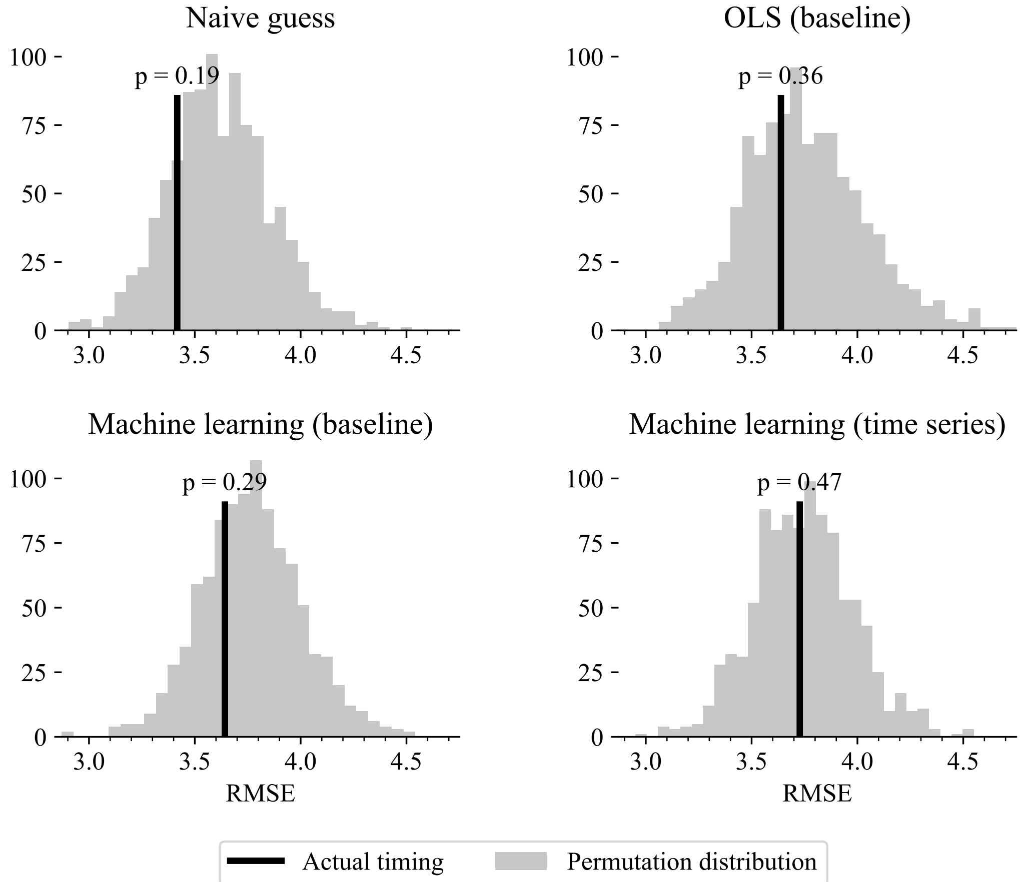
Figure 4 presents the results. A "naïve" model making a constant prediction equal to the average entry date in the training data achieves a median RMSE of 3.9 years, with an associated p-value of 0.19 compared to the random benchmark distribution. Linear regression models using baseline observables do not improve the predictive accuracy ($p = 0.36$).[20] Next, I consider a Random Forest regressor (Breiman, 2001) with hyperparameters described in the Appendix, which is a commonly used general-purpose

[19]To split the data, I randomly draw three coordinates in the contiguous US and assign all zip codes within 500 km of these coordinates to the test data set. The training data comprises the complement after removing a "buffer zone" in the shape of a ring with a width of 300 km around the test data to ensure independence across samples. The diameter of the buffer zone was chosen such that the coverage area of a radio station does not intersect both the test and the training data.

[20]The variables include the zip code and county population, population density, age shares (10-year bins), female, white, Hispanic and Christian population shares, per-capita income, the county unemployment rate and the degree of urbanization.

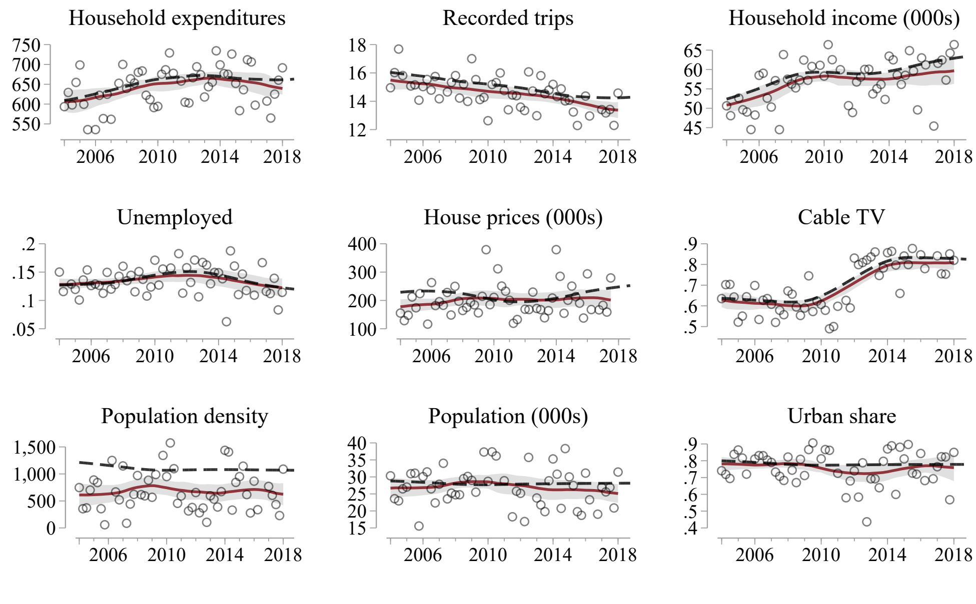
machine learning technique (Varian, 2014; Wager and Athey, 2018; Besley et al., 2019). Despite its flexibility, the Random Forest using baseline covariates has low predictive accuracy in this setting ($p = 0.49$). While this addresses endogeneity concerns based on baseline variables, the timing of market entry could also have depended dynamically on local trends in economic conditions. However, the results from a Random Forest regressor using panel data on the local unemployment rate and local average income suggest otherwise ($p = 0.47$). This evidence leaves little scope for local economic conditions to have driven the timing of market entry.

Figure 4: Predictability of the timing of market entry



Notes: This figure displays the results of a permutation test of the predictability of the timing of market entry across zip codes. In each panel, the black vertical line indicates the average root mean squared prediction error (RMSE) of the model obtained from a spatial cross-validation procedure. The implied p-values obtained from the permutation distribution are indicated. The permutation distribution is obtained from 1,000 random permutations of the dates at which affiliated radio stations started to carry the show, and subsequently recomputing the implied coverage across zip codes using the predicted signal strength. The "Naive guess" always predicts the empirical mean in the training data. "OLS (baseline)" and "Machine learning (baseline)" try to predict the timing of entry based on baseline zip code and county characteristics from 2000, including the demographic composition and local economic conditions. "Machine learning (time series)" shows the result of a Random Forest using annual data on the county unemployment rate and the average per-capita income from 2002–2016 as features.

Figure 5: Covariates of households that gain access to the radio show



Notes: This figure compares the characteristics of households that received access to the *Dave Ramsey Show* in a given year-quarter to the average across all households in the Nielsen panel. The hollow circles indicate the quarter-by-quarter average characteristics of households that gained access to the *Dave Ramsey Show* for the first time in the given quarter. The red line indicates a smoothed local approximation of this average (Epanechnikov kernel, rule-of-thumb bandwidth estimator), with shaded areas indicating 95% confidence interval. Quarter-averages are trimmed at the 1st and 99th percentile prior to estimating the local approximation. The dashed black line indicates the local approximation of the quarter-by-quarter average characteristic of all Nielsen panelists. "Household expenditures" are total monthly expenditures in dollars. "Recorded trips" are the number of different shopping trips for which a household recorded purchases. "Household income" is measured annually. "Unemployed households" is the share of panelists that are unemployed at the beginning of the calendar year. "House prices" is the zip code-level Zillow House Price Index (monthly frequency). "Cable TV" is the share of panelists that have access to cable television. "Population density" is the zip code population density in 2010. "Population" is the zip code population in 2010. "Urban share" is the share of the population living in urban areas in 2000.

4.3.3 Characteristics of treatment cohorts

Idiosyncratic timing of market entry would imply that the covariates of incoming treatment cohorts evolve in parallel to the covariates of the average Nielsen household. If, however, the *Dave Ramsey Show* strategically timed its expansion based on information about the local audience, the characteristics of incoming treatment cohorts should change over time. To explore this, I group Nielsen households into different "treatment cohorts" based on the year-quarter in which they receive access to the radio show. For each treatment cohort, I then calculate the average covariates of these households in the year-quarter in which they are treated for the first time. Similarly, I calculate the average covariates of all Nielsen households for each year-quarter and subsequently compare differences in observables between incoming treatment cohorts and the average Nielsen household over time. Figure 5 presents the results. Each circle represents the average characteristic of the incoming treatment cohort, and the solid red line indicates a smoothed local average across treatment cohorts. The average across all Nielsen households is indicated by the black dashed line. The evidence suggests that incoming treatment cohorts are very similar to the average household at that point in time across a rich set of observables. For example, the expenditure levels of incoming treatment cohorts closely track average expenditures in the sample. This provides additional evidence suggesting that there was no selection based on observables such as household expenditures, income, local house price or population.

5 Results

5.1 Household expenditures

I first examine the impact of the *Dave Ramsey Show* on household expenditures. In Table 1, I estimate different versions of the baseline specification (equation 1) using the log of monthly household expenditures as the dependent variable. The main finding is that household expenditures decline after the market entry of the *Dave Ramsey Show*. Across specifications, I find a statistically significant decrease between 1.2% and 1.6%, which implies a decrease in annual expenditures of $70–93. Column 1 shows that when including only household and year-month fixed effects, the effect is a 1.3% decrease in household expenditures ($p < 0.01$). This effect remains statistically significant and quantitatively stable once I control for time-varying household characteristics ($p < 0.01$, column 2), which addresses concerns about household-level labor market shocks. Column 3 further controls for house prices and the local unemployment rate to account for heterogeneous trends in local economic conditions. The resulting decrease of 1.6% is slightly larger than the estimate without these controls. Moreover, the effect is robust both to the inclusion of state×year-month fixed effects that account for unobserved economic changes at the state level ($p < 0.01$, column 4), as well as to the inclusion of interactions between county baseline characteristics and year-month fixed effects (column 5).

Figure 6 presents the corresponding event-study estimates (equation 2) using log expenditures as dependent variable. The estimates show the absence of any statistically significant difference in pre-trends in the twelve months before market entry, supporting the plausibility of the identification assumption of parallel trends in household expenditures in the absence of the radio show. The effects are stable in the first year after market entry, suggesting a persistent change in behavior. The decrease in expenditures in the

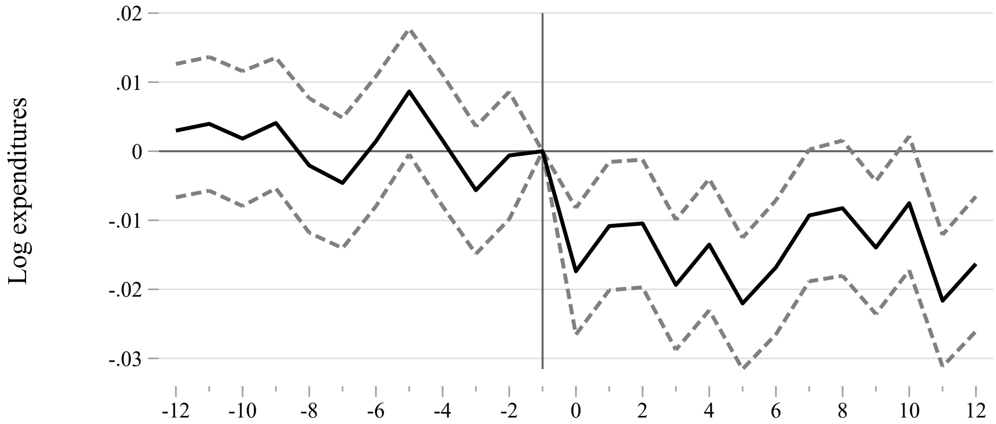
Table 1: Household expenditures

	Dependent variable: log (Expenditures)					
	(1)	(2)	(3)	(4)	(5)	(6)
Radio show	-0.0131***	-0.0128***	-0.0161***	-0.0121***	-0.0133***	-0.0140***
	(0.0027)	(0.0026)	(0.0027)	(0.0034)	(0.0034)	(0.0042)
N	3,744,066	3,744,066	3,407,700	3,407,700	3,355,677	3,354,689
R^2	0.518	0.521	0.522	0.524	0.525	0.529
Mean of dep. var.	6.185	6.185	6.186	6.186	6.185	6.185
Household & Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Household controls		Yes	Yes	Yes	Yes	Yes
Local economic conditions			Yes	Yes	Yes	Yes
State x Time FEs				Yes	Yes	
County controls x Time FEs					Yes	Yes
DMA x Time FEs						Yes

Notes: This table uses 2004–2019 Nielsen Homescan data at the household-by-month level. The dependent variable is the log of household expenditures. "Radio show" is a binary indicator taking value one after local market entry of the *Dave Ramsey Show*. Individual controls include the log of household income, age indicators, household size, married indicator and employment status indicators (full-time, part-time, unemployed). Local economic conditions comprise controls for house prices and the unemployment rate. Baseline county controls include the racial composition (share of whites), log per-capita income, log population and the share of Christians. Robust standard errors clustered by zip code are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 6: Event-study – Household expenditures



Notes: This figure uses 2004–2019 Nielsen Homescan data at the household-by-month level and depicts the results of a regression of log household expenditures on a set of event time indicators for the twelve months before and after market entry (see equation 2). The month before market entry serves as the omitted category. The regression also includes household and year-month fixed effects. 95% confidence intervals are constructed from robust standard errors clustered at the zip code level.

first months following market entry is consistent with the strong and immediate impact of listening to the radio show on consumption attitudes (see Section 6).

To address concerns that the decrease in expenditures reflects selection on unobserved economic shocks, I conduct two robustness checks. First, I estimate equation (1) on a subset of the data where the identification assumption is more likely to hold. Specifically, I exclude zip codes within 500 km of the radio show's headquarter in Nashville, Tennessee. Moreover, I exclude all 443 counties with an affiliated radio station. Appendix Table C.6 shows that applying these exclusion criteria individually (columns 2 and 4) or jointly produces similar results (column 6). Second, I include DMA×year-month fixed effects in equation (1), which effectively restricts to local comparisons within the same media market. Trends in economic conditions are more likely to be comparable within media markets. Moreover, ratings information from market research companies are only available at the media market level. Flexible DMA-level trends thus account for selection on such unobserved information about audience preferences. In column 6 of Table 1, I find that the decrease in expenditures is robust to adding DMA×year-month fixed effects ($p < 0.01$). This evidence suggests that the results are not driven by endogenous market entry based on private information available to the radio show and its affiliated radio stations.

5.2 Magnitudes

As the empirical strategy identifies an intent-to-treat effect, the 1.3% decrease in expenditures is the most conservative estimate of the impact of the *Dave Ramsey Show* on the behavior of its audience. Ideally, one would use individual radio listenership information or geographically disaggregated audience data to estimate the local average treatment effect of the radio show's message on its actual audience. In the absence this data, I conduct a bounding exercise. Specifically, I divide the intent-to-treat effect by a range of alternative estimates of the share of Americans that have been exposed to the radio show's content, assuming that this percentage is constant across geographic areas.

An upper bound on the reach of the radio show are the 49% of Americans that have heard of the radio show (YouGov, 2021b), which implies a lower bound on the impact of the radio show on its audience of 2.7%. A lower bound on its audience can be derived from its weekly audience, which suggests a 6.5% national audience share. While this disregards sporadic and past exposure to the radio show, it implies an upper bound on the radio show's impact of about 20%. These bounds on the radio show's impact on its audience are likely to be non-binding, as they rely on very broad and very narrow notions of exposure. Alternatively, slightly tighter bounds can be derived from the following statistics. First, the radio show is "liked" by about 24% of Americans (YouGov, 2021b). Second, in my own representative survey, 8.3% of Americans can recall the name of the *Dave Ramsey Show* after listening to it for five minutes (see Section 6 for more details). These statistics would suggest that the *Dave Ramsey Show* causes a decrease in expenditures between 5.4% and 15.7% among its audience. The magnitude of the effect is thus economically meaningful, suggesting that mass media programs can have a substantial impact on the primary economic decision of how much to consume.

The magnitude of the effect is not implausible in light of the economically large impact of mass media on behavior documented in previous studies, in particular in settings where the media delivers an unusual message (DellaVigna and La Ferrara, 2015). These studies typically consider binary outcomes and calculate persuasion rates, a methodology pioneered by Della Vigna and Kaplan (2007), to compare media effects across settings. For example, Martin and Yurukoglu (2017) estimate that Fox News persuaded 58% of its viewers to vote Republican in 2000, while Wang (2021) finds that exposure to Father Coughlin's radio show persuaded 28% of his listeners to vote against Roosevelt. Moreover, Yanagizawa-Drott (2014) attributes 10% of the total violence during the Rwandan genocide to the impact of a popular radio station. While direct comparisons to voting or violent behavior are very difficult, the effect of the *Dave Ramsey Show* on consumption is consistent with the persuasiveness of mass media documented in other domains.

5.3 Purchased items

Next, I examine the mechanism through which households decreased their monthly expenditures. The radio itself provides comparatively little practical guidance on this question. Instead, its main advice is to "get on a budget" and keep track of all household expenditures to prevent overspending and impulse purchases. In light of this advice, one potential explanation for the decrease in expenditures is that households purchase fewer goods. To investigate this mechanism, I use the log of the total number of purchased items as a dependent variable, which I obtain by counting the number of UPC-level purchase records over the course of a calendar month. Table 2 provides estimates for different versions of the baseline specification (equation 1). I find that the availability of the *Dave Ramsey Show* causes households to purchase 1.7% fewer products ($p < 0.01$, column 1), which is robust across specifications (columns 2–6).[21] Figure 7 provides the corresponding event-study estimates, which indicate the absence of pre-existing differences in trends before the show's market entry. The implied effect of decreasing the number of purchased products on total household expenditures depends on the average price of the products which are no longer bought. Even if this price is 50% smaller than the price of the average product, a mechanism based on purchasing fewer goods would still account for at least half of the decrease in monthly household expenditures. This suggests that changes in the "extensive margin" are an important channel through

[21]Appendix Table C.1 presents analogous estimates from a Poisson regression.

which households decrease their expenditures.

Table 2: Number of purchased items

	Dependent variable: log (Number of purchased products)					
	(1)	(2)	(3)	(4)	(5)	(6)
Radio show	-0.0168***	-0.0161***	-0.0210***	-0.0217***	-0.0232***	-0.0204***
	(0.0029)	(0.0028)	(0.0030)	(0.0036)	(0.0036)	(0.0046)
N	3,734,881	3,734,881	3,399,597	3,399,597	3,347,655	3,346,664
R^2	0.541	0.545	0.546	0.548	0.549	0.553
Mean of dep. var.	4.189	4.189	4.186	4.186	4.184	4.184
Household & Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Household controls		Yes	Yes	Yes	Yes	Yes
Local economic conditions			Yes	Yes	Yes	Yes
State x Time FEs				Yes	Yes	
County controls x Time FEs					Yes	Yes
DMA x Time FEs						Yes

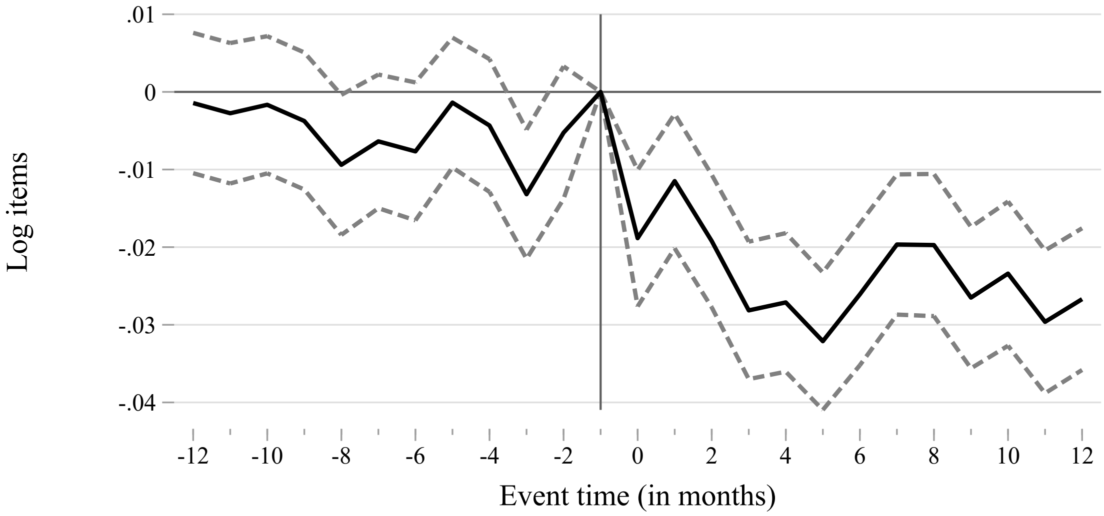
Notes: This table uses 2004–2019 Nielsen Homescan data at the household-by-month level. The dependent variable is the log of the number of purchased items per month. "Radio show" is a binary indicator taking value one after local market entry of the *Dave Ramsey Show*. Individual controls include the log of household income, age indicators, household size, married indicator and employment status indicators (full-time, part-time, unemployed). Local economic conditions comprise controls for house prices and the unemployment rate. Baseline county controls include the racial composition (share of whites), log per-capita income, log population and the share of Christians. Robust standard errors clustered by zip code are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.4 Bulk and on-sale purchases

In addition to purchasing fewer products, it is ex-ante possible that households also try to reduce the amount they spend on their current basket of goods. Leveraging the richness of the Nielsen Homescan data, I construct two measures of savings efforts. First, I use UPC-level information about the packaging of each purchased product to construct a measure of bulk purchasing. Specifically, I rank products by their package size within their Nielsen product module. I subsequently construct the monthly share of expenditures accounted for by "large packages", which I define as belonging to the top quintile of the package size distribution. Second, using data on whether an item was purchased at a discount, I construct the expenditure share of discounted items. Table 3 reports the estimates of equation 1 using the measures of bulk purchases and discounted items as dependent variables. Columns 1 and 2 indicate more bulk purchases, as the share of expenditures accounted for by large items increases by approximately 0.5–0.6 percentage points ($p = 0.01$). Similarly, the expenditure share of on-sale items increased by about 0.3–0.4 percentage points ($p = 0.01$, columns 3–4). However, these ultimate effects of these behavioral changes on monthly household expenditures are likely to be modest compared to the effect of decreasing the number of products purchased, which

Figure 7: Event-study – Number of purchased items



Notes: This figure uses 2004–2019 Nielsen Homescan data at the household-by-month level and depicts the results of a regression of the log of the total number of purchased items per month on a set of event time indicators for the twelve months before and after market entry (see equation 2). The month before market entry serves as the omitted category. The regression also includes household and year-month fixed effects. 95% confidence intervals are constructed from robust standard errors clustered at the zip code level.

is evident from the following example. Griffith et al. (2009) estimate a mean discount of 20% from purchasing on-sale items and average savings of 16% from purchasing bulkier products. Thus, the maximum decline in expenditures that is attributable to both activities is 15%, which suggests that the decrease in expenditures is primarily driven by the extensive margin.[22] Figure 8 presents the corresponding event study estimates for both measures.

