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Leveraging Large-Scale Granular Single-Source Data for TV Advertising

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Abstract

This study introduces a novel instrumental variable (IV) for estimating the causal effects of linear TV advertising using large-scale panel data that link household second-by-second show viewership and ad exposure with daily purchase behavior. We exploit an institutional feature of linear TV: while advertisers choose which shows to target, networks quasi-randomly determine within-show ad airing times. This creates exogenous variation in focal brand ad exposure among partial show viewers, which we nonparametrically extract to construct a household-show-level IV. We establish the IV's validity in the presence of endogeneity arising from advertisers' show targeting decisions and households' TV viewing behavior. Our IV offers a generalizable and flexible solution for household-level linear TV ad effect measurement using modern single-source data. Applying this method to data from a major food delivery platform, we estimate an ad response model in which both baseline purchase propensity and ad responsiveness vary with purchase history. Naïve estimates overstate ad elasticities by 55% compared to IV-corrected estimates. We also find that ad responsiveness is nonmonotonic with respect to purchase frequency and recency. These findings underscore the importance of addressing endogeneity in observational household TV ad exposure data and highlight the potential of behaviorally targeted TV advertising.

Keywords: TV Advertising, Causal Inference, Instrumental Variable, Ad Response Model, Single-Source Data

1. Introduction

We propose a method to address a persistent challenge faced by advertisers: how to better quantify the causal effects of linear TV ads using observational data that link household-level ad exposures with purchases over extended periods. This method is especially timely, as TV advertisers and their agencies increasingly gain access to modern single-source data that track second-by-second TV viewership for millions of households and merge it with advertisers' first-party records of customer purchase history.[1]

Despite the advent of a new generation of TV audience measurement data, the fundamental challenge of causal inference remains. Analysts must still contend with two key sources of endogeneity in observed household linear TV ad exposures: targeting bias and activity bias (Lewis et al. 2011, Lewis and Rao 2014, Zhang et al. 2017, Gordon et al. 2019). Targeting bias arises when a focal brand places ad buys in shows based on the baseline purchase propensities of the audience. Activity bias occurs when individual households' TV viewership at specific times correlates with their baseline purchase propensities for the focal brand during those times.

Recent research comparing effect estimates from observational data with those from randomized controlled trials (RCTs) in digital advertising suggests that overcoming these biases with observational data is far from straightforward. Neither extensive controls nor algorithmic flexibility appears sufficient to fully address the endogeneity inherent in the data-generating process (DGP) underlying observed household-level ad exposures (e.g., Gordon et al. 2019, Gordon et al. 2023).

In contrast to digital advertising, RCTs for linear TV ads are largely impractical. Advertisers cannot readily randomize ad exposure at the household level due to the broadcast nature of linear TV and the limited infrastructure for customized ad delivery. Consequently, both practitioners and academics primarily rely on observational data. While many identification strategies have been proposed, most focus

[1] With the advent of automatic content recognition (ACR) technology and the widespread adoption of ACR-enabled smart TVs and set-top boxes (STBs), providers such as Comscore, iSpot, and LG Ad Solutions now collect second-by-second TV viewership data from tens of millions of households (NBCUniversal 2022). When such granular TV viewing data from large-scale panels are merged with advertisers' first-party response data (e.g., via meshed IP-address matching), they give rise to what we refer to as modern single-source data.

on aggregate data and exploit non-experimental, exogenous variation in TV ad exposures at the market level (e.g., Hartmann and Klapper 2017, Stephens-Davidowitz et al. 2017, Shapiro 2018, Thomas 2020). These strategies are ill-suited for household-level analysis, as they cannot capture variation across households within the same market or over time within the same household.

We address this gap by proposing an identification strategy that is both generalizable and flexible. It is generalizable in that it does not rely on exogenous variation unique to a particular advertiser or campaign. It is flexible in that it can be applied to millions of households with repeated ad exposures and purchases observed over extended periods. Such an identification strategy facilitates practical adoption, enabling improved measurement of cross-sectional heterogeneity in TV ad effects and clearer differentiation between short-term responses and long-term effects such as carryover and state dependence. A more accurate and nuanced understanding can help advertisers refine strategies and enhance the effectiveness of TV as an advertising medium.