5.5 Heterogeneity

A heterogeneous impact of the radio show across different groups could be driven by (i) differential selection into the radio show's audience, or (ii) differences in the susceptibility of these groups to the radio show's persuasive messages. As individual exposure to the radio show is unobserved, it is difficult to distinguish these explanations, which makes it difficult to derive ex-ante hypotheses about which patterns of effects one would expect along dimensions such as gender, age, or education. However, from a policy perspective, it matters whether the radio show persuades the intended target population, i.e., those households that are likely to overspend. These households may both be more

[22]The potential savings as a fraction of expenditures can be bounded from above by $0.006 \times 0.16 + 0.004 \times 0.20 = 0.00176$, which is 13.4% of the 1.31% decrease in overall expenditures.

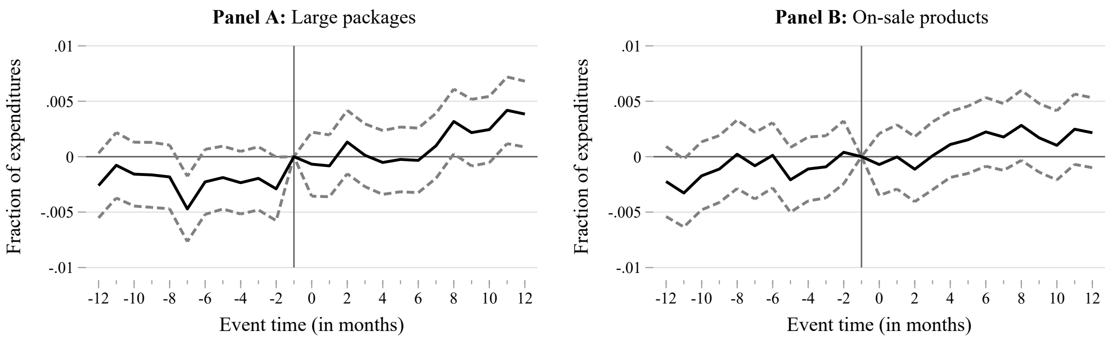
Table 3: Bulk purchases and on-sale products

	Dependent variable: Expenditures share of					
	Large packages			On-sale products		
	(1)	(2)	(3)	(4)	(5)	(6)
Radio coverage	0.0043***	0.0047***	0.0064***	0.0035***	0.0029***	0.0043***
	(0.0007)	(0.0007)	(0.0008)	(0.0009)	(0.0010)	(0.0012)
N	3,734,872	3,399,588	3,399,588	3,734,881	3,399,597	3,399,597
R^2	0.460	0.463	0.465	0.714	0.714	0.716
Mean of dep. var.	0.290	0.290	0.290	0.299	0.305	0.305
Household & Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Household controls		Yes	Yes		Yes	Yes
Local economic conditions		Yes	Yes		Yes	Yes
State x Time FEs			Yes			Yes

Notes: This table shows OLS regression estimates of equation (1) using a monthly panel of households. "Radio show" is a binary indicator taking value one after local market entry of the *Dave Ramsey Show*. The dependent variables are the share of monthly expenditures accounted for by purchasing items large items or on-sale products, respectively. "Large packages" is the share of expenditures accounted for by items in the top quintile of the package size distribution. "On-sale products" is the share of expenditures accounted for by items that were purchased on-sale. Robust standard errors clustered at the zip code are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 8: Event-study – Bulk purchases and on-sale products



Notes: This figure uses 2004–2019 Nielsen Homescan data at the household-by-month level and depicts the results of regressions of different outcomes on a set of event time indicators for the twelve months before and after market entry (see equation 2). The month before market entry serves as the omitted category. The regression also includes household and year-month fixed effects. 95% confidence intervals are constructed from robust standard errors clustered at the zip code level. Panel A uses the share of expenditures accounted for by items in the top quintile of the package size distribution as dependent variable. Panel B uses the share of expenditures accounted for by items that were purchased on-sale as dependent variable.

prone to listen to the radio show and more likely to follow its advice in light of the fact that the radio show is specifically geared towards people who "live beyond their means." It is thus natural to hypothesize that initial expenditures moderate the magnitude of the radio show's effect.

To test this hypothesis, I construct a proxy for baseline household expenditures and examine heterogeneity in effects along absolute and relative expenditures. Baseline expenditures are constructed as the average of inflation-adjusted monthly expenditures in the first year in the panel and excluding months in which households have access to the radio show.[23] I then separately estimate the impact of the radio show on expenditures among household whose baseline expenditures lie above or below the median household. Table 4 presents the results. I find a large and highly statistically significant effect among households with high baseline expenditures ($p < 0.01$, column 1). In contrast, column 2 reveals that the effect of the radio show is economically small and statistically insignificant among households with low baseline expenditures. Moreover, the negative point estimate in column 2 suggests that the effect among high-expenditure households is not driven by mean reversion. To examine whether this merely reflects differences in income, I construct baseline household income using the same procedure as above. Columns 3 and 4 show that there is no differential impact of the radio show among households with high or low baseline incomes. Indeed, when exploring heterogeneity by baseline expenditures relative to income, I again find a large decrease of 1.6% among households with high expenditures relative to their income ($p < 0.01$, column 5) and no statistically significant effect among households with low expenditures relative to their income (column 6). This evidence is consistent with the fact that the radio show's advice is geared towards people who overspend and suggests that the radio show primarily affects those who may stand to gain most from changing their behavior.

5.6 Exploiting topographic variation

This section considers a more demanding specification in which the impact of the radio show is identified using only residual variation in the continuous radio signal strength arising from the interaction between the timing of the staggered expansion and the influence of the local topography. This approach further alleviates endogeneity concerns based on strategic market entry as the factors driving market entry decisions are likely to be uncorrelated with local topographic variation. Specifically, I estimate the following equation:

$$\log(\text{Expenditures})_{itz} = \beta Signal_{zt} + \gamma SignalFree_{zt} + \phi_{iz} + \psi_t + X'_{itz}\lambda + \varepsilon_{itz} \qquad (3)$$

Signal is the standardized, continuous measure of signal strength in zip code z at time t, and *SignalFree* is its free-space analog, which differs from the former whenever topographic features interfere with the transmission of radio signals between the transmitter

[23]To account for the household composition, I normalize expenditures using an equivalence scale that assigns a weight of 0.7 to each additional adult and a weight of 0.5 to each child within a household.

Table 4: Heterogeneity analysis by expenditures

	Dependent variable: log (Expenditures)					
	Expenditures		Income		Expenditures / income	
	(1) High	(2) Low	(3) High	(4) Low	(5) High	(6) Low
Radio show	-0.019***	-0.002	-0.012**	-0.012**	-0.016***	-0.007
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
N	1,812,463	1,595,237	1,887,781	1,519,910	1,667,035	1,740,651
R^2	0.463	0.455	0.524	0.523	0.527	0.484
Mean of dep. var.	6.447	5.890	6.233	6.129	6.357	6.023
Full controls	Yes	Yes	Yes	Yes	Yes	Yes
State x Time FEs	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table uses 2004–2019 Nielsen Homescan data at the household-by-month level. The dependent variable is the log of household expenditures. "Radio show" is a binary indicator taking value one after local market entry of the *Dave Ramsey Show*. In all regressions, the set of control variables includes household covariates and controls for local economic conditions. Robust standard errors clustered by zip code are shown in parentheses. Each column provides estimates from a subset of households obtained by a median split based on the household covariate indicated in the column's header. For the median split in columns 1–2, I use the average, inflation-adjusted and equivalized expenditures in the first year a household is in the panel. For the median split in columns 3–4, I use the average, inflation-adjusted and equivalized household income in the first year in the panel. For the median split in columns 5–6, I use the average household expenditures normalized by income in the first year in the panel.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

and the receiver location. By controlling for *SignalFree*, the main coefficient of interest, β, is only estimated from residual, plausibly exogenous variation in the radio signal strength. The nested zip code fixed effects account for any direct effects of topography on household expenditures. The identifying assumption underlying this approach is that the residual variation in signal strength arising from the interaction of the staggered expansion of the radio show and the detrimental effect of topographic obstructions on signal strength is uncorrelated with time-varying determinants of household expenditures. Appendix Figure C.8 supports this assumption by documenting economically small and statistically insignificant correlations between the signal strength residuum and a large set of time-varying county-level characteristics.

Table 5 reports the results from estimating equation (3). Excluding the free space signal, a one standard deviation increase in signal strength leads to a statistically significant decrease in expenditures by 0.56% (column 1). Using only residual variation in radio signal strength, this effect increases to a 0.96% decline in expenditures per standard deviation change in signal strength (column 2). The effect is robust to including additional controls (columns 3–5). These estimates corroborate the baseline results and are quantitatively similar to the estimates from the specification using the binarized radio coverage variable presented in Table 1.

Table 5: Exploiting topographic variation in signal strength for identification

	Dependent variable: log (Expenditures)				
	(1)	(2)	(3)	(4)	(5)
Signal	-0.0056***	-0.0096***	-0.0088***	-0.0098***	-0.0082**
	(0.0016)	(0.0027)	(0.0027)	(0.0028)	(0.0037)
SignalFree		0.0049*	0.0039	0.0044	0.0092**
		(0.0028)	(0.0028)	(0.0029)	(0.0039)
N	3,599,959	3,599,959	3,599,959	3,272,490	3,272,490
R^2	0.521	0.521	0.524	0.525	0.527
Mean of dep. var.	6.185	6.185	6.185	6.186	6.186
Household & Time FEs	Yes	Yes	Yes	Yes	Yes
Household controls			Yes	Yes	Yes
Local economic conditions				Yes	Yes
State x Time FEs					Yes

Notes: This table presents OLS regression estimates of equation 3. "Signal" is the continuous measure of signal strength and "SignalFree" is the signal strength in free space. Both signal measures are standardized to have mean zero and standard deviation one. Robust standard errors clustered at the zip code level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.7 Additional analyses and robustness checks

Dynamic treatment effects I conduct several robustness checks to address concerns arising from recent work on the econometrics of two-way fixed effects models (Goodman-Bacon, 2019; de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2021; Callaway and Sant'Anna, 2020). Specifically, these studies show that two-way fixed effects estimators can be biased in the presence of heterogeneous treatment effects across cohorts and over time. First, I re-estimate equation 1, while excluding different treatment cohorts based on the year when they received access to the *Dave Ramsey Show*. Table C.3 presents statistically significant estimates independent of which treatment cohorts are excluded. Notably, the results are robust to excluding households that receive access to the radio show during the Great Recession. Second, the Nielsen panel's sample is skewed towards top media markets as measured by population, which could bias results if across-market cohorts experience different dynamic effects. However, columns 1–4 of Appendix Tables C.4 and C.5 suggest that the effects are robust to excluding DMAs based on their Nielsen rank. Columns 5–7 show that the effects are additionally robust to focusing on homogeneous groups of markets, except for the lower tail where limited sample sizes become a concern. Third, I replicate the event-study using the imputation estimator proposed by Borusyak et al. (2021), which is robust to dynamic treatment effects and efficient in finite samples. The estimates presented in Appendix Figure C.1 closely resemble the dynamic patterns derived from OLS estimates (see Figure 6). This evidence suggests that treatment effect heterogeneity over time is not a major concern in this setting.

Falsification To test whether the difference-in-differences estimates pick up spurious correlations, I conduct a falsification exercise. Specifically, I repeatedly assign a randomly chosen counterfactual market entry date to each zip code. If a zip code is outside the actual coverage area of all affiliated radio stations, the zip code is always assigned to the control group without any market entry. I thus vary only the timing of entry in a zip code, while the set of zip codes that receive access to the radio show remains unchanged. Based on the counterfactual timing of market entry, I apply equivalent sample restrictions (see Section 3.2) and re-estimate equation 1 using household expenditures as the dependent variable. Figure D.1 compares the actual effect to the distribution of coefficients obtained from 500 repetitions of this procedure. The mean of the distribution is close to zero and negative, which reflects the fact that the set of treated zip codes is held constant and only the timing of entry changes. Moreover, the effect based on the actual timing of market entry is outside the empirical support of the distribution, suggesting that it is unlikely to arise by chance.

Placebo outcomes As the *Dave Ramsey Show* is a non-political talk show, it should not affect political outcomes. I thus use the electoral turnout and the vote share of the Republican party as placebo outcomes. Specifically, I obtain county-level data for the 2000-2016 Presidential elections from the MIT Election Data and Science Lab (2018). Table D.1 presents estimates from a panel regression of these political outcomes on the corresponding share of the county population that could listen to the radio show in the election year. As counties vastly differ in their population size, I weigh observations by the county's voting-age population. As expected, the radio show has no statistically significant effects on political outcomes.[24]

Additional robustness checks The baseline results are robust to alternative specification choices. First, Appendix Tables C.2, C.9 and C.10 document the robustness to (i) alternative constructions of household expenditures and the exclusion of outliers, (ii) alternative clustering of standard errors, (iii) using Nielsen's post-stratification weights.

[24]It is difficult to construct a placebo variable using only data from the Nielsen Homescan panel because it is ex-ante not clear whether a particular product category should be unaffected by the impact of the radio show. For example, households could decrease their expenditures by using goods more efficiently and thus reducing waste.

Second, Appendix Figures C.2 and C.5 document the robustness of the event-study approach to (i) the choice of control variables, (ii) state-specific trends, or (iii) replacing unit fixed effects with treatment cohort fixed effects (Imai and Kim, 2019). Third, the Nielsen Homescan sample is unbalanced for two reasons: Some households have missing purchase records for individual months, and households eventually leave the panel. While household fixed effects already account for unobserved differences, compositional changes might affect the event-study estimates. I therefore re-estimate equation 2 on a balanced sample of households by excluding never treated households with gaps in their expenditure records and households that are not observed continuously during the event window. Despite reducing the sample size substantially, Appendix Figure C.3 and C.4 show that the results are robust to these changes. Fourth, Appendix Table C.8 shows that the results are robust to excluding observations from the years following the introduction of the *Dave Ramsey Show* on other media channels, such as YouTube or satellite radio.

6 Experimental evidence

The above results reveal that the *Dave Ramsey Show* has economically large and meaningful effects on household behavior. The impact of the radio show is persistent and does not dissipate over the twelve months following market entry, which begs the question of how the radios how achieves persistent behavioral change. A distinguishing feature of the radio show is its regularly repeated narrative about consumption and debt—the notion that borrowing money and living beyond one's mean is wrong—which permeates every aspect of its three-hour program. Exposure to this narrative may cause people to revise fundamental attitudes towards consumption and debt, which would explain the persistence of behavioral change. While a multi-faceted radio program like the *Dave Ramsey Show* may also affect behavior through other channels, a mechanism based on attitudinal changes is likely to be particularly powerful. To examine the relevance of this mechanism, I conduct an experiment in which I exogenously vary whether respondents listen to the *Dave Ramsey Show* or a neutral audio recording before measuring attitudes. I provide evidence that listening to the *Dave Ramsey Show*'s narrative for *only* five minutes negatively affects people's attitudes towards consumption and debt.

6.1 Experimental design

6.1.1 Sample

The experiment was conducted in collaboration with *Lucid*, a professional survey company frequently used in social science research (Chopra et al., 2021; Haaland and Roth, 2021). To be eligible, respondents needed to reside in the US and be at least 18 years old. At the beginning of the survey, I screen out respondents that do not pass an attention check (see Appendix Figure F.2). I also screen out respondents that cannot play audio files on their devices (see Appendix Figure F.3), as this was a necessary technical requirement to administer the treatment manipulation. These exclusion criteria were preregistered (see Appendix F.1). The final sample of 1,500 respondents is broadly representative of the general population in terms of age, gender, education and region (as shown in Table F.1). Appendix Tables F.2 and F.3 present tests of balance to assess the integrity of the randomization procedure.

6.1.2 Main study

Panel A of Appendix Figure F.1 provides an overview of experimental design. The full experimental instructions can be found in Appendix Section F.4. The main experimental design was preregistered (see Appendix F.1). Respondents first answer basic demographic questions and provide information about their personal finances. Then, respondents are randomly assigned to one of three experimental conditions: a treatment group, a control group, and a robustness control group.

Experimental conditions The treatment group and the control group listen to different audio recordings, while the robustness control group proceeds without listening to anything.[25] The treatment group listens to a five minute audio recording of the *Dave Ramsey Show*, which was carefully chosen to include the major narrative elements of the show, such as the ubiquity of debt and the tendency of Americans to spend and borrow money to impress others. This allows me to mimic the experience of listening to the radio show for a longer period of time in which these elements would have naturally occurred. The control group listens to an unrelated podcast arguing that people should more carefully choose which "battles to fight" in their life. The podcast was deliberately chosen to hold many features constant, such as the total length, the gender of the speaker, the topical focus on self-help and personal improvement, and the narrator's

[25]Respondents cannot proceed to the next page for five minutes. They are told that they will have to answer some questions related to the audio recording after having finished listening to it, which serves to increase their engagement with the audio recording.

paternalistic attitude. Appendix Section F.5 contains a verbatim transcript of both audio recordings.

Obfuscation and delay Experimenter demand effects induced by the audio recording might affect response behavior in the treatment group. To address this concern, I take several steps. First, I embed an obfuscation module directly after the audio recording. This module contains questions that mimic standard consumer research surveys, such as whether they would be more likely to listen to a particular radio station if it featured similar content. Second, I implement a "cool-off" period of about three minutes before measuring respondents' attitudes towards consumption and debt. Specifically, I elicit additional demographics, administer the "Big 5" financial literacy module (Hastings et al., 2013), and measure demand for information about personal finances. Respondents should thus be uncertain about the primary interest of the study, which are attitudes towards consumption and debt—as specified in the pre-analysis plan.

Outcome To measure attitudes towards debt, I elicit respondents' agreement with four items from Davies and Lea's (1995) validated debt attitude scale. These items contain negative statements about debt, such as "There is no excuse for borrowing money." To measure attitudes towards consumption, I use two items from Richins and Dawson's (1992) validated materialism scale: "I admire people who own expensive homes, cars, and clothes" and "The things I own say a lot about how well I'm doing in life." Respondents' agreement with these items is measured on a 5-point Likert scale from "strongly agree" to "strongly disagree". For my primary analysis, I construct a (pro-)debt attitude index and a (pro-)consumption attitude index by summing responses to these items. Both indices are then z-scored using the control group mean and standard deviation. When estimating treatment effects on individual items, I recode answers such that larger values coincide with stronger agreement.

6.1.3 Follow-up survey

To shed light on the persistence of treatment effects over time, I conduct an obfuscated follow-up survey exactly one-week after the main experiment without administering any additional experimental treatments. I obfuscate the link between the main experiment and the follow-up survey by using a different survey layout and consent form, again eliciting basic demographics and including an additional obfuscation module measuring people's satisfaction with their primary bank. I then re-elicit attitudes towards consumption and debt using the original instructions from the main experiment. I managed to recontact 522 respondents, which corresponds to a recontact rate of 35%. Appendix Table F.9 documents balanced baseline covariates in the follow-up survey, and Appendix Table F.8 shows that there is no differential attrition across treatment arms.

6.2 Experimental results

Treatment effects Table 6 documents the main result that the treatment effect of listening to the *Dave Ramsey Show* for five minutes causally affects people's attitudes towards consumption and debt.[26] In the main experiment, treated respondents have 53% of a standard deviation more negative attitudes towards debt and borrowing money compared to respondents in the control group ($p < 0.01$, column 1). They also have 24% of a standard deviation more negative attitudes towards consumption ($p < 0.01$, column 2). The magnitudes of the treatment effects are economically large and suggest that narratives embedded in mass media programs have the power to substantially affect people's attitudes. The effects are robust to using respondents who did not listen to an audio recording as a comparison group (columns 3–4), suggesting that the treatment effects are not an artifact of the audio recording used in the control group.

In the obfuscated one-week follow-up survey, I find that treated respondents still hold 30% of a standard deviation more negative attitudes towards debt compared to control group respondents ($p < 0.01$, column 5).[27] This corresponds to 57% of the original effect size. Similarly, I still find a negative effect of 21% of a standard deviation on consumption attitudes ($p < 0.05$, column 6), which is an economically large effect in light of the minimalist intervention of listening to the radio show for a mere five minutes in the previous week. Appendix Section F.3 provides additional results from secondary outcomes suggesting that the effect of the radio show is driven primarily by changes in attitudes.

Attitudes and behavior The experimental findings raise a question about downstream effects of attitudinal changes on people's behavior, and in particular whether the treatment effect on consumption attitudes is large enough to explain the decrease in expenditures observed in the Nielsen panel. To address this question, I utilize correlational evidence from respondents in the control groups. Table F.4 shows that consumption and debt attitudes correlate with self-reported behavior. Specifically, having a standard

[26] I include the numerical age and age squared, log income, female indicator, and an indicator for having completed a Bachelor's degree or higher as control variables. Table F.5 provides estimates without control variables. Appendix Table F.6 shows that treatment effects on attitudes are not driven by individual items used to construct the indices.

[27] In the follow-up survey, I pool recontacted respondents from both control groups to maximize statistical power. I obtain quantitatively similar effect sizes without pooling these experimental groups. The results are robust to using inverse probability of attrition weights obtained from regressing a binary attrition indicator on a comprehensive set of baseline covariates (as shown in Appendix Table F.7).

Table 6: Treatment effects on attitudes across studies

	Main study		Robustness: Passive control		One-week follow-up	
	(1) Debt attitudes	(2) Consumption attitudes	(3) Debt attitudes	(4) Consumption attitudes	(5) Debt attitudes	(6) Consumption attitudes
Treatment	-0.530***	-0.237***	-0.603***	-0.230***	-0.303***	-0.208**
	(0.065)	(0.061)	(0.061)	(0.060)	(0.094)	(0.090)
N	962	962	1,030	1,030	522	522
z-scored	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows OLS regression estimates where the dependent variables are attitudes towards consumption and debt. The debt attitude index and the consumption attitude index are constructed as described in the main text and oriented such that larger values correspond to more positive attitudes towards the object. Both indices are normalized to have mean zero and standard deviation one. "Treatment" is a binary indicator taking value one for respondents who listened to a five minute recording from the *Dave Ramsey Show*. Columns 1 and 2 use data from the main experiment, focusing on the subset of respondents assigned to the treatment group and the control group. Columns 3 and 4 focus on respondents from the main experiment that were assigned to the treatment group or the robustness control group. Columns 5 and 6 use data from the one-week follow-up survey and pools respondents from both control group conditions (neutral podcast and no audio) as a joint control group. Control variables include numerical age and age squared, log income, female indicator, an indicator for having completed a Bachelor's degree or higher, and region indicators. Robust standard errors are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

deviation more positive attitudes towards debt is associated with a 39% increase in personal debt ($p < 0.05$, column 1) and a 4.5 percentage points lower probability of having no debt ($p < 0.01$, column 3). Consumption attitudes are associated with a 17% increase in past spending on food, restaurants and leisure activities per standard deviation ($p < 0.01$, column 5). These correlations are robust to including sociodemographic controls (columns 2, 4, 6).[28] Assuming that the correlation of 0.1 between consumption attitudes and past expenditures in column 6 of Appendix Table F.4 is causal, the 20.8% of a standard deviation decrease in consumption attitudes in the follow-up survey would imply a decrease in expenditures of about 2%, which is in the ballpark range of the observed effect of 1.3–1.6%. This back-of-the-envelope calculation provides additional support for changes in attitudes as a key mechanism through which the *Dave Ramsey Show* affects household behavior.

[28]Reassuringly, the attitudinal measures capture conceptually distinct facets: Consumption attitudes do not correlate with debt conditional debt attitudes (columns 1 and 3 of Appendix Table F.4), while debt attitudes do not correlate with spending conditional on consumption attitudes (column 5).

7 Concluding remarks

This paper provides causal evidence of mass media persuasion in the core economic domain of consumption and savings decisions. Specifically, I show that exposure to a popular US radio talk show arguing that Americans overspend and under-save causes people to decrease their consumption. To identify the causal impact of the radio show, I exploit quasi-natural variation in the availability of the radio show created by its staggered expansion from 2004 to 2019.

I provide three main results. First, I document that exposure to the radio show decreases household expenditures. Event-study estimates suggest that the effect of the radio show is not short-lived and instead persists for at least one year after the local introduction of the show. Second, I examine how households decrease their expenditures. My evidence suggests that the decrease in expenditures is best explained by households purchasing fewer products rather than exerting more effort to decrease the price of their current basket of goods. Third, I shed light on the underlying mechanism using a pre-registered experiment. I find that exposure to the radio show's message for a mere five minutes has an economically large and persistent, negative effect on people's attitudes towards consumption and debt.

My findings inform the debate on which policies are likely to be effective in mobilizing savings efforts. The evidence from the *Dave Ramsey Show* suggests that people act on the financial advice provided by mass media programs. Specifically, households are responsive to repeated messages on mass media advocating savings behaviors and cautioning against household debt. The finding that the radio show has larger effects among households with initially high expenditures relative to their income further suggests that the *Dave Ramsey Show* might have had positive effects from a welfare perspective.

This suggests that entertaining mass media are a promising avenue for behavioral change interventions aimed at improving financial outcomes. Financial advice on entertaining mass media programs, such as the *Dave Ramsey Show*, can reach millions of people on a regular basis at comparatively low marginal cost compared to other approaches such as classroom-based financial education programs. Moreover, entertaining mass media programs may appeal to people that are otherwise difficult to reach because of lacking interest in household finance.

However, effectively leveraging the power of mass media for behavioral interventions is not without its own limitations. For instance, it requires access to and collaboration with media production firms to tap their knowledge on how to design a product that is entertaining enough to appeal to a broad audience, while at the same time including carefully crafted messages aimed at behavioral change. This naturally constrains the type of information that can be disseminate through mass media. Whereas other channels might be better suited to teach intricate and detailed financial concepts, my evidence suggests that mass media can be used to raise awareness and change people's attitudes towards important issues such as insufficient retirement savings. Mass media interventions are hence best utilized in concert with a broader mix of policies and interventions aimed at improving financial outcomes.

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Online Appendix for

MEDIA PERSUASION AND CONSUMPTION: EVIDENCE FROM THE DAVE RAMSEY SHOW

Felix Chopra

November 15, 2021

Summary of the Online Appendices

Appendix Section A contains details about the data sources and the construction of variables. Section A.2 provides additional information on the data and procedures used to obtain the radio coverage indicator.

Appendix Section B contains additional descriptive material. Figure B.1 indicates the number of new affiliates by year. Figure B.2 shows the Google Trend's popularity of the *Dave Ramsey Show* and *Sean Hannity* over time. Figure B.3 presents the spatial distribution of affiliated radio stations across the US. Figure B.4 and Figure B.5 show DMA-level summary statistics for the number of Nielsen panelists and household expenditures.

Appendix Section C contains additional robustness checks. Figure C.1 presents event-study estimates using the imputation estimator proposed by Borusyak et al. (2021).

Appendix Section D contains additional analyses. Figure D.1 presents the distribution of effects on household expenditures obtained from a repeated assignment of counterfactual market entry dates. Table D.1 shows estimates of the effect of the radio show on political outcomes.

Appendix Section E contains additional results related to the content analysis. Section E.1 presents qualitative evidence on the radio show's narrative, advice and content. Section E.2 presents quantitative evidence from text analysis of about 3,000 hours of content uploaded by the *Dave Ramsey Show* on its YouTube channel. Section E.2.1 presents the YouTube data and how I prepare the text data for text analysis. Section E.2.2 contains topic model estimates from Latent Dirichlet Allocation. Section E.2.3 contains additional results on the most frequently spoken non-stopwords and keywords used to describe the videos (Table E.1) and the top correlates of the word "debt" (Figure E.2).

Appendix Section F provides supplementary material for the experimental part of the paper (discussed in Section 6). I provide information about research transparency in Section F.1, including a discussion of the preregistration, ethical approval, data and code availability, and a declaration of no conflict of interest. Section F.2 contains additional figures and tables. In particular, a design overview (Figure F.1), a comparison of sample characteristics to the general population (Table F.1), a test of balance (Table F.2), a test of balance for demographics elicited post-treatment (Table F.3). The correlation between consumption and debt attitudes and self-reported behavior are shown in Table F.4. Table F.5 presents the main results without the inclusion of control variables. Table F.6 presents treatment effects on individual items used to construct the consumption and debt attitude indices in the main experiment. Table F.7 presents treatment effects on individual items used to construct the consumption and debt attitude indices in the follow-up survey. Table F.8 tests for differential attrition across treatment arms between the main experiment and the follow-up survey. Table F.9 presents a test of balance of covariates across treatment arms in the follow-up survey. Section F.4 and Section F.6 contain the original instructions used in the main experiment and the obfuscated follow-up survey, respectively.

A Data

A.1 Data sources

Table A.1: Data sources

Variables	Source	Comment
Dependent variable		
Household expenditures, number of products purchased, other household-level outcomes based on UPC-level purchase records	Nielsen Homescan Data	Monthly household-level statistics result from aggregating purchase records across individual shopping trips
Radio coverage		
Signal strength, free-space signal strength	Own calculations	Derived from an implementation of the Longley-Rice/Irregular Terrain Model
Radio coverage	Ramsey Media, own calculations	Construction as described in Section 3, combining signal strength measures and information about the timing of market entry. This variable varies at the zip code-month level.
Control variables		
Household-level covariates	Nielsen Homescan Data	Self-reported sociodemographic variables, elicited each fall
Unemployment rate	US Bureau of Labor Statistics	The unemployment rate varies at the county-month level
Urbanization	US Census Bureau	Share of the zip code population living in urban areas. Based on data from the H002 Urban and Rural Summary File 1.
House prices	Zillow Group	This is the Zillow Home Price Index. Data series are obtained at the zip code and the county-month level. Available at: https://www.zillow.com/research/data/

Variables	Source	Comment
Christian share	US Religion Census	US Religion Census: Religious Congregations and Membership Study, 2010 (County File), accessed: October 2019.
County-level demographics	US Census (2000, 2010), American Community Survey	Vary at the county-year level
	Radio transmitter characteristics	
Transmitter height, power, frequency, and location	Federal Communications Commission (FCC)	Power in kilowatt, height in meter, frequency in MHz and coordinates of the transmitter in NAD83 coordinates. Data obtained using the AM and FM Query tools available at: https://www.fcc.gov/licensing-databases/search-fcc-databases; accessed February 2019
	Geographical variables	
State, county and zip code boundaries	US Census Bureau	Shapefiles for state, county and ZCTA representation of 5-digit zip codes (1:500k) in WGS84 coordinates. Data available at: https://www2.census.gov/geo/tiger/GENZ2017/shp/
Boundaries for Designated Market Areas (DMAs)	Nielsen	Based on a cross-walk from Designated Market Areas to US counties available from Nielsen.
Latitude and longitude of the geographic center of administrative units		Derived from the corresponding shapefiles using the Python package `geopandas` after applying a distance-preserving projection
Terrain elevation	Global Land One-km Base Elevation Project (GLOBE)	Height above mean sea levels (in meters). Available at: https://www.ngdc.noaa.gov/mgg/topo/globe.html; accessed October 2020
	Other variables	
Political outcomes (turnout, vote shares)	MIT Election Data and Science Lab (2018)	County-level electoral results for the Presidential elections between 2000–2016

A.2 Radio coverage

This section provides additional details on how I determined the spatial radio coverage of affiliated radio stations.

The information on the affiliated radio stations of the *Dave Ramsey Show* included their current their call sign, frequency, and the DMA, state and city where the radio station is located. However, radio stations often change their call sign when they switch to a new format. To obtain time-invariant identifiers, I manually match all affiliated radio stations with the FCC transmitter identifier of their primary transmitter ("Facility ID"). Moreover, many radio stations operate multiple transmitters in different locations to increase their service area and provide better radio coverage. For all affiliated stations, I thus obtain a complete list of their secondary transmitters from the FCC, including the exact date when the secondary transmitter started to broadcast. In my analysis, I include the radio coverage of secondary transmitters after the latter of (i) the date when their primary transmitter started to broadcast the radio show and (ii) the date the secondary transmitter actually started to broadcast.

For each transmitter, I then obtain the geographic coordinates of their location and the technical parameters needed for the signal propagation models. In the case of the Longley-Rice/Irregular Terrain Model, these parameters include the effectively radiated power (in kilowatts), the height of the transmitter antenna above ground levels (in meters), and the broadcast frequency (in MHz). The model also requires topographic information on the elevation profile to account for the effect of obstructions that block line-of-sight transmission. I use data from the Global 30 Arc-Second Elevation Database.

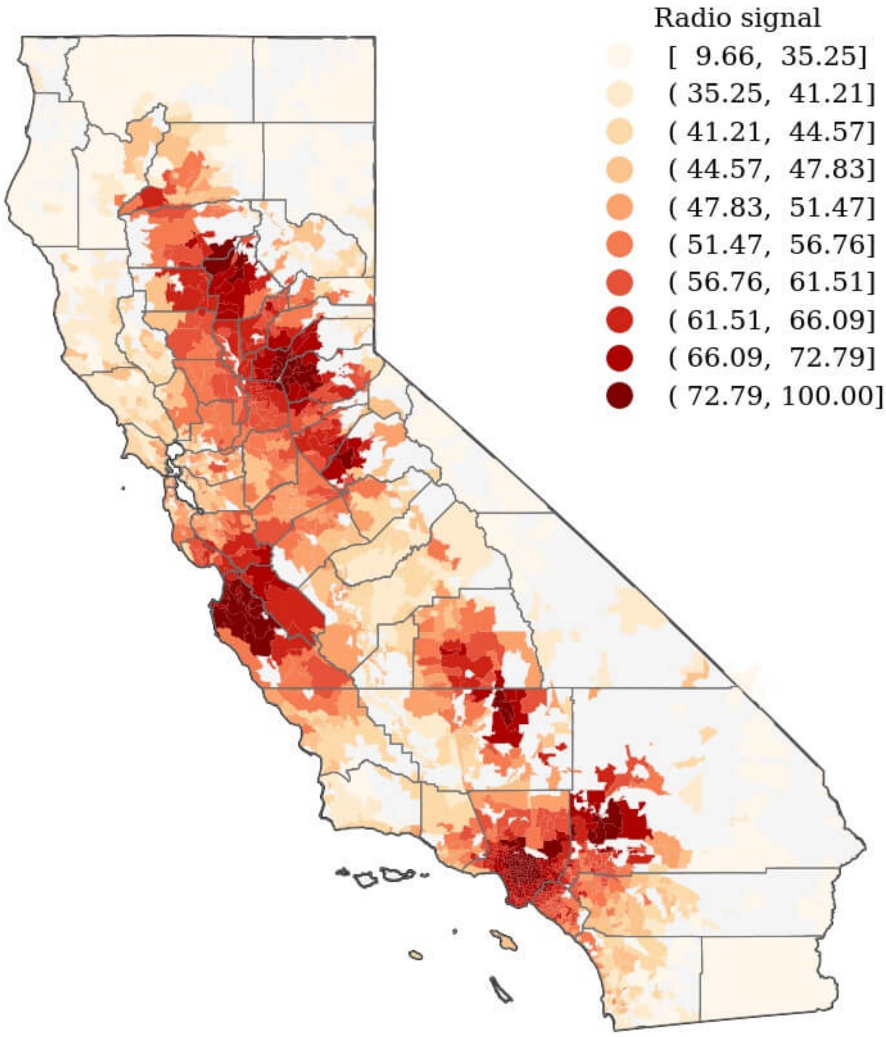
I then use the Longley-Rice/Irregular Terrain Model to calculate the path loss (in dB) between pairs of receiver and transmitter locations. The program code was obtained from Benjamin Olken. As the residency of Nielsen households is known up to the 5-digit zip code, I use the geographic coordinates of the centroid of zip codes as potential receiver locations. For each transmitter, I calculate the signal loss for all zip codes within 600 km of the transmitter's location. In addition, I calculate the free-space path loss using the same parameters. I then deduct the path loss from the transmitter signal strength to obtain the receiver signal strength. Whenever a zip code receives a radio signal from multiple transmitters, I follow the literature and use the maximum receiver signal strength.

For county-level analyses, I calculate the share of the population with access to the *Dave Ramsey Show*. Specifically, I use a signal strength threshold of 50 dBμV/m to classify zip codes as having radio coverage. I calculate the share of the county population accounted for by zip codes with radio coverage.

Figure A.1 provides an example of the zip code-level variation in radio signal strength.

The figure plots radio signal strength (in deciles) in California at the end of 2012.

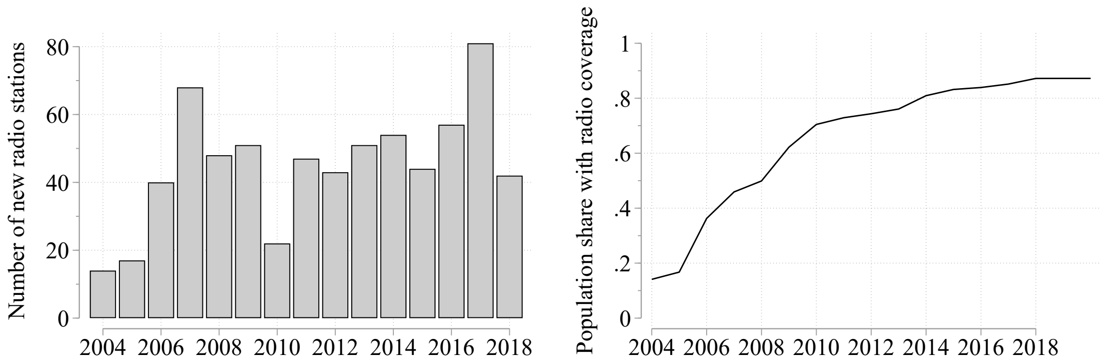
Figure A.1: Radio coverage at the zip code level: Example



Notes: This figure displays the radio signal strength (in dBμV/m) across zip codes in California as of 2012. The radio signal is the maximum signal strength across all transmitters of affiliated radio stations and capped at 100 dBμV/m.

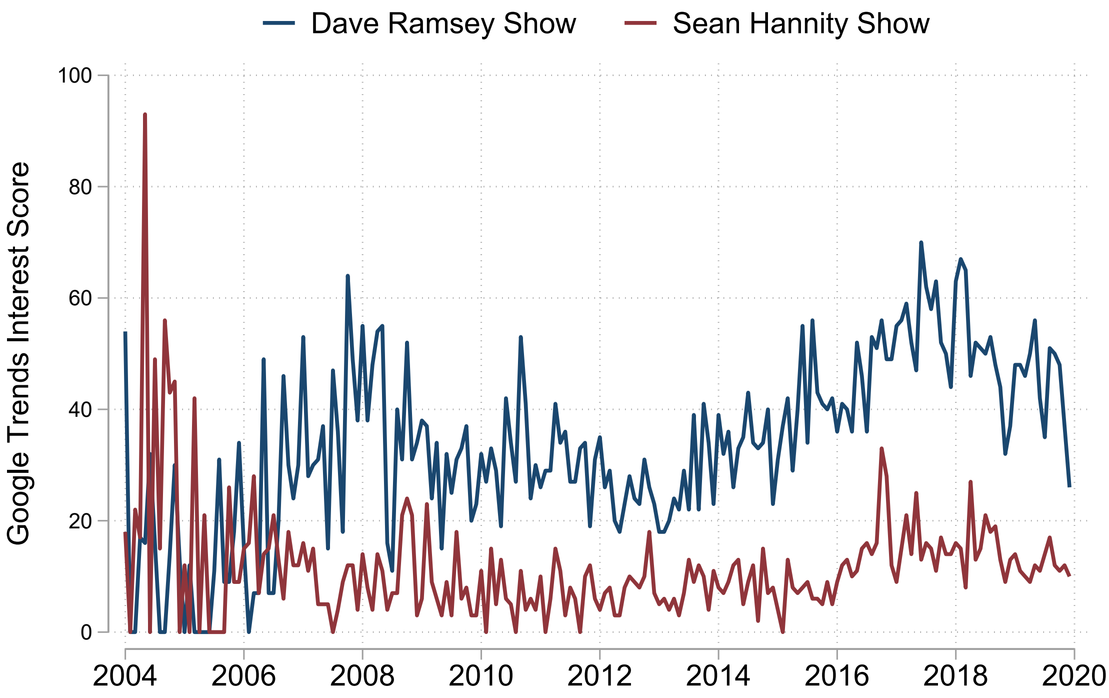
B Descriptives

Figure B.1: Expansion of the affiliate network over time



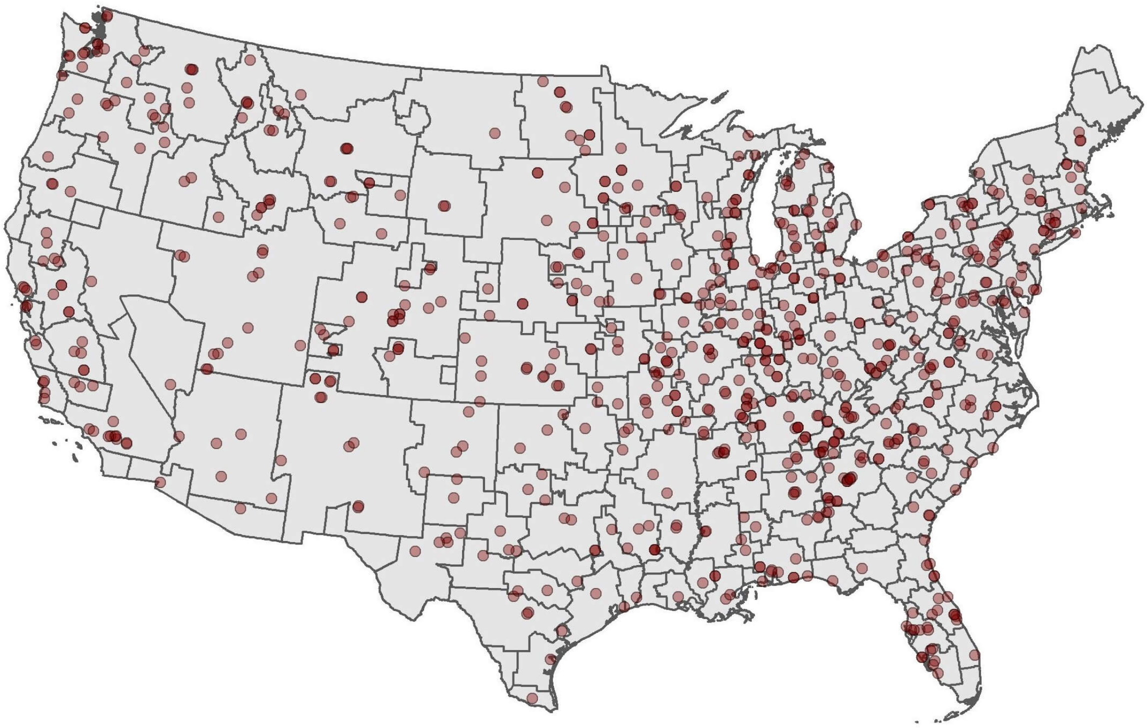
Notes: This panel on the left displays the number of new affiliated radio stations starting to broadcast the *Dave Ramsey Show* over time. The panel on the right plots the share of the US population with radio coverage from an affiliated radio stations over time.