The core of our identification strategy leverages an institutional feature of U.S. linear TV: while advertisers choose which shows to target, networks control the exact timing of ad airings within those shows. To ensure fairness, networks typically implement an equitable rotation of advertisers' ads across the available ad slots within a show—a practice commonly referred to as "quasi-random ordering" (Wilbur et al. 2013, Gordon et al. 2021, McGranaghan et al. 2022). This generates exogenous variation in focal brand ad exposure when a household watches only part of a targeted show, as exposure depends on whether focal brand ad airing time happens to fall within the segment(s) viewed by the household.

Building on this insight, we propose a household-show-level instrumental variable (IV) to identify the causal effects of linear TV ad exposures. Intuitively, when a household watches only part of a show targeted by the focal brand, the network's quasi-random assignment of within-show ad slots serves as a source of natural experimentation, introducing a degree of randomness in realized focal brand ad exposure that is uncorrelated with its endogenous determinants.

We isolate this network-induced exogenous shock by nonparametrically estimating a household's expected focal brand ad exposure based on (a) its second-by-second viewership of the targeted show and

(b) the empirical distribution of the network's ad airing times across show durations. The deviation of the household's realized ad exposure from this expectation constitutes our household-show-level IV.

Conceptually, the validity of our decompositional approach to IV construction—subtracting expected from realized treatment—depends on the former capturing all endogenous determinants of the latter, such that the residual contains only exogenous variation. In other words, our identification strategy hinges on obtaining an estimate of expected focal brand ad exposure that accounts for all pathways through which unobserved confounders may influence realized exposure (e.g., via the focal brand's show targeting decisions or the household's show viewing behavior).

Two identifying assumptions are required for our proposed IV to satisfy the exogeneity condition. First, we assume that networks assign within-show ad slots quasi-randomly across advertisers, regardless of the identity of the focal brand or household. This "quasi-random ordering" assumption ensures a degree of exogeneity in the exact timing of focal brand ad airing within a targeted show.

Second, we assume that a household's viewership of a targeted show is independent of the timing of the focal brand ad during that show. That is, households are assumed to watch or skip the focal brand ad in the same manner as ads from other brands, without selectively adjusting their viewing behavior based on whether they are exposed to the focal brand ad. This "non-strategic viewership" assumption enables us to estimate a household's probability of within-show focal brand ad exposure based on its actual viewership of the targeted show. In our empirical application, both identifying assumptions pass their respective falsification checks.

As we demonstrate through formal proofs in a general setting, as well as a stylized numerical example with a known DGP, our proposed IV is valid and exhibits (a) zero mean, (b) positive correlation with realized treatment, and (c) no correlation with confounders, provided the identifying assumptions hold. Although instrument exogeneity in a real-world setting is inherently untestable, our method of constructing the IV from observables enables falsification checks that can be implemented in empirical applications—an approach we also validate through both formal proofs and the numerical example.

We apply our method to a panel dataset combining second-by-second TV viewing data from LG Ad Solutions (LGADS)[2] with first-party purchase data from a major food delivery platform—the focal brand in our study. Our data cover 1.4 million U.S. households over a 4.5-month period (November 15, 2020–March 28, 2021), linking linear TV ad exposures to daily household purchase behavior over time.

Our empirical application focuses on two primary goals. First, we illustrate the use of the proposed IV to estimate the causal impact of linear TV advertising. We empirically verify that, across networks, the within-show timing distributions of focal brand ads are statistically indistinguishable from those of non-focal brands, consistent with the practice of quasi-random ordering in linear TV. We also show that our IV passes the falsification checks—exhibiting zero mean and no correlation with the expected treatment—thereby supporting its validity in our empirical setting.

Second, we leverage repeated ad exposures and purchases over time to examine how ad effects vary with households' purchase histories. This is important because it enables advertisers to target TV ads—much like digital ads—not only based on demographic variables such as age and gender, but also on past purchase behavior. As the availability of addressable TV ad inventory continues to grow, targeting based on behavioral history is becoming increasingly relevant (Malthouse et al. 2018, eMarketer 2022).

To achieve the goals of our empirical application, we specify a household daily ad response model in which baseline purchase propensity and ad responsiveness vary with purchase frequency (i.e., number of past purchases) and recency (i.e., days since the last purchase). To correct for endogeneity, we incorporate the proposed IV via a control function term in the latent utility of a probit purchase model.