Figure B.2: Popularity of the Dave Ramsey Show as measured by Google searches



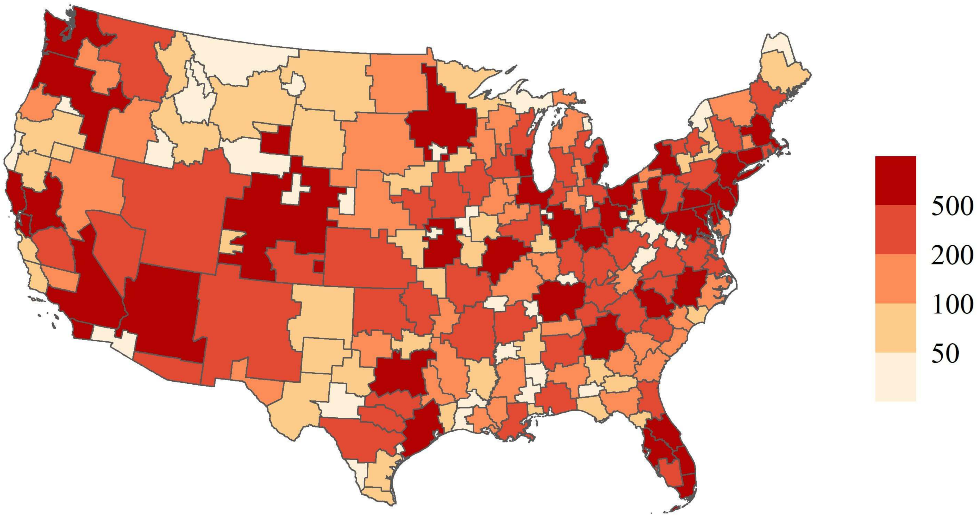
Notes: This figure uses monthly Google Trends data for the period from January 1, 2004, to December 31, 2019. For each month, the figure indicates the interested in the two topics "The Dave Ramsey Show" and "The Sean Hannity Show" as determined by Google searches related to these topics. The Google Trends data is normalized to a scale ranging from 0 to 100, where larger values indicate more searches. The data was obtained on June 17, 2021, from `https://trends.google.com`.

Figure B.3: Transmitter locations of affiliated radio stations



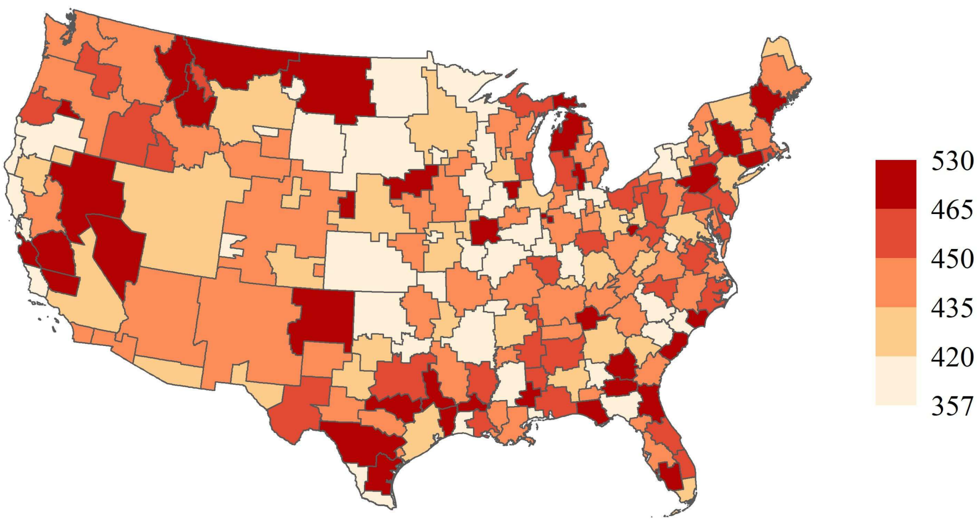
Notes: This map plots the locations of the transmitters of all radio stations broadcasting the radio show together with the boundaries of Nielsen's Designated Market Areas (DMAs).

Figure B.4: Nielsen panelists by Designated Market Area



Notes: This map shows the total number of Nielsen panelists in 2017 by Designated Market Area.

Figure B.5: Monthly expenditures by Designated Market Area



Notes: This map shows the average monthly expenditure of Nielsen panelists (in $) in 2017 by Designated Market Area.

C Robustness checks

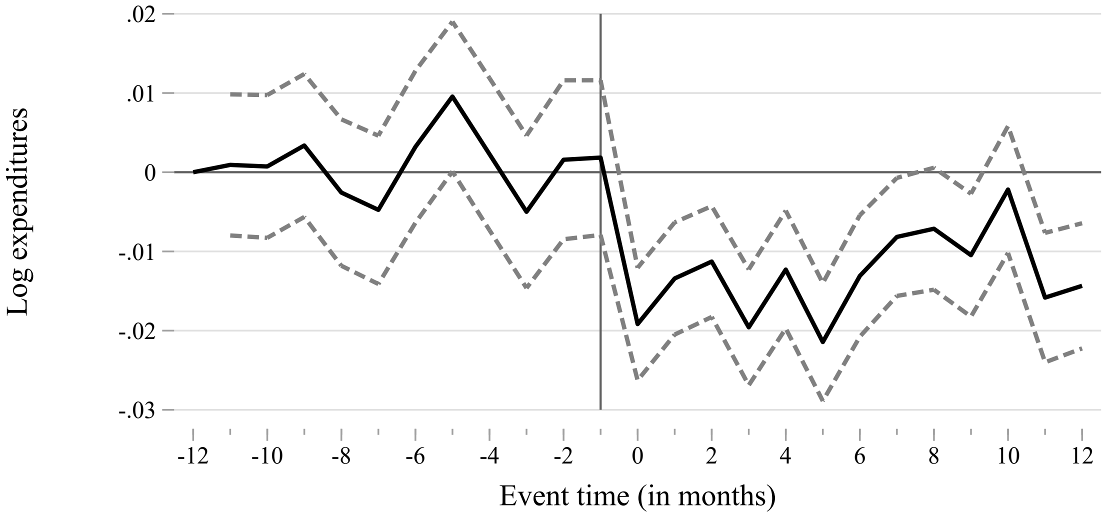
Table C.1: Poisson regression – Number of purchased items

	Dependent variable: Number of purchased products			
	(1)	(2)	(3)	(4)
Radio show	-0.0187***	-0.0182***	-0.0224***	-0.0240***
	(0.0031)	(0.0031)	(0.0031)	(0.0035)
N	3,744,054	3,744,054	3,407,688	3,407,688
Pseudo R^2	0.517	0.520	0.521	0.523
Mean of dep. var.	83.30	83.30	83.06	83.06
Household & Time FEs	Yes	Yes	Yes	Yes
Household controls		Yes	Yes	Yes
Local economic conditions			Yes	Yes
State x Time FEs				Yes

Notes: This table show Poisson regression estimates using 2004–2019 Nielsen Homescan data at the household-by-month level. The dependent variable is the number of purchased items per month. "Radio show" is a binary indicator taking value one after local market entry of the *Dave Ramsey Show*. Individual controls include the log of household income, age indicators, household size, married indicator and employment status indicators (full-time, part-time, unemployed). Local economic conditions comprise controls for house prices and the unemployment rate. Robust standard errors clustered by zip code are shown in parentheses.

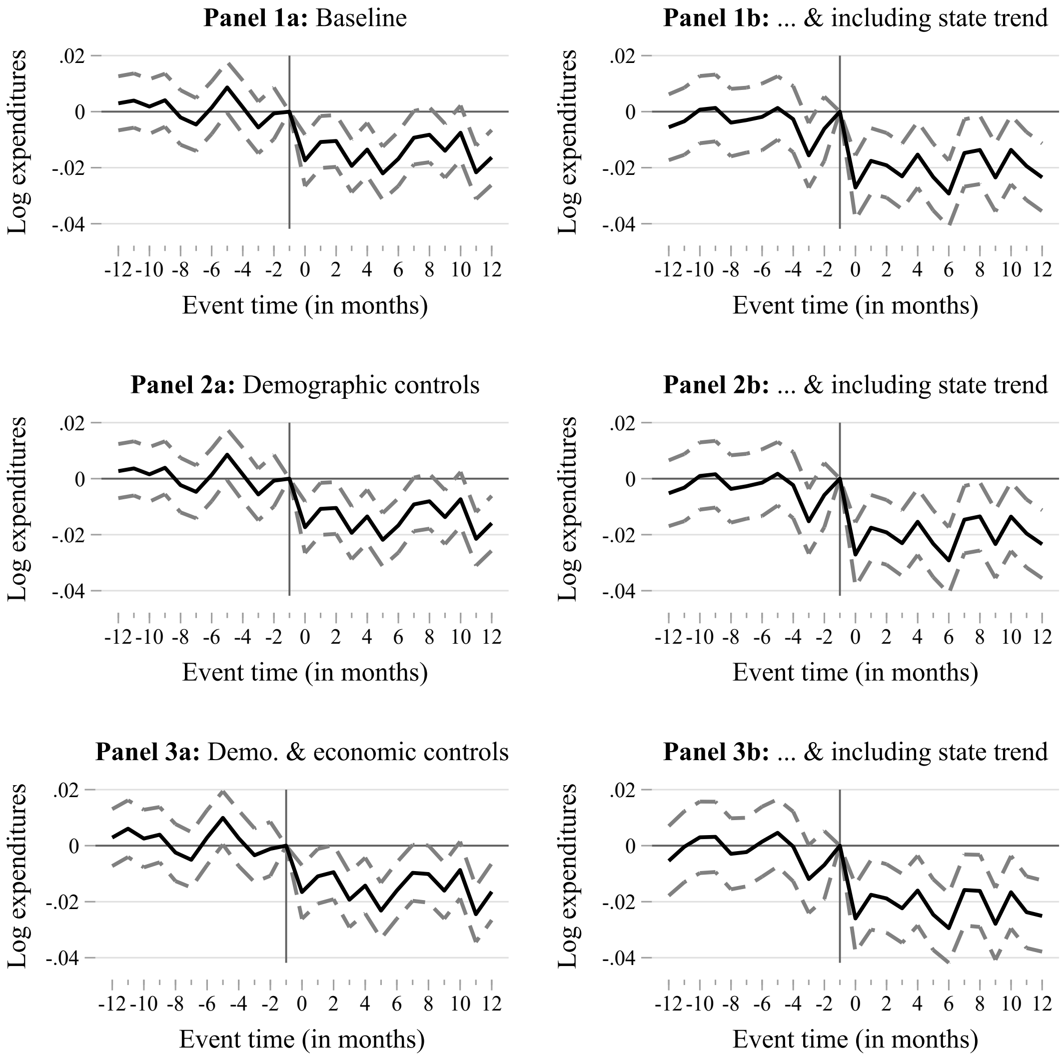
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.1: Robustness to treatment effect heterogeneity: Borusyak et al. (2021) imputation estimator



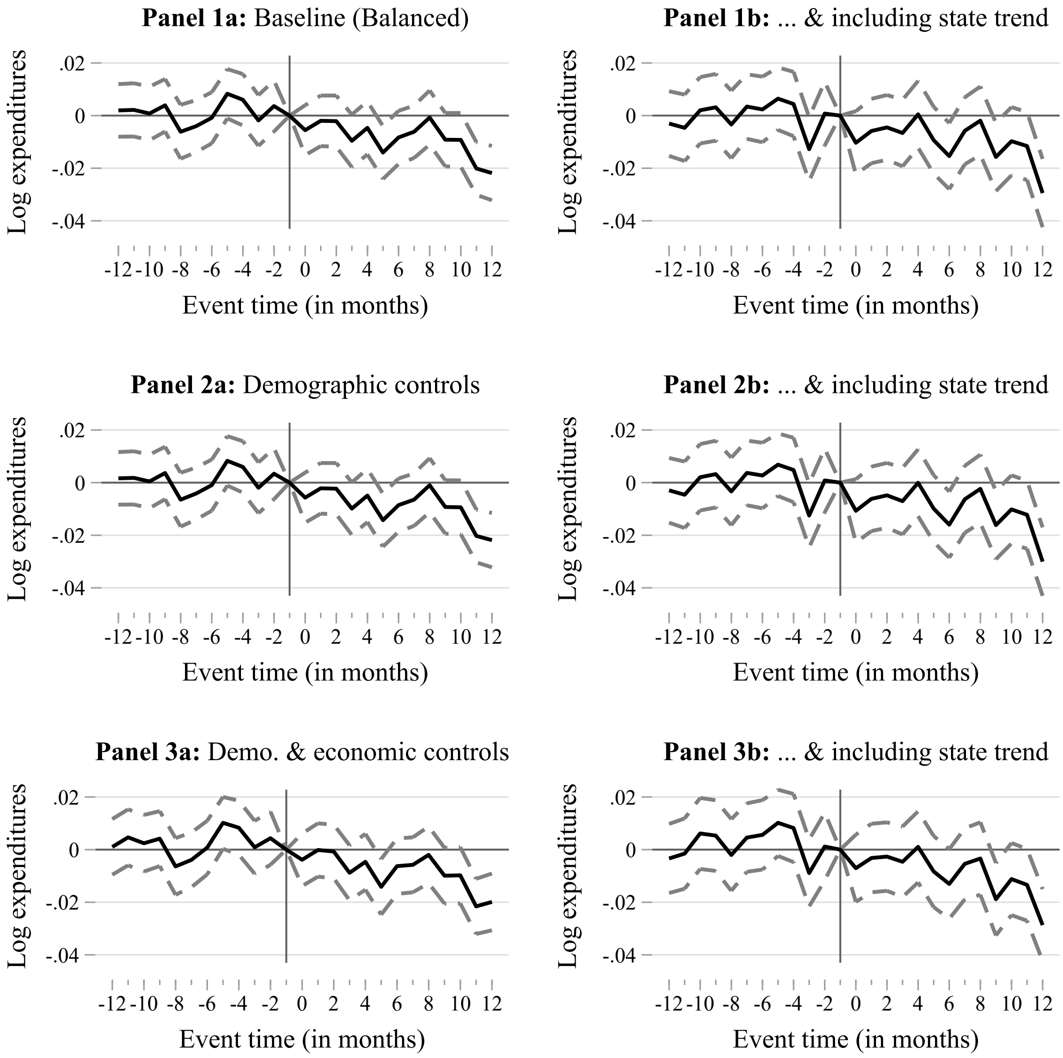
Notes: This figure uses 2004–2019 Nielsen Homescan data at the household-by-month level. The dependent variable are log expenditures. The omitted category is 12 months before market entry. Estimates of the treatment effect dynamics are obtained from the imputation estimator proposed by Borusyak et al. (2021). The estimator includes household and year-month fixed effects. 95% confidence intervals are constructed from robust standard errors clustered at the zip code level.

Figure C.2: Robustness: Control variables and state-specific time trends



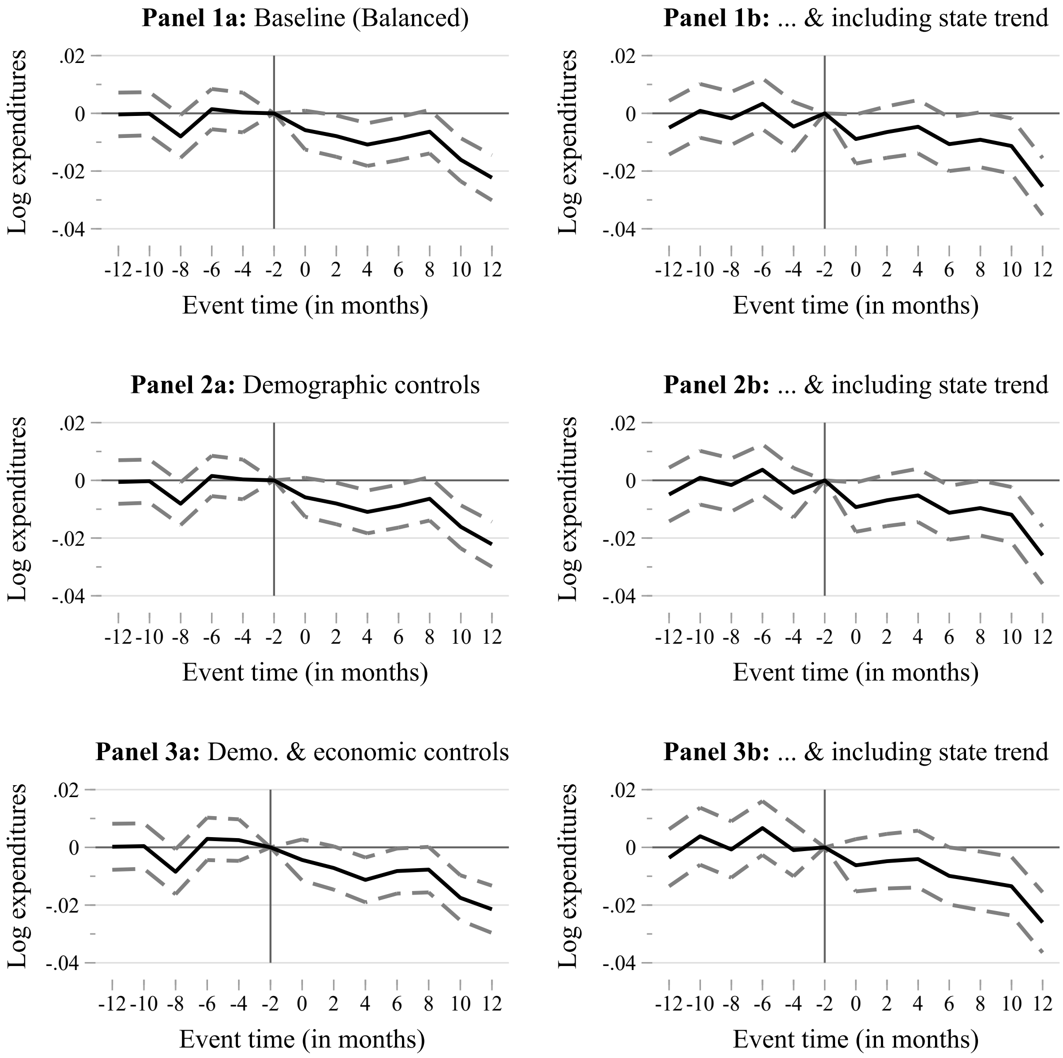
Notes: This figure uses 2004–2019 Nielsen Homescan data at the household-by-month level. The dependent variable are log expenditures. The baseline specification in Panel 1a includes event time indicators, household fixed effects and year-month fixed effects. The month before market entry serves as the omitted category. 95% confidence intervals are constructed from robust standard errors clustered at the zip code level. Panel 1b, 2b and 3b include state×year-month fixed effects to the specification in Panel 1a, 2a and 3a, respectively. Panel 2a includes time-varying household-level demographic controls. Panel 3a includes time-varying household-level demographic controls and proxies for local economic conditions, including monthly house prices (zip code) and the monthly unemployment rate (county level).