Two key findings emerge. First, failing to correct for endogeneity results in a 55% overstatement of TV ad effectiveness: average same-day (30-day) ad elasticity declines from 0.072 (0.222) to 0.045 (0.143) after IV correction. Second, both baseline purchase propensity and ad responsiveness vary significantly with purchase history, indicating state dependence. For baseline purchase propensity, we

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[2] LGADS collects TV viewing data through Automatic Content Recognition (ACR) technology from over 35 million opt-in smart TVs in the U.S., spanning various TV brands including LG, Seiki, Skyworth, RCA, and Askey. Retrieved on August 19, 2024 from https://lgads.tv/tv-data/.

observe a "habit formation" effect, whereby a prior purchase increases the likelihood of a subsequent one, and a "recency trap" effect, whereby the likelihood of a new purchase declines as more time elapses since the last purchase. For ad responsiveness, we find an inverted U-shaped relationship with purchase frequency and a U-shaped relationship with purchase recency.

Together, these findings underscore (a) the debiasing power of the proposed IV and (b) the value of targeting TV ads based on household purchase history. Unlike causal inferences based on aggregate data—which obscure cross- and within-household variation in ad exposures and responses—more accurate and nuanced ad effect estimates derived from modern single-source data enable TV advertisers to better identify which consumers to target and when. This methodological advance offers a powerful tool for enhancing the return on investment (ROI) of TV advertising.

2. Related Literature and Intended Contributions

Our research contributes to a growing body of work that develops identification strategies to estimate the causal impact of TV advertising using observational data. Table 1 summarizes selected studies focused on causal identification in the context of TV advertising. Notably, most existing strategies are designed for market-level rather than household-level data. For example, Hartmann and Klapper (2017) and Stephens-Davidowitz et al. (2017) use regional shocks in exposure to national Super Bowl ads to identify the impact of Super Bowl TV advertising. Shapiro (2018) exploits variation in TV ad exposures across DMA border areas as a source of identification. Thomas (2020) leverages the spillover of mass media advertising into smaller local markets for causal inference.

Li et al. (2024) develop an IV based on preference externalities, wherein an individual's treatment depends on the preferences of others in the group. Sinkinson and Starc (2019) and Moshary et al. (2021) identify the causal effect of TV advertising using shocks in demand for political TV ads during election cycles. Joo et al. (2014), Liaukonyte et al. (2015), and Du et al. (2019) exploit the precise timing of TV ad insertions and use narrow temporal windows (e.g., one hour before and after) to identify immediate effects on aggregate consumer responses at the brand level. McGranaghan et al. (2022) apply a similar identification strategy to individual-level data, focusing on the immediate effect of ad exposure on TV viewing behavior (i.e., tuning, presence, and attention), and treat ad exposures as exogenous based on the quasi-random ordering of ads within a show.[3]

Table 1. Selected Prior Studies on Causal Identification Strategies in TV Advertising

	Unit of Analysis	Temporal Granularity	Dependent Variable (Industry)	Identification Strategy	Ad Elasticity
Joo et al. (2014)	Nation	Hourly	Online search (financial services)	Exogeneity in precise ad insertion timing	0.17
Hartmann and Klapper (2018)	DMA	Weekly	Sales (beverage)	Exogeneity in local Superbowl viewership	0.03-0.1
Shapiro (2018)	County	Monthly	Sales (pharmaceuticals)	DMA border discontinuity	0.01-0.037
Sinkinson and Starc (2019)	DMA	Monthly	Revenue (pharmaceuticals)	Political advertising as IV	0.076
Thomas (2020)	Store	Weekly	Sales (pharmaceuticals)	Spillover from mass advertising as IV	0.042
Li et al. (2024)	County	Yearly	Vote share (presidential elections)	Preference externality caused by others as IV	-
Current Study	Household	Daily	Purchase (food delivery services)	Quasi-random within-show ad airing timing	0.045 (same-day) 0.143 (30-day)

Notes. This table presents a selection of representative studies in the literature; other studies employing similar methods, as referenced in the text, are omitted for brevity.

We advance the literature on causal identification in TV advertising by introducing a novel and generalizable IV using household-level observational data. Although the quasi-random ordering of ads within a show has been leveraged in prior studies (e.g., Liaukonyte et al. 2015, McGranaghan et al. 2022), our innovation lies in how we exploit this variation: we construct an instrument by taking the residual between a household's realized treatment and its expected treatment, conditional on the household's second-by-second show viewership and the network's empirical distribution of ad airing times across show durations.