Figure C.3: Robustness: Balanced sample



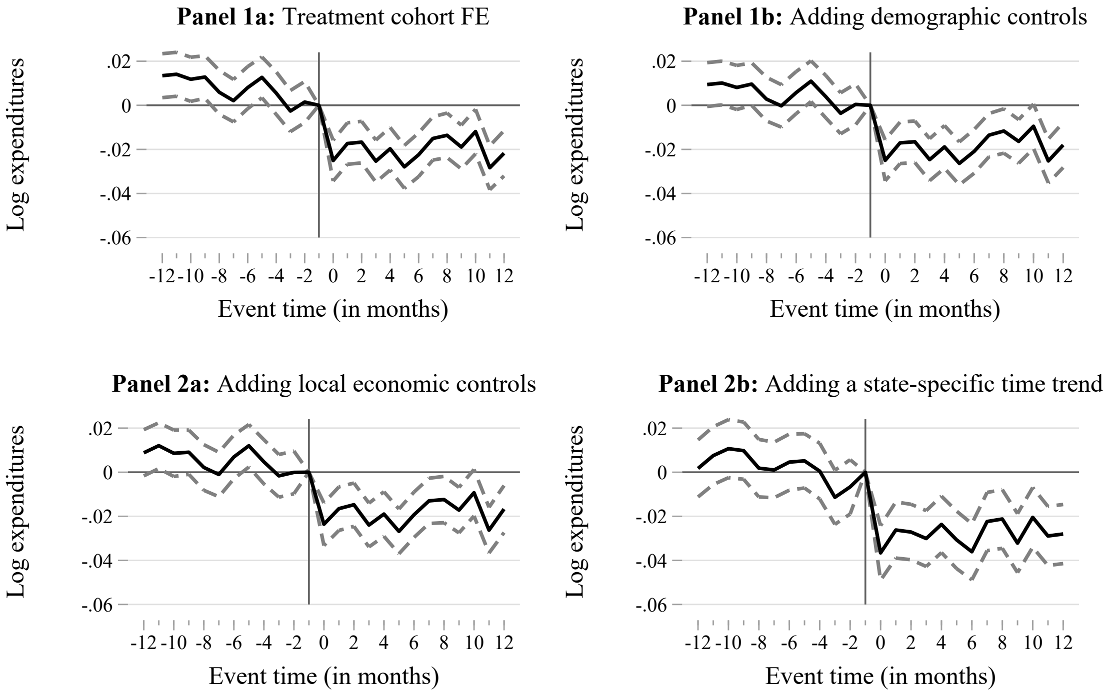
Notes: This figure uses 2004–2019 Nielsen Homescan data at the household-by-month level. All panels use a balanced sample in event time. The dependent variable are log expenditures. The baseline specification in Panel 1a includes event time indicators, household fixed effects and year-month fixed effects. The month before market entry serves as the omitted category. 95% confidence intervals are constructed from robust standard errors clustered at the zip code level. Panel 1b, 2b and 3b include state×year-month fixed effects to the specification in Panel 1a, 2a and 3a, respectively. Panel 2a includes time-varying household-level demographic controls. Panel 3a includes time-varying household-level demographic controls and proxies for local economic conditions, including monthly house prices (zip code) and the monthly unemployment rate (county level).

Figure C.4: Robustness: Balanced sample with binned event time indicators



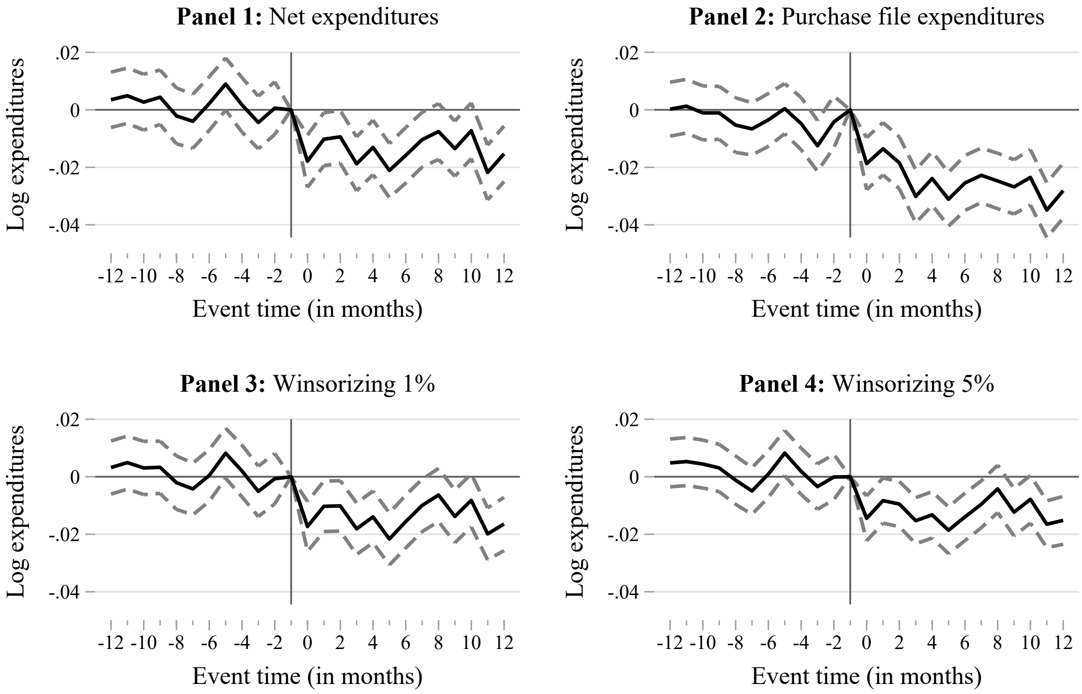
Notes: This figure uses 2004–2019 Nielsen Homescan data at the household-by-month level. All panels use a balanced sample in event time. The dependent variable are log expenditures. The baseline specification in Panel 1a includes event time indicators, household fixed effects and year-month fixed effects. The month before market entry serves as the omitted category. 95% confidence intervals are constructed from robust standard errors clustered at the zip code level. Panel 1b, 2b and 3b include state×year-month fixed effects to the specification in Panel 1a, 2a and 3a, respectively. Panel 2a includes time-varying household-level demographic controls. Panel 3a includes time-varying household-level demographic controls and proxies for local economic conditions, including monthly house prices (zip code) and the monthly unemployment rate (county level).

Figure C.5: Robustness: Treatment cohort instead of household fixed effects



Notes: This figure uses 2004–2019 Nielsen Homescan data at the household-by-month level. The dependent variable are log expenditures. All regressions include event time indicators and year-month fixed effects. Moreover, all regressions include treatment cohort fixed effects (defined by the year-month a household is first treated) and zip code fixed effects instead of household fixed effects. 95% confidence intervals are constructed from robust standard errors clustered at the zip code level. Panel 1b adds time-varying household-level demographic controls to the specification from Panel 1a. Panel 2a further adds proxies for local economic (house prices, unemployment rate) to the set of control variables. Panel 2b includes the full set of controls and state×year-month fixed effects.

Figure C.6: Event study: Alternative measures of household expenditures



Notes: This figure uses 2004–2019 Nielsen Homescan data at the household-by-month level. All regressions include event time indicators, household and year-month fixed effects, the full set of controls, and state×year-month fixed effects. 95% confidence intervals are constructed from robust standard errors clustered at the zip code level. Panels differ in how monthly household expenditures are constructed. Panel 1 uses monthly expenditures net of the value of redeemed coupons as the dependent variable. Panel 2 uses the sum of all expenditures recorded in the Nielsen Homescan purchase files, excluding data supplied to Nielsen from retailers. Panel 3 and 4 winsorize household expenditures at the 1% and 5% level, respectively.

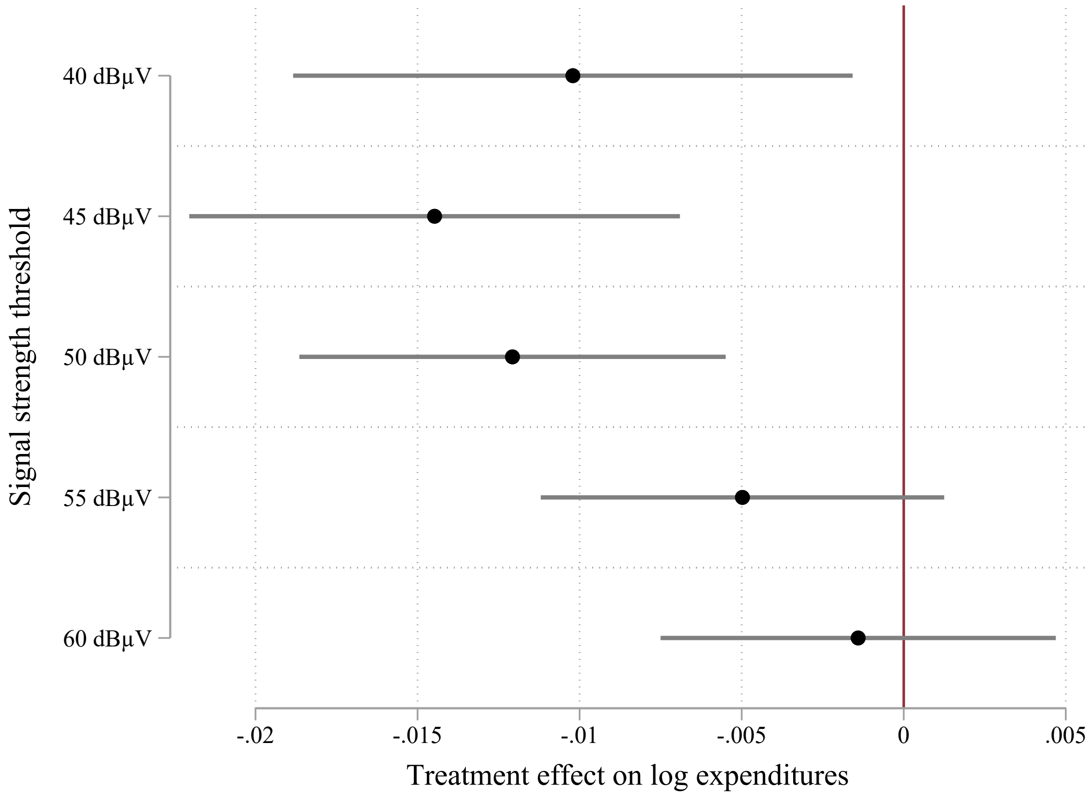
Table C.2: Robustness to using alternative measures of household expenditures

	(1) Net expenditures	(2) Purchase file expenditures	(3) Winsorizing 1%	(4) Winsorizing 5%
Radio coverage	-0.016***	-0.032***	-0.016***	-0.015***
	(0.003)	(0.003)	(0.003)	(0.002)
N	3,399,591	3,399,566	3,407,700	3,407,700
R^2	0.527	0.551	0.537	0.549
Mean of dep. var.	6.169	5.639	6.190	6.201
Household & Time FEs	Yes	Yes	Yes	Yes
Household controls	Yes	Yes	Yes	Yes
Local economic conditions	Yes	Yes	Yes	Yes

Notes: This table shows OLS regression estimates where the unit of observation is a household-month. The dependent variable is the log of household expenditures, where expenditures are constructed as indicated by the column header. Specifically, column 1 uses monthly expenditures net of the value of redeemed coupons. Column 2 uses the sum of all expenditures recorded in the Nielsen Homescan purchase files, excluding data supplied to Nielsen from retailers. Columns 3 and 4 winsorize the household expenditures at the 1% and 5% level, respectively. Robust standard errors clustered at the zip code level are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.7: Robustness – Alternative signal strength thresholds



Notes: This figure plots estimates of the baseline model (equation 1) using alternative thresholds to binarize the continuous signal strength measure. The dependent variable are log household expenditures. All regressions include household and year-month fixed effects, state×year-month fixed effects and the set of time-varying controls. 95% confidence intervals are constructed from robust standard errors clustered at the zip code level.

Table C.3: Robustness – Excluding households based on when they receive radio coverage

	Excluded treatment cohorts:							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	04/05	06/07	08/09	10/11	12/13	14/15	16/17	18/19
Radio coverage	-0.013***	-0.015***	-0.012***	-0.012***	-0.011***	-0.012***	-0.012***	-0.013***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
N	3,744,066	3,020,964	3,462,420	3,583,859	3,641,719	3,511,737	3,720,165	3,696,707
R^2	0.518	0.520	0.518	0.518	0.518	0.517	0.518	0.517
Household FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows OLS regression estimates where the unit of observation is a household-month. The dependent variable in all regressions are log household expenditures. Robust standard errors clustered at the zip code level are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: Robustness: Log expenditures – Varying the sample of DMAs

	Dependent variable: log (Expenditures)						
	Excluded DMA ranks				Included DMA ranks		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	1–50	51–100	101–150	150–210	1–50	51–100	101–210
Radio coverage	-0.010**	-0.012***	-0.015***	-0.013***	-0.014***	-0.015**	-0.002
	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.008)
N	1,209,747	3,006,281	3,447,726	3,568,444	2,534,319	737,785	471,962
R^2	0.521	0.517	0.518	0.518	0.517	0.523	0.517
Household FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows OLS regression estimates where the unit of observation is a household-month. The dependent variable in all regressions are log household expenditures. Robust standard errors clustered at the zip code level and shown in parentheses. Nielsen DMA market rankings are from 2017.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Robustness: Log items – Varying the sample of DMAs

	Dependent variable: log (Number of purchased items)						
	Excluded DMA ranks				Included DMA ranks		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	1–50	51–100	101–150	150–210	1–50	51–100	101–210
Radio coverage	-0.024***	-0.013***	-0.017***	-0.017***	-0.014***	-0.030***	-0.014*
	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.007)	(0.008)
N	1,206,284	2,998,991	3,439,762	3,559,606	2,528,597	735,890	470,394
R^2	0.538	0.540	0.542	0.542	0.541	0.542	0.532
Household FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows OLS regression estimates where the unit of observation is a household-month. The dependent variable in all regressions is the log of the number of purchased products per month. Robust standard errors clustered at the zip code level and shown in parentheses. Nielsen DMA market rankings are from 2017.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Robustness: Expenditures – Excluding counties with affiliates and areas close to Nashville

	Dependent variable: log (Expenditures)					
	Drop zip codes close to Nashville		Drop counties with affiliate stations		Apply both restrictions	
	(1)	(2)	(3)	(4)	(5)	(6)
Radio coverage	-0.013***	-0.012***	-0.013***	-0.009**	-0.013***	-0.011**
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)
N	3,345,355	3,048,109	2,314,720	2,036,495	2,050,384	1,804,011
R^2	0.519	0.525	0.520	0.527	0.521	0.529
Mean of dep. var.	6.190	6.191	6.186	6.187	6.191	6.192
Household & Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Household controls		Yes		Yes		Yes
Local economic conditions		Yes		Yes		Yes
State x Time FEs		Yes		Yes		Yes

Notes: This table shows OLS regression estimates where the unit of observation is a household-month. The dependent variable in all regressions are log household expenditures. Columns 1–2 exclude households residing in zip codes within 500 km of Nashville, Tennessee. Columns 3–4 exclude all households that reside in a county with a radio station that broadcasts the *Dave Ramsey Show* at some point. Columns 5–6 apply both restrictions. Robust standard errors clustered at the zip code level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: Robustness: Log items – Excluding counties with affiliates and areas close to Nashville

	Dependent variable: log (Number of purchased products)					
	Drop zip codes close to Nashville		Drop counties with affiliate stations		Apply both restrictions	
	(1)	(2)	(3)	(4)	(5)	(6)
Radio coverage	-0.016***	-0.021***	-0.019***	-0.020***	-0.017***	-0.019***
	(0.003)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)
N	3,337,267	3,040,998	2,309,039	2,031,659	2,045,509	1,799,901
R^2	0.542	0.549	0.542	0.551	0.542	0.550
Mean of dep. var.	4.182	4.179	4.204	4.201	4.198	4.194
Household & Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Household controls		Yes		Yes		Yes
Local economic conditions		Yes		Yes		Yes
State x Time FEs		Yes		Yes		Yes

Notes: This table shows OLS regression estimates where the unit of observation is a household-month. The dependent variable in all regressions is the log of the number of purchased products per month. Columns 1–2 exclude households residing in zip codes within 500 km of Nashville, Tennessee. Columns 3–4 exclude all households that reside in a county with a radio station that broadcasts the *Dave Ramsey Show* at some point. Columns 5–6 apply both restrictions. Robust standard errors clustered at the zip code level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Robustness: Availability of the radio show on other channels

	Excluding years after joining:		
	(1) 2016 SiriusXM	(2) 2015 Everydollar	(3) 2013 YouTube
Panel A: Log expenditures			
Radio coverage	-0.011***	-0.010***	-0.010***
	(0.003)	(0.003)	(0.003)
N	3,248,939	2,935,565	2,604,519
R^2	0.528	0.534	0.541
Mean of dep. var.	6.182	6.180	6.175
Household FEs	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes
Panel B: Log items			
Radio coverage	-0.010***	-0.007**	-0.006**
	(0.003)	(0.003)	(0.003)
N	3,240,312	2,927,445	2,597,187
R^2	0.557	0.566	0.575
Mean of dep. var.	4.188	4.188	4.190
Household FEs	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes

Notes: This table shows OLS regression estimates where the unit of observation is a household-month. The dependent variable in Panel A is the log of monthly household expenditures. The dependent variable in Panel B is the log of the number of purchased items. Columns exclude all observations after the point in time when the *Dave Ramsey Show* launched on the channel indicated in the column header. The radio show launch on SiriusXM in November 2016. It launched EveryDollar.com in March 2013. Robust standard errors clustered at the zip code level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: Robustness – Alternative clustering of standard errors

	Robust standard errors clustered at the level of:			
	(1) Zip code	(2) County	(3) DMA	(4) State
Panel A: Log expenditures				
Radio coverage	-0.0131***	-0.0131***	-0.0131***	-0.0131***
	(0.0027)	(0.0032)	(0.0036)	(0.0028)
N	3,744,066	3,744,066	3,744,066	3,744,066
R^2	0.518	0.518	0.518	0.518
Mean of dep. var.	6.185	6.185	6.185	6.185
Household FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
Panel B: Log items				
Radio coverage	-0.0168***	-0.0168***	-0.0168***	-0.0168***
	(0.0029)	(0.0035)	(0.0041)	(0.0038)
N	3,734,881	3,734,881	3,734,881	3,734,881
R^2	0.541	0.541	0.541	0.541
Mean of dep. var.	4.189	4.189	4.189	4.189
Household FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes

Notes: This table shows OLS regression estimates where the unit of observation is a household-month. The dependent variable in Panel A are log expenditures. The dependent variable in Panel B are log purchased items. Each column uses robust standard errors clustered at the geographic or administrative unit indicated by the column header.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

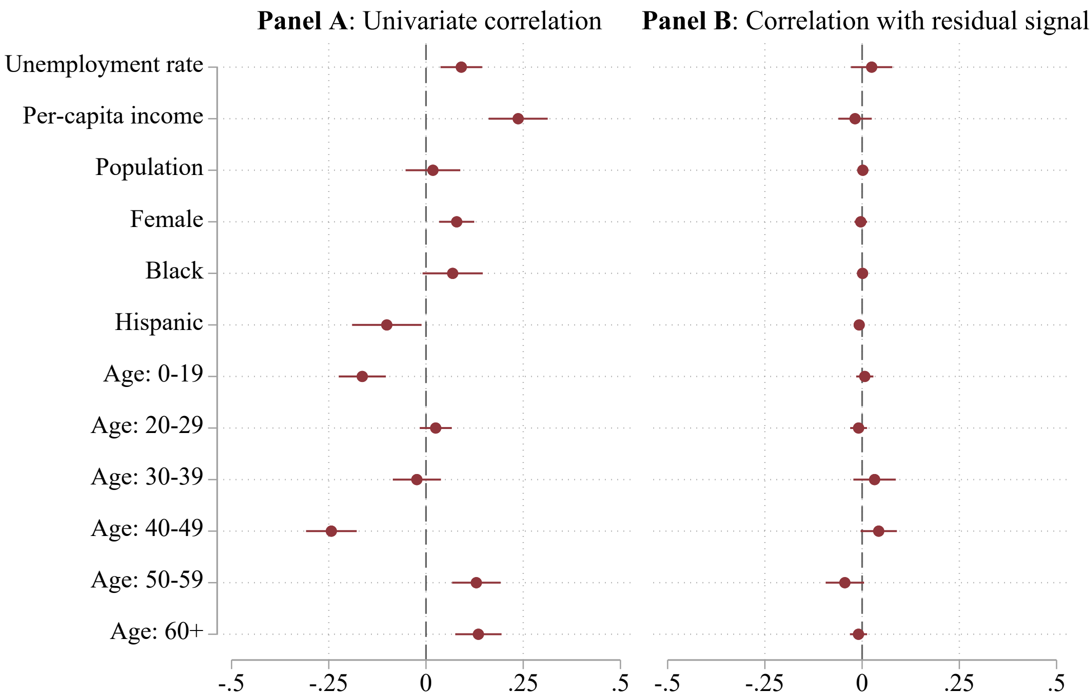
Table C.10: Robustness – Using Nielsen projection factors to re-weigh households

	(1)	(2)	(3)	(4)
Panel A: Log expenditures				
Radio coverage	-0.0110***	-0.0099**	-0.0156***	-0.0155***
	(0.0040)	(0.0039)	(0.0041)	(0.0051)
N	3,683,294	3,683,294	3,353,738	3,353,738
R^2	0.530	0.533	0.535	0.538
Mean of dep. var.	6.145	6.145	6.148	6.148
Household & Time FEs	Yes	Yes	Yes	Yes
Household controls		Yes	Yes	Yes
Local economic conditions			Yes	Yes
State x Time FEs				Yes
Panel B: Log items				
Radio coverage	-0.0169***	-0.0152***	-0.0232***	-0.0250***
	(0.0043)	(0.0042)	(0.0045)	(0.0054)
N	3,674,329	3,674,329	3,345,823	3,345,823
R^2	0.555	0.558	0.559	0.562
Mean of dep. var.	4.158	4.158	4.156	4.156
Household & Time FEs	Yes	Yes	Yes	Yes
Household controls		Yes	Yes	Yes

Notes: This table shows WLS regression estimates of equation 1. Households are weighted using the weights supplied by Nielsen. Households with weights above 10,000 are excluded. The dependent variable in Panel A are log expenditures. The dependent variable in Panel B are log purchased items. Robust standard errors clustered at the zip code level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.8: Residual signal strength and time-varying characteristics



Notes: This figure plots OLS regression coefficients on a county-year panel using different time-varying county-level characteristics as dependent variable. Dependent variables are standardized to have mean zero and standard deviation one to facilitate comparisons. Each point estimate is obtained from a separate regression. Panel A reports the regression coefficient between the time-varying county characteristics and the standardized, predicted radio signal strength. Panel B reports analogous estimates conditional on the predicted free-space signal, its square, county and year fixed effects, and region×year fixed effects. The county-year panel is derived from the baseline sample by collapsing variables to the county-year level. Robust standard errors clustered at the DMA level are used to construct 95% confidence intervals.

Table C.11: Robustness: Heterogeneity analysis by financial struggles without controls

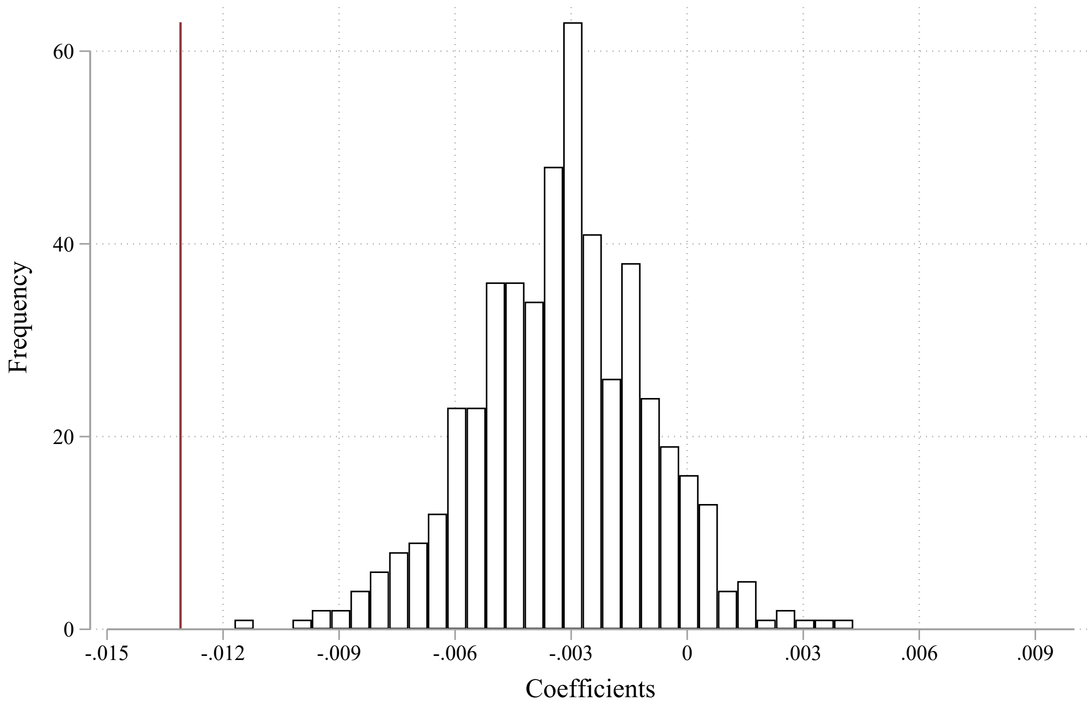
	Dependent variable: log (Expenditures)					
	Expenditures		Income		Expenditures to income	
	(1) High	(2) Low	(3) High	(4) Low	(5) High	(6) Low
Radio show	-0.020***	-0.005	-0.013***	-0.014***	-0.020***	-0.007*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
N	1,982,051	1,762,015	2,032,501	1,711,565	1,864,927	1,879,139
R^2	0.453	0.445	0.518	0.513	0.518	0.476
Mean of dep. var.	6.449	5.889	6.233	6.129	6.353	6.019

Notes: This table uses 2004–2019 Nielsen Homescan data at the household-by-month level. The dependent variable is the log of household expenditures. "Radio show" is a binary indicator taking value one after local market entry of the *Dave Ramsey Show*. Robust standard errors clustered by zip code are shown in parentheses. Each column provides estimates from a subset of households obtained by a median split based on the household covariate indicated in the column's header. For the median split in columns 1–2, I use the average, inflation-adjusted and equivalized expenditures in the first year in the panel. For the median split in columns 3–4, I use the average, inflation-adjusted and equivalized household income in the first year in the panel. For the median split in columns 5–6, I use the average household expenditures normalized by income in the first year in the panel.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Additional analyses

Figure D.1: Counterfactual radio coverage



Notes: This figure plots a histogram of coefficients from a regression of log household expenditures on a counterfactual radio coverage indicator, including household and year-month fixed effects (see equation (1)). The distribution of coefficients is obtained from 500 counterfactual assignments. The coefficient from the actual radio coverage is shown as a red vertical line. Each counterfactual estimate is obtained as follows. I repeatedly assign a randomly chosen counterfactual market entry date to each zip code. If a zip codes is outside the actual coverage area of all affiliated radio stations, the zip code is always assigned to the control group without any market entry. I thus only vary the timing but not the set of zip codes that eventually receive access to the radio show. Based on the counterfactual timing of market entry, I apply equivalent sample restrictions as described in Section 3.2, and re-estimate equation 1 without time-varying controls using household expenditures as the dependent variable.

Table D.1: Presidential elections: Turnout and voting behavior

	Turnout in Presidential election			Republican vote share		
	(1)	(2)	(3)	(4)	(5)	(6)
Radio coverage	-0.009	-0.005	0.003	0.008**	0.002	0.004
	(0.007)	(0.005)	(0.003)	(0.004)	(0.003)	(0.002)
N	15415	15415	15410	15415	15415	15410
R^2	0.937	0.963	0.977	0.943	0.954	0.977
Mean of dep. var.	0.470	0.470	0.471	0.563	0.563	0.563
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Baseline covar. x Year FEs		Yes	Yes		Yes	Yes
State x Year FEs			Yes			Yes

Notes: This table shows OLS regression estimates using electoral outcomes from the Presidential elections in 2000–2016. The unit of observation is a county-election. Turnout is measured as the ratio of cast votes to the voting age population. Radio coverage is the share of the county population with radio coverage. Observations are weighted by the voting age population Baseline county characteristics in 2000 include the percent of females, blacks, Hispanics and age group shares in 10 year bins. Robust standard errors clustered at the state level are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E Content analysis

E.1 Qualitative evidence

This section contains a collection of quotes from Dave Ramsey that shed light on his views on consumption, debt and the role of social and cultural expectations.

Social and cultural expectations

- "We buy things we don't need with money we don't have to impress people we don't like."
- "We lived our lives according to the standards set to 'keep up with the Joneses.' Turns out they were broke and living in debt, too." (The Total Money Makeover, p. 20)
- "It is human nature to want it and want it now; it is also a sign of immaturity. Being willing to delay pleasure for a greater result is a sign of maturity. However, our culture teaches us to live for the now. 'I want it' we scream, and we can get it if we are willing to go into debt. Debt is a means to obtain the 'I want its' before we can afford them." (The Total Money Makeover, p. 16)
- "We live in a culture that quit asking, 'How much?' and instead asks, 'How much down, and how much a month?'" (The Total Money Makeover, p. 33)
- "Peer pressure, cultural expectations, 'reasonable standard of living' – I don't care how you say it, we all need to be accepted by our crowd and our families. This need for approval and respect drives us to do some really insane things. One of the paradoxically dumb things we do is to destroy our finances by buying garbage we can't afford to try to make ourselves appear wealthy to others." (The Total Money Makeover, p. 78)
- "Peer pressure is very, very powerful. 'We are scaling down' is a painful statement to make to friends or family. 'We will have to pass on that trip or dinner because it is not in our budget' is virtually impossible for some people to say. Being real takes tremendous courage. We like approval, and we like respect, and to say otherwise is another form of denial. The wish for the admiration of others is normal. The problem is that this admiration can become a drug. Many of you are addicted to this drug, and the destruction to your wealth and financial well-being caused by your addiction is huge." (The Total Money Makeover, p. 80)

- “Financial peace isn’t the acquisition of stuff. It’s learning to live on less than you make, so you can give money back and have money to invest. You can’t win until you do this.”
- “You must walk to the beat of a different drummer. The same beat that the wealthy hear. If the beat sounds normal, evacuate the dance floor immediately! The goal is to not be normal, because as my radio listeners know, normal is broke.”
- “70% of Americans live paycheck to paycheck. Seven out of ten people you walk past going down the sidewalk are broke. You can model your life after them, and you will be one of them. Or you can mode your life after the weird people. Because wealth is unusual. It’s not normal. So you have to engage in unusual behaviors and habits to create unusual results.”

Debt

- “Debt has been sold to us so aggressively, so loudly, and so often that to imagine living without debt requires myth-busting.” (The Total Money Makeover, p. 17)
- “Debt is so ingrained into our culture that most Americans cannot even envision a car without a payment, a house without a mortgage, a student without a loan, and credit without a card.” (The Total Money Makeover, p. 17-18)
- “Debt is not a tool; it is a method to make banks wealthy, not you. The borrower truly is slave to the lender.” (The Total Money Makeover, p. 48)
- “My contention is that debt brings on enough risk to offset any advantages that could be gained through leverage of debt.” (The Total Money Makeover, p. 20)
- “Larry Burkett said debt is not the problem; it is the symptom. I feel debt is the symptom of overspending and undersaving.” (The Total Money Makeover, p. 45)

Behavior

- “Winning at money is 80 percent behavior and 20 percent head knowledge. What to do isn’t the problem; doing it is. Most of us know what to do, but we just don’t do it. If I can control the guy in the mirror, I can be skinny and rich.” (The Total Money Makeover, p. 3)
- “I teach concepts, not mathematical formulas.” (The Total Money Makeover, p. xvi)
- “Break through the temptation to remain in the same situation, and opt for the pain of change before the pain of not changing searches you out.” (The Total Money Makeover, p. 14)

- “Living on less than you make is matter of controlling yourself, not a matter of math.”

- “I can always tell which ones are serious and which aren’t. There’s something in their voices that communicates passion and conviction when they’re really excited about getting out of debt. But if they’re just playing around with the idea, if they’re simply curious about it, then their voices are flat. If I don’t hear any passion behind what they’re saying, I know they aren’t ready to cut up the credit cards and dump their debt for good. That’s because getting out of debt isn’t about solving a math problem; it’s about changing your life–and that requires a change of heart.”

- “One thing I am sure of in my Total Money Makeover: I had to quit telling myself that I had innate discipline and fabulous natural self-control. That is a lie. I have to put systems and programs in place that make me do smart things. Saying, ‘Cross my fingers and hope to die, I promise, promise, promise I will pay extra on my mortgage because I am the one human on the planet who has that kind of discipline,’ is kidding yourself. A big part of being strong financially is that you know where you are weak and take action to make sure you don’t fall prey to the weakness.”

E.2 Quantitative evidence

E.2.1 Data and text processing

I use a Python-based command line program to collect the automatically generated subtitles of the 5,587 YouTube videos uploaded by the *Dave Ramsey Show* between August 13, 2013, and May 31, 2021. These subtitles are available in `WebVTT` format, which includes both the audio transcripts as well as timestamps indicating the start time for each line of text. I remove timestamps and aggregate subtitles to documents containing 5 minutes of contiguous speech.

I apply a series of commonly used processing steps to prepare the raw text data for analysis. I convert the text to lowercase and remove whitespace. Next, I remove English language stopwords that occur very frequently. In addition, I remove numerals (e.g. "five", "thousand") as those occur frequently when Dave Ramsey asks callers for information about their finances. Moreover, I remove a list context specific words mentioned in the radio show's jingle and during commercial breaks: `headquarter`, `bmw`, `king`, `blinds.com`, `promo`, `code`, `sample`, `churchill`, `zander`, `mama`, `ship`, `shipping`, `blinds`, `window`, `special`, `smartvestor`. I also remove names of personalities appearing on the radio show such as `dave`, `ramsey`, `chris` and `logan`. I then apply the Porter stemmer, one of the most common English language stemming algorithms. I remove all non-alphanumeric characters, exclude words that occur less than 100 times, and all words that only include numbers.

E.2.2 Latent Dirichlet Allocation

For topic analysis, I use Latent Dirichlet Allocation (LDA) which is an unsupervised machine learning technique for topic modeling (Gentzkow et al., 2019). As an input, I use the document-term matrix of all unigrams that appear in at most 90% of all documents. Here, a document corresponds to the words spoken in a 5-minute interval. I train the LDA model using an online learning method with hyperparameters $\kappa = 0.7$, $\tau_0 = 10$ and a batch size of 512.

Figure E.1 shows the 50 words with the highest probability by topic. Differences in the size corresponds to differences in probabilities. To assign labels to topics, I rely both on the word cloud and manual inspection of text segments where the model has a high confidence in its classification.

E.2.3 Keywords and word co-occurrences

To complement the topic model approach, I explore common words and their associations across documents. Table E.1 provides an overview of the most frequent words and keywords across the 5,587 YouTube videos uploaded by the *Dave Ramsey Show*. Figure E.2 illustrates the network of words with the highest co-occurrence rates with the word “debt”, using a methodology proposed by Bail (2016) and excluding the same set of stop words as in the LDA analysis. This complementary approach confirms that paying off debt is a central theme of the radio show.

Figure E.1: Word distribution by topic

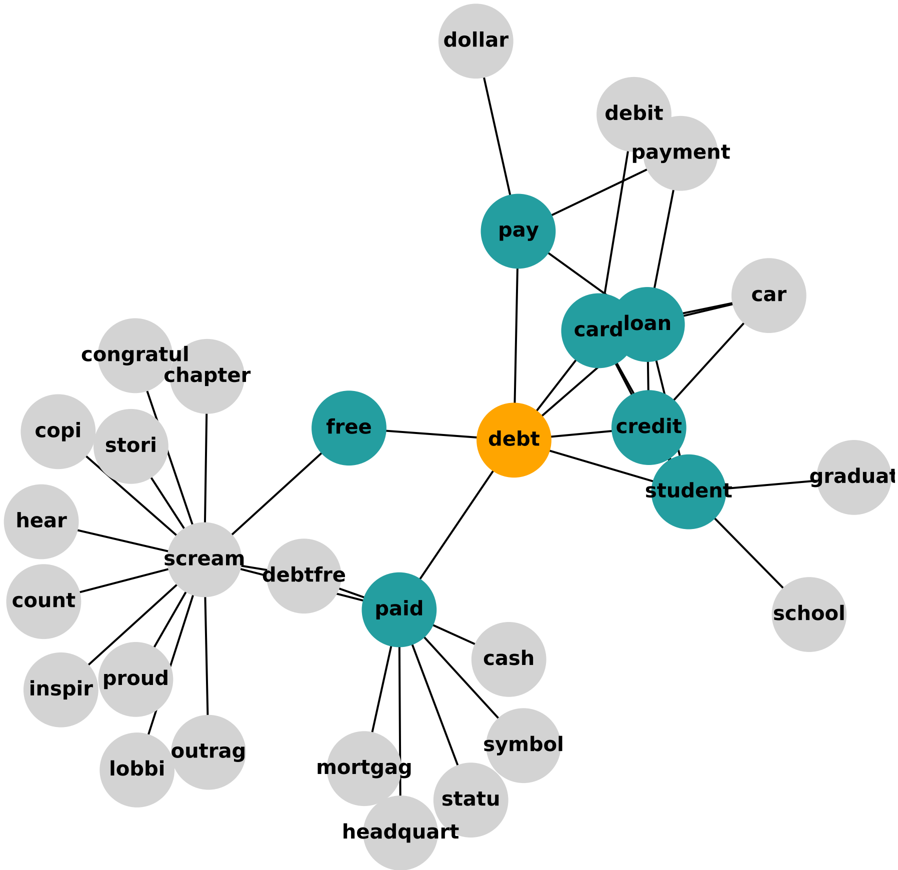
Notes: This figure illustrates the results of the Latent Dirichlet Allocation. Each word cloud shows the top 50 words by topic based on their frequency of occurrence. The font size is proportional to the word frequency.

Table E.1: Most frequent spoken words and keywords for YouTube videos of the *Dave Ramsey Show*

Rank	Word	Frequency	Video keyword	Frequency
1	money	105286	money	2553
2	debt	78079	credit card	2546
3	pay	64709	real estate	2544
4	start	60781	buy	2518
5	dollar	56420	insurance	2510
6	hous	51554	save	2509
7	car	47284	how to make money	2504
8	life	47121	snowball	2501
9	live	46014	buying house	2497
10	month	45808	compound interest	2495
11	save	38015	budget money debt cash	2493
12	busi	34036	debt	2213
13	home	33057	debt free scream	868
14	loan	31505	personal finance	697
15	incom	31105	budget	559
16	paid	30377	student loans	369
17	job	30140	finance	355
18	plan	29521	drtlgi	336
19	step	29282	family	331
20	question	28915	credit	326
21	buy	28783	marriage	316
22	free	28470	investing	301
23	love	27938	debt free	293
24	stuff	26490	paying off debt	266
25	kid	26236	free	247
26	financi	25822	loans	244
27	fund	25414	student loan debt	236
28	famili	25327	loan	235
29	care	23960	car	227
30	babi	23864	scream	214
31	budget	23412	pay off debt	213

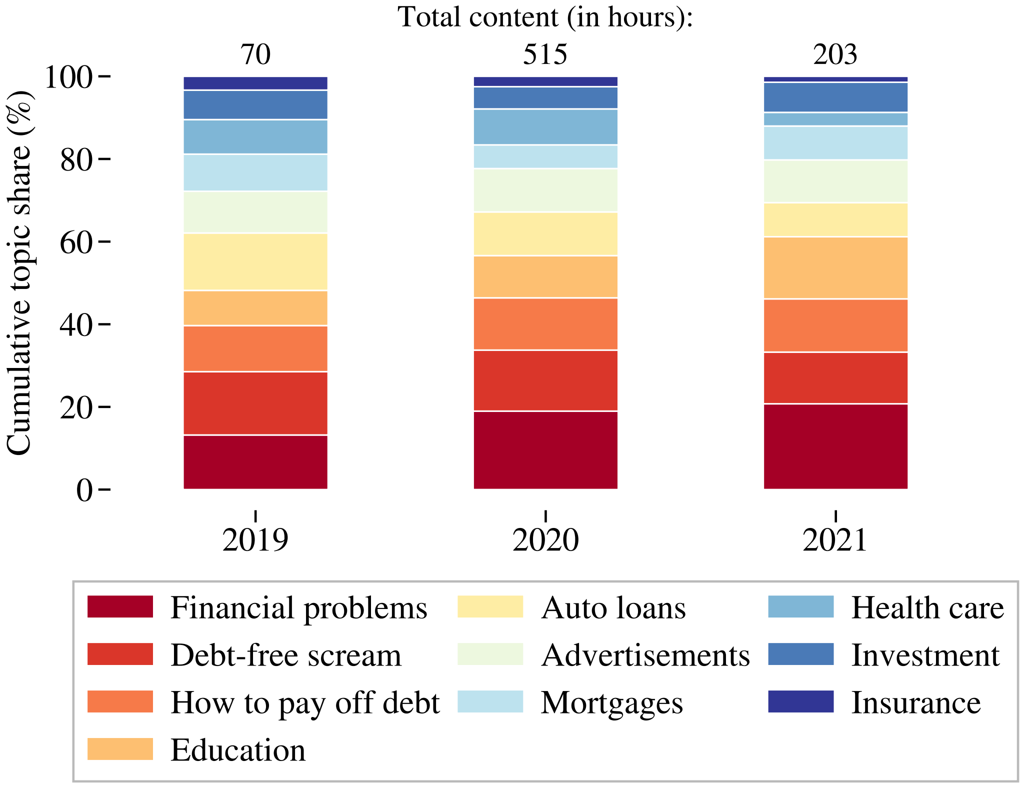
Notes: This table shows 30 most frequent spoken words as well as the most commonly used keywords attached to YouTube videos uploaded by the channels "The Ramsey Show – Full Episodes" and "The Ramsey Show – Highlights" between August 13, 2013, and May 31, 2021. The list excludes all keywords that include "dave", "ramsey", "video", or "show". The most frequent spoken words exclude a list of commonly used English words.

Figure E.2: Correlates of the word “debt”



Notes: This figure uses data from YouTube. It shows words that frequently occur with the word “debt” in a 5 minute segment of audio. Edges between words indicate that when constructing binary indicators for the presence of these words in a document, these indicators have a correlation of 0.20 or above. To generate this list, I start with the word “debt” and collect all words with a correlation of at least 0.20. For these “direct links”, I obtain all words that have a correlation of at least 0.30. I then plot the connections among these words.

Figure E.3: Topic distribution: Restricting to full episodes



Notes: This figure displays the distribution of topics featured in the *Dave Ramsey Show* in the videos uploaded on its YouTube channel. This figure restricts to videos covering full episodes of the show. Topic shares are obtained from *Latent Dirichlet Allocation* by calculating the average probability of each topic across documents. For each year, the total content (in hours) uploaded on YouTube is indicated above each bar.

F Experiment

This section contains additional material and information about the survey experiment discussed in Section 6.

F.1 Research transparency

Preregistration The main experiment was preregistered on the AEA RCT Registry as project #AEARCTR-0008050. The preregistration includes details on the experimental design, the sampling process, planned sample size, exclusion criteria, hypotheses and the main analyses. Below, I document deviations from the preregistration:

- The preregistration uses a different title and different treatment labels.
- The preregistration did not include quotas based on sociodemographic characteristics. In practice, the sampling process was stratified based on age, gender and education, which results in a more representative sample of the US population.
- Respondents below the age of 18 and those who do not reside in the US were not eligible to participate in the survey, which was not preregistered.
- When construction attitudinal indices, I normalize the indices using the mean and standard deviation in the control group used in the analysis. The preregistration did not specify the reference group for the normalization. However, the normalization does not affect the economical or statistical significance of the results.
- In contrast to the preregistration, I include control variables when estimating treatment effects in the main experiment. The results are robust to not including controls, as shown in Table F.5.
- Non-preregistered analyses include (i) a robustness exercises estimating treatment effects on individual items used to measure attitudes and (ii) the descriptive evidence on the correlation between attitudes and behavior.

The one-week follow-up survey was not preregistered.

Ethical approval The experimental study received ethics approval from the German Association for Experimental Economic Research (#T7wapLjB, 07/20/2021).

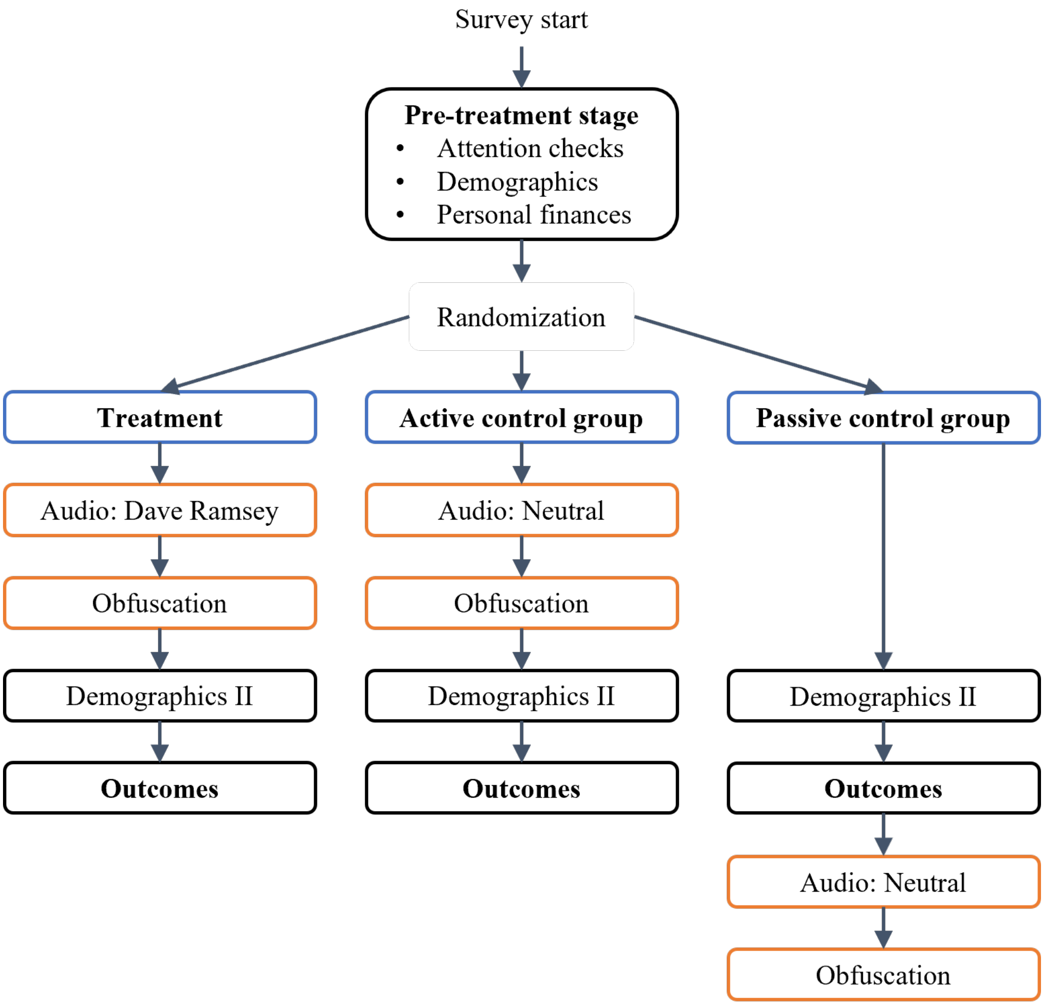
Data and code availability The experimental data and the analysis code will be made available online.

Competing interests I declare no competing interests.

F.2 Figures and tables

Figure F.1: Design overview and timing

Panel A: Main experiment – Design overview



Panel B: Timing of the main experiment and the follow-up survey

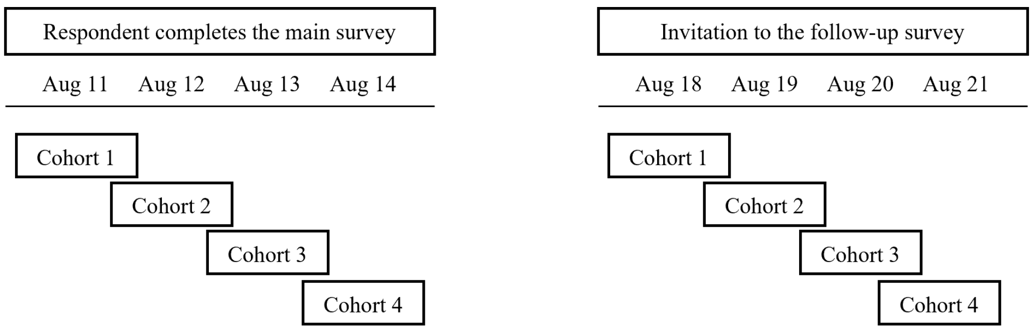


Table F.1: Comparison of the survey sample to the general US population

Variable	Survey sample	American Community Survey (2019)
Female	50%	51%
Age: 18–34	30%	30%
Age: 35–54	30%	32%
Age: 55+	40%	38%
Education: Bachelor's degree or above	30%	31%
Region: Northeast	19%	17%
Region: Midwest	21%	21%
Region: South	43%	38%
Region: West	17%	24%

Notes: This table provides summary statistics for the sample in the main experiment (column 1) and the general US population (column 2) for basic demographic characteristics.

Table F.2: Test of balance: Main experiment

	Means (std. dev.)			Differences (p-values)		
	Treatment group (T)	Active control (A)	Passive control (P)	T - A	T - P	A - P
Age	47.825	48.015	48.071	-0.190	-0.245	0.056
	(17.763)	(17.504)	(18.351)	(0.868)	(0.828)	(0.961)
Female	0.494	0.504	0.491	-0.010	0.003	-0.014
	(0.500)	(0.501)	(0.500)	(0.749)	(0.918)	(0.668)
College degree	0.445	0.447	0.446	-0.002	-0.001	-0.001
	(0.497)	(0.498)	(0.498)	(0.958)	(0.975)	(0.982)
Log income	10.628	10.558	10.646	0.070	-0.018	0.088
	(0.891)	(0.930)	(0.889)	(0.232)	(0.750)	(0.126)
Log debt	6.302	6.170	6.186	0.133	0.117	0.016
	(4.538)	(4.461)	(4.539)	(0.647)	(0.680)	(0.954)
Democrat	0.437	0.417	0.429	0.020	0.008	0.012
	(0.497)	(0.494)	(0.495)	(0.532)	(0.805)	(0.693)
Republican	0.297	0.285	0.283	0.012	0.014	-0.003
	(0.457)	(0.452)	(0.451)	(0.691)	(0.616)	(0.928)
Subjective financial literacy	4.699	4.523	4.619	0.176*	0.080	0.096
	(1.405)	(1.452)	(1.295)	(0.057)	(0.341)	(0.270)
Savings ability	0.638	0.587	0.608	0.051	0.030	0.021
	(0.481)	(0.493)	(0.489)	(0.105)	(0.315)	(0.507)
Region: Northeast	0.222	0.160	0.178	0.062**	0.043*	0.019
	(0.416)	(0.367)	(0.383)	(0.015)	(0.084)	(0.426)
Region: Midwest	0.205	0.191	0.242	0.014	-0.036	0.050*
	(0.404)	(0.394)	(0.428)	(0.592)	(0.163)	(0.055)
Region: South	0.396	0.472	0.416	-0.076**	-0.020	-0.056*
	(0.490)	(0.500)	(0.493)	(0.017)	(0.514)	(0.074)
Region: West	0.177	0.177	0.164	0.000	0.013	-0.013
	(0.382)	(0.382)	(0.370)	(0.992)	(0.572)	(0.583)
p-value of joint F-test				0.313	0.796	0.689
Observations	492	470	538	962	1,030	1,008

Notes: This table shows a test of balance for the main experiment. Columns 1–3 show the means and standard deviations of respondent covariates in the different treatments arms. Columns 4–6 show differences in means between the groups indicated in the column header together with p-values in parentheses. The p-values of the joint F-test are determined by regressing the treatment indicator on the vector of covariates. The F-test tests the joint hypothesis that none of the covariates predicts treatment assignment.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.3: Balance of post-treatment demographics

	Means (std. dev.)			Differences (p-values)		
	Treatment group (T)	Active control (A)	Passive control (P)	T - A	T - P	A - P
Black	0.126	0.126	0.113	0.000	0.013	-0.012
	(0.332)	(0.332)	(0.317)	(0.982)	(0.533)	(0.553)
White	0.799	0.777	0.805	0.022	-0.006	0.028
	(0.401)	(0.417)	(0.397)	(0.401)	(0.808)	(0.271)
Hispanic	0.083	0.074	0.072	0.009	0.011	-0.002
	(0.277)	(0.263)	(0.260)	(0.611)	(0.517)	(0.905)
Full-time employment	0.325	0.338	0.325	-0.013	-0.000	-0.013
	(0.469)	(0.474)	(0.469)	(0.667)	(0.998)	(0.662)
Unemployed	0.108	0.111	0.126	-0.003	-0.019	0.016
	(0.310)	(0.314)	(0.333)	(0.885)	(0.353)	(0.441)
Not in labor force	0.376	0.360	0.348	0.016	0.028	-0.012
	(0.485)	(0.480)	(0.477)	(0.598)	(0.343)	(0.691)
p-value of joint F-test				0.837	0.816	0.865
Observations	492	470	538	962	1,030	1,008

Notes: This table shows a balance test for the main experiment using post-treatment demographic variables. Columns 1–3 show the means and standard deviations of respondent covariates in the different treatments arms. Columns 4–6 show differences in means between the groups indicated in the column header together with p-values in parentheses. The p-values of the joint F-test are determined by regressing the treatment indicator on the vector of covariates. The F-test tests the joint hypothesis that none of the covariates predicts treatment assignment.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.4: Correlation between attitudes and past behavior

	Log debt		Debt-free		Log spending	
	(1)	(2)	(3)	(4)	(5)	(6)
Debt attitudes	0.391**	0.398***	-0.045***	-0.047***	-0.049	-0.037
	(0.152)	(0.151)	(0.015)	(0.015)	(0.049)	(0.046)
Consumption attitude	0.171	0.213	-0.015	-0.015	0.169***	0.107***
	(0.145)	(0.152)	(0.015)	(0.016)	(0.042)	(0.039)
N	1,008	1,008	1,008	1,008	1,008	1,008
Mean of dep. var.	6.178	6.178	0.301	0.301	4.805	4.805
Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows OLS regression estimates using respondents from the main study, excluding respondents in the treatment group. The debt attitude index and the consumption attitude index are constructed as described in the main text and oriented such that larger values correspond to more positive attitudes towards the object. Both indices are normalized to have mean zero and standard deviation one. Control variables include numerical age and age squared, log income, female indicator, and an indicator for having completed a Bachelor's degree or higher.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.5: Robustness: Treatment effects on attitudes across studies without controls

	Main study		Robustness: Passive control		One-week follow-up	
	(1) Debt attitudes	(2) Consumption attitudes	(3) Debt attitudes	(4) Consumption attitudes	(5) Debt attitudes	(6) Consumption attitudes
Treatment	-0.535***	-0.219***	-0.605***	-0.227***	-0.313***	-0.187*
	(0.065)	(0.065)	(0.061)	(0.065)	(0.096)	(0.099)
N	962	962	1,030	1,030	522	522
z-scored	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No

Notes: This table shows OLS regression estimates where the dependent variables are attitudes towards consumption and debt. The debt attitude index and the consumption attitude index are constructed as described in the main text and oriented such that larger values correspond to more positive attitudes towards the object. Both indices are normalized to have mean zero and standard deviation one. "Treatment" is a binary indicator taking value one for respondents who listened to a five-minute recording from the *Dave Ramsey Show*. Columns 1 and 2 use respondents from the main study assigned to the treatment group or the control group. Columns 3 and 4 use respondents from the main study assigned to the treatment group or the robustness control group. Columns 5 and 6 use respondents from the one-week follow-up survey pooling respondents from both control group conditions as a joint control group. Robust standard errors are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.6: Main experiment – Treatment effects on attitudes by item

	Debt attitudes				Consumption attitudes	
	(1)	(2)	(3)	(4)	(5)	(6)
	There is no excuse for borrowing money	You should always save up first before buying something	You can live a good life without borrowing money	All in all, borrowing money is not worth the cost	I admire people who own expensive homes, cars, and clothes	The things I own say a lot about how well I'm doing in life
Panel A: Active control						
Treatment	0.318***	0.270***	0.363***	0.507***	-0.134**	-0.257***
	(0.066)	(0.062)	(0.061)	(0.064)	(0.064)	(0.066)
N	962	962	962	962	962	962
z-scored	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Passive control						
Treatment	0.452***	0.292***	0.352***	0.590***	-0.221***	-0.176***
	(0.065)	(0.059)	(0.057)	(0.061)	(0.063)	(0.064)
N	1,030	1,030	1,030	1,030	1,030	1,030
z-scored	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows OLS regression estimates using respondents from the main experiment. The dependent variables are respondents' agreement with the statements indicated in the column header and measured on a 5-point Likert scale from "Strongly agree" to "Strongly disagree". Responses are coded such that larger values indicate stronger agreement, and z-scored using the mean and standard deviation in the respective control group. "Treatment" is a binary indicator taking value one for respondents who listened to a five-minute recording from the *Dave Ramsey Show*. Panel A uses respondents from the treatment group and the control group. Panel B uses respondents from the treatment group and the robustness control group. Robust standard errors are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.7: Follow-up survey – Treatment effects on attitudes by item

	Debt attitudes				Consumption attitudes	
	(1)	(2)	(3)	(4)	(5)	(6)
	There is no excuse for borrowing money	You should always save up first before buying something	You can live a good life without borrowing money	All in all, borrowing money is not worth the cost	I admire people who own expensive homes, cars, and clothes	The things I own say a lot about how well I'm doing in life
Panel A: Baseline						
Treatment	0.277***	0.035	0.339***	0.260***	-0.123	-0.211**
	(0.093)	(0.101)	(0.093)	(0.096)	(0.094)	(0.100)
N	522	522	522	522	522	522
z-scored	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Controls						
Treatment	0.260***	0.030	0.323***	0.267***	-0.142*	-0.228**
	(0.091)	(0.100)	(0.093)	(0.096)	(0.086)	(0.095)
N	522	522	522	522	522	522
z-scored	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: IPAW						
Treatment	0.275***	0.049	0.361***	0.266***	-0.124	-0.215**
	(0.093)	(0.103)	(0.094)	(0.097)	(0.094)	(0.100)
N	522	522	522	522	522	522
z-scored	Yes	Yes	Yes	Yes	Yes	Yes
Panel D: Controls & IPAW						
Treatment	0.259***	0.041	0.344***	0.274***	-0.145*	-0.232**
	(0.091)	(0.102)	(0.094)	(0.097)	(0.086)	(0.095)
N	522	522	522	522	522	522
z-scored	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows regression estimates using respondents from the one-week follow-up survey. The dependent variable are respondents' agreement with the statements indicated in the column header and measured on a 5-point Likert scale from "Strongly agree" to "Strongly disagree". Responses are coded such that larger values indicate stronger agreement, and z-scored using the mean and standard deviation of non-treated respondents. "Treatment" is a binary indicator taking value one for respondents who listened to a five-minute recording from the *Dave Ramsey Show*. Panel A presents baseline OLS estimates without controls . Panel B includes numerical age and age squared, log income, female indicator, an indicator for having completed a Bachelor's degree or higher, and region indicators as controls. Panel C uses inverse probability of attrition weights (IPAW) obtained from a logistic regression of the attrition status dummy on the vector of baseline covariates from Table F.2 to reweigh respondents. Panel D adds the control variables from Panel B to the specification from Panel C. Robust standard errors are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.8: Follow-up survey – Test for differential attrition across treatment arms

	(1) Age	(2) Female	(3) College	(4) Log income	(5) Log debt	(6) Democrat	(7) Republican	(8) Financial literacy	(9) Savings ability	(10) Northeast	(11) Midwest	(12) South	(13) West
Treatment	-0.40	0.01	-0.03	0.01	0.45	0.03	0.01	0.04	0.03	0.05*	0.00	-0.05	-0.00
	(1.21)	(0.03)	(0.03)	(0.06)	(0.30)	(0.03)	(0.03)	(0.10)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Follow-up	4.04***	0.01	-0.06*	-0.10*	0.63**	0.03	0.03	0.04	-0.05	-0.01	0.01	0.03	-0.04
	(1.18)	(0.03)	(0.03)	(0.06)	(0.30)	(0.03)	(0.03)	(0.09)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)
Treatment x Follow-up	0.54	-0.02	0.09	0.05	-0.94*	-0.04	-0.00	0.24	0.03	-0.00	-0.05	0.02	0.03
	(2.04)	(0.06)	(0.06)	(0.10)	(0.53)	(0.06)	(0.05)	(0.16)	(0.06)	(0.05)	(0.05)	(0.06)	(0.04)
Constant	46.64***	0.49***	0.47***	10.64***	5.96***	0.41***	0.27***	4.56***	0.62***	0.17***	0.21***	0.43***	0.18***
	(0.70)	(0.02)	(0.02)	(0.04)	(0.18)	(0.02)	(0.02)	(0.05)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
N	1,500	1,500	1,500	1,500	1,500	1,500	1,500	1,500	1,500	1,500	1,500	1,500	1,500

Notes: This table shows OLS regression estimates using baseline demographic characteristics as dependent variable. Each regression includes the full interaction between the binary treatment indicator and a binary dummy indicating whether a respondent is part of the follow-up sample. Robust standard errors are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F.9: Test of balance: Follow-up survey

	Means (std. dev.)		Difference (p-values)
	Treatment group (T)	Control group (C)	T - C
Age	50.813	50.678	0.135
	(17.455)	(17.723)	(0.935)
Female	0.485	0.504	-0.019
	(0.501)	(0.501)	(0.686)
College degree	0.462	0.407	0.055
	(0.500)	(0.492)	(0.237)
Log income	10.595	10.538	0.057
	(0.822)	(0.888)	(0.483)
Log debt	6.097	6.586	-0.489
	(4.703)	(4.436)	(0.247)
Democrat	0.427	0.442	-0.015
	(0.496)	(0.497)	(0.751)
Republican	0.316	0.305	0.011
	(0.466)	(0.461)	(0.800)
Subjective financial literacy	4.877	4.598	0.279**
	(1.261)	(1.355)	(0.024)
Savings ability	0.626	0.564	0.062
	(0.485)	(0.497)	(0.181)
Region: Northeast	0.216	0.165	0.051
	(0.413)	(0.372)	(0.156)
Region: Midwest	0.181	0.225	-0.044
	(0.386)	(0.418)	(0.250)
Region: South	0.433	0.464	-0.032
	(0.497)	(0.499)	(0.497)
Region: West	0.170	0.145	0.024
	(0.376)	(0.353)	(0.471)
p-value of joint F-test			0.246
Observations	171	351	522

Notes: This table shows a test of balance for the sample in the follow-up survey. Columns 1–2 show the means and standard deviations of respondent covariates in the treatment group and the pooled control group comprising respondents in the control group and the robustness control group. Columns 3 show differences in means between the treatment group and the control group together with p-values in parentheses. The p-value of the joint F-test is determined by regressing the treatment indicator on the vector of covariates. The F-test tests the joint hypothesis that none of the covariates predicts treatment assignment.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

F.3 Secondary outcomes

To obfuscate the purpose of the main study, the survey includes an obfuscation module with several non-attitudinal measures. While these measures are not the primary interest of the experiment, this section provides a discussion of the treatment effects on these secondary outcomes. Table F.10 presents estimates of the treatment effect of listening to the *Dave Ramsey Show* for five minutes on these outcomes using the audio control group (Panel A) or the robustness control group (Panel B) as comparison group.

First, column 1 shows that there is no statistically significant and robust treatment effect on respondents' demand for information about personal finances. Consistent with the hypothesis in the preregistration, the point estimate for the effect is larger when using the audio control group as comparison group, although the difference is not statistically significant at conventional levels. Second, consistent with my preregistered hypothesis, there is no statistically significant and robust treatment effect on general financial literacy as measured by the Big 5 survey module (column 2). Indeed, the audio recording from the *Dave Ramsey Show* does not include any information that would be help respondents answer the factual questions in the Big 5 module. Third, while I do not find treatment effects on respondents' beliefs about the average debt of US households (column 3), column 4 shows that treated respondents think that a larger share of Americans has any kind of debt ($p < 0.05$). The effect size is modest and depends on the comparison group and varies from 2.8 to 5.5 percentage points relative to a baseline of about 60-63%.

This provides further suggestive evidence that the *Dave Ramsey Show* affects the behavior of its listeners primarily by changing attitudes towards consumption and debt using its consistent and persuasive narrative.

Table F.10: Treatment effects on secondary outcomes

	(1) Information demand	(2) Financial literacy	(3) Belief: Average debt	(4) Belief: Any debt
Panel A: Audio control group				
Treatment	0.052*	0.159*	3.231	5.457***
	(0.029)	(0.082)	(3.463)	(1.404)
Constant	0.253***	3.034***	75.376***	60.223***
	(0.020)	(0.058)	(2.337)	(1.067)
N	962	962	962	962
Panel B: Robustness control group				
Treatment	-0.004	-0.086	6.047*	2.841**
	(0.029)	(0.080)	(3.266)	(1.267)
Constant	0.309***	3.279***	72.560***	62.840***
	(0.020)	(0.055)	(2.034)	(0.880)
N	1,030	1,030	1,030	1,030

Notes: This table shows OLS regression estimates using respondents from the main experiment. "Information demand" takes value one for respondents who said that they would like to receive information about personal finances, and zero otherwise. "Financial literacy" is the number of correctly answered questions (out of 5) from the Big 5 financial literacy questionnaire. "Belief: Average debt" is the respondent's belief about the average debt of US Americans in thousand US dollars. "Belief: Any debt" is the belief about the share of Americans that have any debt at all. "Treatment" is a binary indicator taking value one for respondents who listened to the five-minute recording from the *Dave Ramsey Show*. Regressions do not include any control variables Panel A uses respondents from the treatment group and the control group (that listened to a neutral audio). Panel B uses respondents from the treatment group and the robustness control group (that did not listen to an audio recording). Robust standard errors are shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

F.4 Experimental instructions: Main study

F.4.1 Pre-treatment stage

Welcome!

Thank you for your interest in completing this survey. The survey has **two parts** and takes about **15 minutes** to complete. By completing this survey, you help us understand how people in the US think about important questions. It is part of a study conducted by researchers from the University of Bonn.

You are not allowed to participate in this study more than once. If you experience a technical error or problem, do not try to restart or retake the study. Rather, send us an email with a description of your problem and we will get back to you. If you have any questions regarding this study, please email felix.chopra@uni-bonn.de

To participate in the study, you have to live in the US, and be 18 years or older.

[Page break]

Please consent to the processing of your data and our privacy policy *Click **here** to display the full privacy policy.*

Your data will be stored and analyzed in full compliance with the highest standards of the data protection laws of the European Union. In particular, no conclusions about your person will be drawn. You can withdraw your consent at any time.

- I consent
- I do not consent

[Page break]

The next question is about the following problem. In questionnaires like ours, sometimes there are participants who do not carefully read the questions and just quickly click through the survey. This compromises the results of research studies. **To show that you are reading the survey carefully, please choose both "Very strongly interested" <u>and</u> "Not at all interested" as your answer to the next question.**

Given the above, how interested are you in politics?

- Very strongly interested
- Very interested
- A little bit interested
- Not very interested
- Not at all interested

[Page break]

As part of this survey, you will listen to an audio recording. You can only participate in this survey if your device can play audio recordings. To see if this works, please try to play the audio below.

[Audio player with controls, see Figure F.3]

Which color was mentioned in the audio recording?
[Dropdown menu]

[Page break]

Please provide us with some information about yourself.

What is your age?
[Dropdown menu]

What is your gender?

- Male
- Female
- Other / Prefer not to say

What was your annual gross household income in 2019?
[Dropdown menu]

What is the highest level of education you have completed or the highest degree you have received?

- Some high school, but no degree
- High school degree (or GED)
- Some college, but no degree
- Associate degree (2-year)

- Bachelor’s degree (4-year)
- Post-graduate degree

With which political party do you identify the most?

- Democratic Party
- Republican Party
- Independent

[Page break]

How would you describe your overall financial knowledge?
[Very low (1), 2, 3, 4, 5, 6, Very high (7)]

Do you usually have money left over at the end of the month that you can save for larger purchases, emergency expenses or to build up savings?
[Yes, No]

Which, if any, of the following types of debt do you have? Please check all that apply.

- Mortgage debt
- Student loan debt
- Credit card debt
- Auto loan debt
- Other types of debt
- I have no debt

[if respondent did not select “I have no debt” in the previous question, display:]

In total, how much debt do you currently have?
[Dropdown menu]

What is the combined dollar value of all your spending on the categories below over the last 7 days?

- food consumed at home
- food consumed away from home
- leisure activities such as visiting the cinema or sport games
- clothing

The combined dollar of my spending on these categories over the last 7 days is...
[Text entry field]

[Page break]

We will now begin with the first part of this survey.

[Page break]

F.4.2 Treatments

On the next page, you will listen to a **5 minute** recording.

[Page break]

Please listen to this audio. We will ask you a few questions about it afterwards.
[Audio player with controls]

You will be able to advance to the next page once you finished listening to the audio.
[Page submit is visible after 5 minutes]

F.4.3 Obfuscation

Please answer these questions about the audio content you just listened to.

Did you enjoy listening to the content?
[Yes, No]

Imagine a local radio station near you would feature content like this. Would you be more or less likely to listen to this station?

- Much more likely
- Somewhat more likely
- About the same
- Somewhat less likely
- Much less likely

How would you rate the production quality of the content?

- Very high
- High
- Low
- Very low

How would you rate the novelty of the content?

- Very high
- High
- Low
- Very low

What is the name of the radio show that you just listened to?
[Text entry field]

On how many days do you listen to the radio in a typical week?
[Dropdown menu, values from 1 to 7]

Which, if any, of the following radio programs have you listened to in the past? Please select all that apply.

- Savage Nation
- Sean Hannity Show
- Dave Ramsey Show
- Marketplace
- BBC World Service
- Howard Stern Show
- Mark Levin Show
- Coast to Coast
- Morning Edition
- I don't listen to these radio shows

[Page break]

You will now continue to the second and final part of this survey.

[Page break]

Please answer these questions about yourself.

Which of the following best describes your race or ethnicity?
[Dropdown menu]

Are you of Hispanic, Latino, or Spanish origin?
[Yes, No]

What is your current employment status?
[Dropdown menu]

In which state do you currently reside?
[Dropdown menu]

What is your zipcode of residence?
[Text entry field]

F.4.4 Post-treatment measures

Suppose you had $100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow?

- More than $102
- Exactly $102
- Less than $102

Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would you be able to buy with the money in this account?

- More than today
- Exactly the same
- Less than today

If interest rates rise, what will typically happen to bond prices?

- They will rise
- They will fall
- They will stay the same
- There is no relationship between bond prices and the interest rate

A 15-year mortgage typically requires higher monthly payments than a 30-year mortgage, but the total interest paid over the life of the loan will be less.
[True, False]

Buying a single company's stock usually provides a safer return than a stock mutual fund.
[True, False]

[Page break]

Information
Would you like to receive free information on how to manage your personal finances and pay off your debt?

- Yes
- No

If you click "Yes", you will receive the information at the end of this survey. If you click "No", you will not receive the information.

[Page break]

How much do you agree or disagree with the statements below?

- There is no excuse for borrowing money
- You should always save up first before buying something
- You can live a good life without borrowing money
- All in all, borrowing money is not worth the cost

[For each item: Strongly agree, Somewhat agree, Neither agree nor disagree, Somewhat disagree, Strongly disagree]

[Page break]

How much do you agree or disagree with the statements below?

- I admire people who own expensive homes, cars, and clothes
- The things I own say a lot about how well I'm doing in life

[For each item: Strongly agree, Somewhat agree, Neither agree nor disagree, Somewhat disagree, Strongly disagree]

[Page break]

In 2019, how much debt did the average American have?
[Slider from $0 to $200,000]

[Page break]

In 2019, what was the share of Americans that had any kind of debt?
[Slider from 0 to 100]

F.4.5 Debrief

What do you think was the main hypothesis of this study?
[Text entry field]

If you have any comments related to this study, please write them down in the field below.
[Text entry field]

[Page break]

For your information, you listened to an excerpt from the [Dave Ramsey Show, Modern Mentor Podcast] previously.

[Page break]

Information about personal finances
Here are some suggestions from the Dave Ramsey Show on how to pay off your debt.

[Figure explaining the 7 Baby Steps from the *Dave Ramsey Show*]

Debt Snowball Method
The debt snowball method is a debt-reduction strategy where you pay off debt in order of smallest to largest, gaining momentum as you knock out each remaining balance. When the smallest debt is paid in full, you roll the minimum payment you were making on that debt into the next-smallest debt payment.

- Step 1: List your debts from smallest to largest regardless of interest rate.
- Step 2: Make minimum payments on all your debts except the smallest.
- Step 3: Pay as much as possible on your smallest debt.
- Step 4: Repeat until each debt is paid in full.

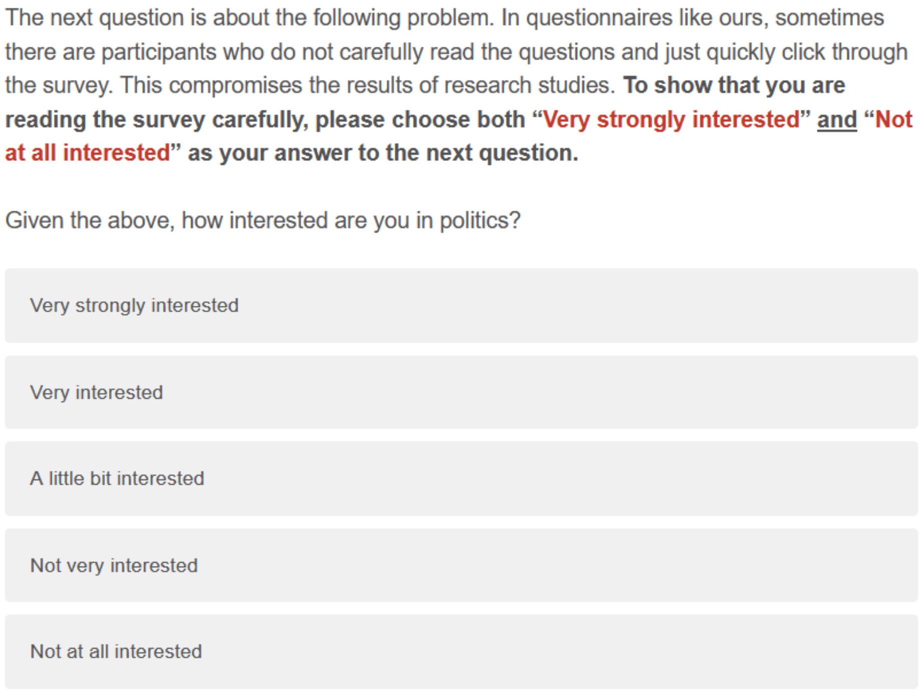
Now, before you start arguing about the interest rates, hear us out. If your largest debt has the largest interest rate, it's going to be a long time before you start to see a dent in that crazy balance of yours. But when you stick to the plan (without worrying about interest rates), you're going to be jumping up and down when you pay off that smallest debt super quick. That excitement is what's going to motivate you to keep working hard—all the way to that debt-free finish line.

How useful was this information?
[Very useful, somewhat useful, not useful, not useful at all]

[End of survey]

F.4.6 Screenshots

Figure F.2: Attention check



The next question is about the following problem. In questionnaires like ours, sometimes there are participants who do not carefully read the questions and just quickly click through the survey. This compromises the results of research studies. **To show that you are reading the survey carefully, please choose both "Very strongly interested" and "Not at all interested" as your answer to the next question.**

Given the above, how interested are you in politics?

Very strongly interested

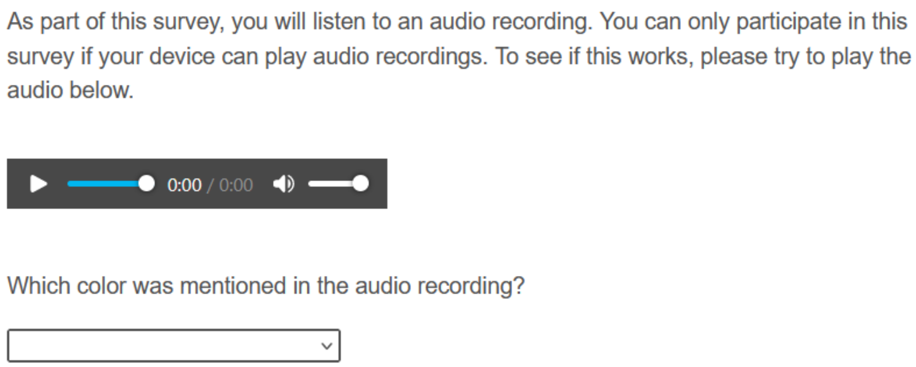
Very interested

A little bit interested

Not very interested

Not at all interested

Figure F.3: Audio check



As part of this survey, you will listen to an audio recording. You can only participate in this survey if your device can play audio recordings. To see if this works, please try to play the audio below.

0:00 / 0:00

Which color was mentioned in the audio recording?

F.5 Audio transcript

The control group listened to Episode 277 of the Modern Mentor Podcast by Stever Robbins and published on August 25, 2015. Respondents listened to the 5 minutes and 8 seconds segment from 00:00:09 to 00:05:17. The treatment group listened to an excerpt from the *Dave Ramsey Show*, which was published on March 20, 2017, on the radio show's YouTube channel.[1] Respondents listened to the 5 minute and 4 seconds segment from 00:00:00 to 00:05:04. A verbatim transcript of both excerpts can be found below.

F.5.1 Control group

They say you should choose your battles wisely. That makes sense. Consider Napoleon. He chose to fight at Waterloo, and that didn't work out well for him. If he'd chosen more wisely, he might have chosen to fight at Gettysburg. He would have given the Gettysburg Address and had a movie made about him, only instead of starring Daniel-Day Lewis, it would have started Daniel DeVito. One unwisely-chosen battle centuries ago changed the entire course of the Academy Awards centuries later. In our daily lives, choosing battles unwisely means we can waste a lot of time and energy on the wrong thing. This very evening, listener Emily proclaimed on her Facebook wall that she was thrilled that a business celebrity sent her a message. Imagine my surprise to find out she was talking about me! I could have spent time arguing that I'm certainly not a celebrity, and I'm far too humble and modest to deserve such acclaim and adoration. But what would have been the point? I'm sure you'll agree it makes much more sense to accept her statement at face value—as simply a statement of fact—and save my energy for an important battle. Where in your life and work do you fight battles? Why? Are those the right battles? Let's explore how you can make sure you fight less and win more.

I know this sounds obvious, but before going into battle, ask yourself honestly whether you can win. I know you feel you can win but think it through. A coaching client was furious that his biggest customer had stolen some of his technology. He wanted to fight it out in court, but if he won the lawsuit, he'd lose the customer and go out of business. This battle couldn't be won.

It's like trying to get your boyfriend, girlfriend, husband, wife, spousal equivalent, or polyamorous family unit to put the toilet paper roll on with the paper facing the other direction. Not only will you lose that battle, but you'll end up bringing home flowers for a month to repair the damage you made with that foolish, foolish request. You cannot win that battle. So why try?

If you do win, make sure you'll get some benefit from the win. I know people who spend years obsessing over how they were right and Jordan Dinklebert was wrong, but Jordan wouldn't listen and insulted them in front of the entire team. Now they're just waiting for a chance to take revenge. They spend years plotting, and the day they're named employee of the year, halfway through their acceptance speech, they say, "And it's no thanks to Jordan Dinklebert. I was right, you were wrong, and you're really just a big poopie head. So there!" Uh, huh. A poopie head. Well, that little bit of revenge was certainly worth the wait.

Revenge is usually a battle that takes up a lot of resources, and even if you win, you don't really benefit. In Star Trek II: The Wrath of Kahn, Kahn declares, "Revenge is a dish best served cold." Really? Who wants

[1]The full video can be found here: `https://www.youtube.com/watch?v=vz-rdaE2uUw`

a cold dinner? Revenge is not a dish best served cold. Oreo ice cream cake is a dish best served cold. So what's the lesson here?

Even if you benefit, make sure you benefit enough to be worth the fight. Take this example: A non-profit organization owned a parcel of undeveloped land. A developer wanted it. He sued the non-profit with a frivolous lawsuit and offered to settle if the non-profit would sell the developer the land for $100,000, which was market price.

The non-profit, on principle, didn't want to give in. But they weren't using the land for anything. And in America, it can cost $20,000 to get a frivolous lawsuit thrown out of court. And the developer, with lawyers on staff, could just sue again. The non-profit realized that even though they could win and keep the land, that win would cost them $20,000. If they didn't fight, they would walk away with $100,000. Were they getting shafted? Yes. But were they smart? Definitely. They chose not to fight a battle that wasn't worth the fight.

Last but not least, consider how else you could spend your time. Even for a battle you can win that is worth the fight, there may be better ways to use your time. One of my clients was spending a lot of time and energy pursuing a contractor who had done shoddy work to his home, defrauding him out of $50,000. When we explored the decision to pursue the case in court, and figured that, given the contractor's resources, my client would recover $25,000 at most, if he won. It would probably take him a day a week for six months, which is 26 days. An entire work month. And that's the best-case scenario.

We looked seriously at all the other opportunities in my client's life and work and realized that he had some business development opportunities that would bring in a six-figure contract if he could work on them full time. The battle with the contractor? He could win. He'd benefit. It would be worth it. But he could spend the same time doing business development instead and make even more money. He chose to forgo the battle and spend his time doing business development. Smart. Next time you start gearing up for a fight, stop. Make sure it's a battle you can win. Make sure you'll benefit if you win it. Make sure the benefit is large, and finally, that there isn't something else you could do instead to get even more benefit elsewhere in your life.

F.5.2 Treatment group

If you wanna to win with money, let me give you a good idea. Figure out what most people are doing and run in the other direction. *Run* in the other direction. Most people are broke. Most people look good, and their broke. They spend more than they have coming in. They don't act their wage. They don't live on a plan. They don't agree on spending with their spouse. Their only hope for retirement is that the government, which is well known for its ability to handle money, will take care of them. They don't have money set aside for emergencies. They run credit card debt and student loans and car debt all day, every day. They spend like they're in Congress. Most people are stupid when it comes to money.

70% of Americans are living paycheck to paycheck. The bankruptcy rate is at an all-time high, and foreclosures are rising again. Credit card debt continues to climb, and we have a trillion dollars of student loan debt out there. The average car payment in America today now is 496 dollars over 84 months. That's stupid. Normal in America is broke and stupid. You don't wanna be normal. You wanna be weird. One of the greatest compliments you can get on this show if you call up and I say, "Man, you're weird. I'm looking at weird people. You guys are weird", which means that you're contrary. You are a contrarian.

You're perpendicular to the culture. When the culture has lost its way the best thing you can do is be opposite.

Figure out whatever they're doing and do the other thing, right? Because you're not gonna get...you're only going to get what they're getting when you do what they're doing. This is not hard to figure out. If you keep doing what you've been doing, you're gonna keep getting what you've been getting. You do reap what you sow. You live in a cause-and-effect world, baby. There is no way around this.

So your goal... When I went broke, my goal is to be weird. My goal was to be different. And personal finances is 80% behavior, it's only 20% head knowledge. So, this not some math formula that you have a problem with, this is a person in your mirror. I figured out if I can make the guy in my mirror behave, he can be skinny and rich. He's got issues. And once we realize that behavior is what causes people to handle their money poorly or handle it well, then what we've got to decide is our behaviors. And if you have the same behaviors as broke people have in when it comes to money, you're gonna have the same results as broke people. You're just gonna be another broke person. And some of you are making 250,000 dollars a year and you're broke. You've got no money at all. You've got a mess. Loans coming out your ears. You can't breathe. You run, run, run, run, run, run, run like a rat in a wheel, have a heart attack and die and wonder what happened.

This is no way to live. Buying things you can't afford with money you don't have to impress people you don't really like. Some of you spend an unbelievable amount of money on a car payment to impress someone at a stop light you will never be introduced to. The buddy you felt cool there for about, what, three and a half seconds? Fool.

I've been that fool, that's why I know who he is. I've been that guy, I've been that shallow where I thought that my car actually mattered to somebody. Give me a break. Nobody gives a rip about your car. It, listen, you know what I drive right now? Anything I want. You know why? Because I drove crap for a long time. I drove cars like nobody else would drive. Now I get to drive whatever I wanna drive, and I don't drive them for you. I drive them because I like them. I couldn't give a... care less what you think about what I drive. It's not my problem. It's not your problem either by the way. I'm gonna enjoy. Boy, I like nice cars. But I'm not gonna have a nice car with a stupid car payment on it. It's ridiculous. If your self-esteem is so screwed up that you're doing that then you're gonna struggle with money. You're normal. People spending a bunch of money to act like they're something they're not. What they call in Texas "big hat, no cattle." You need to decide: I don't care what other people think and I'm gonna be weird. Whatever you're doing with money, I'm going to do the opposite thing. And when you decide that, you will start winning with money.

F.6 Experimental instructions: Follow-up study

Household Finance Survey 2021

Thank you for your interest in this survey, which is part of a study conducted by researchers from the Bonn Graduate School of Economics. By dedicating **5 minutes** of your time to complete this survey, you help us gain valuable insights about personal finances in America.

Your data will be stored and analyzed in full compliance with the General Data Protection Regulation. In particular, your responses are confidential and no conclusions about your person will be drawn. You can withdraw your consent at any time.

You can read the full privacy policy by clicking here.

Please consent to the processing of your data and our privacy policy.

- I consent
- I do not consent

[Page break]

The next question is about the following problem. In questionnaires like ours, sometimes there are participants who do not carefully read the questions and just quickly click through the survey. This compromises the results of research studies. To show that you are reading the survey carefully, please choose both "Very strongly interested" and "Not at all interested" as your answer to the next question.

Given the above, how interested are you in sports?

- Very strongly interested
- Very interested
- A little bit interested
- Not very interested
- Not at all interested

[Page break]

What is your age?
[Dropdown menu]

What is your gender?

- Male
- Female
- Other / Prefer not to say

In which region do you currently reside?

- Northeast
- Midwest
- South
- West

How many people usually live in your primary residence (including yourself, and excluding non-relatives like roommates or renters)?
[Dropdown menu]

[Page break]

Do you hold any shares of stock in publicly held corporations, stock mutual funds, or investment trusts?

- Yes
- No

How many credit cards do you have?
[Dropdown menu]

[Page break]

We would like to learn more about your primary bank.

[Page break]

How satisfied are you with your primary bank's...?

- Customer service
- Checking account
- Branch and ATM locations

- Mobile banking
- Online banking

[For each item: 5-point scale from “Very satisfied” to “Very dissatisfied”]

[Page break]

How likely are you to recommend your primary bank to a friend or colleague?
[11-point Likert-scale from “Not at all likely” to “Extremely likely”]

[Page break]

Now think about household finances more generally.

[Page break]

How much do you agree or disagree with the statements below?

- There is no excuse for borrowing money
- You should always save up first before buying something
- You can live a good life without borrowing money
- All in all, borrowing money is not worth the cost

[For each item: Strongly agree, Somewhat agree, Neither agree nor disagree, Somewhat disagree, Strongly disagree]

[Page break]

How much do you agree or disagree with the statements below?

- I admire people who own expensive homes, cars, and clothes
- The things I own say a lot about how well I’m doing in life

[For each item: Strongly agree, Somewhat agree, Neither agree nor disagree, Somewhat disagree, Strongly disagree]