Our research also contributes to prior literature examining the effects of TV advertising on household purchase dynamics using single-source data. Unlike traditional scanner panel-based single-source data, the modern single-source data employed in our study offer significantly greater granularity

[3] Most prior studies using household-level data have not explicitly addressed the endogeneity of TV ad exposure. Notable exceptions include Lodish et al. (1995) and Hu et al. (2007), who employ split-cable field experiments, and Tuchman et al. (2018), who use a simultaneous equation model to account for endogenous ad-skipping behavior.

and scale. These advancements enable several distinctive features that set our work apart from earlier studies, as summarized in Table 2.

Table 2. Selected Prior Studies on the Impact of TV Advertising Using Single-Source Data

	Data				Model		
	Number of Households	Temporal Granularity	Trial Purchase Observed	TV Viewing Data	State Dependence in Baseline Propensity	State Dependence in Ad Responsiveness	Treatment of Endogeneity
Tellis (1988)	251	Weekly	No	Ad Exposure	Frequency	Frequency	No
Pedrick & Zufryden (1991)	584	Daily	No	Ad Exposure	Frequency & Recency	No	No
Deighton et al. (1994)	167	Weekly	No	Ad Exposure	Frequency	Frequency	No
Lodish et al. (1995)	389	Weekly	No	Test Conditions	No	No	Split Cable Tests
Tellis & Weiss (1995)	162	Daily	No	Ad Exposure	Frequency	No	No
Ackerberg (2001)	1,775	Weekly	Yes	Ad Exposure	Frequency	Frequency	No
Ackerberg (2003)	1,775	Weekly	Yes	Ad Exposure	Frequency	Frequency	No
Hu et al. (2007)	3,000	Weekly	No	Test Conditions	No	No	Split Cable Tests
Deng & Mela (2018)	834	Second-by-Second	No	Ad & Show Exposure	Frequency	No	No
Tuchman et al. (2018)	6,437	Second-by-Second	No	Ad Exposure	Frequency	Frequency	Simultaneous Equation
Current Study	1.4 Million	Second-by-Second	Yes	Ad & Show Exposure	Frequency & Recency	Frequency & Recency	IV with Control Function

The first distinction is our sample size. Our panel of 1.4 million households is at least two orders of magnitude larger than the single-source data panels used in prior studies (e.g., 1,775 households in Ackerberg 2001, 2003), providing the statistical power necessary to implement our identification strategy and to measure dynamic ad effects such as state dependence.

Another distinctive aspect of this research is our approach to capturing how the impact of TV advertising evolves as a function of past purchases. Accounting for these dynamics is crucial because, for example, the impact of TV advertising may be overestimated if intertemporal substitution is overlooked (Lambrecht et al. 2023), or underestimated if accelerated habit formation is not considered. With our identification strategy and ad response model, advertisers can assess the relative efficacy of targeting TV ads not only based on generic consumer characteristics such as age and gender, but also on prior purchase frequency and recency—an approach that is highly relevant to the ongoing debate about the cost-effectiveness of TV advertising and the need for strategies to improve its ROI (Shapiro et al. 2021).

Finally, while much prior research using traditional single-source data has focused on consumer packaged goods (CPGs) tracked through scanner panels (e.g., Bronnenberg et al. 2008), our study extends the empirical context by linking TV viewing data to first-party customer purchase data from a digital platform. A notable contextual difference is that, for CPGs, consumers typically respond to TV ads during subsequent shopping trips, resulting in a longer lag between ad exposure and purchase. In contrast, for digital platforms like our focal brand, consumers can respond to ad exposure more quickly via mobile or desktop devices, leading to a shorter response window and potentially different patterns of carryover effects.[4] In short, our study complements prior CPG-focused research and illustrates how modern single-source data can enrich our understanding of TV advertising effectiveness across diverse industry contexts.

3. Identification Strategy

3.1. Sources of Endogeneity and Exogeneity

In the context of linear TV, a household's exposure to a focal brand ad within a show results from the intersection of three decisions made by distinct entities. First, the focal brand must purchase an ad slot within the show (the show-targeting decision). Second, the household must watch at least part of the show (the show-viewing decision). Third, the network broadcasting the show must air the focal brand's ad during the portion of the show that the household watches (the within-show ad airing time decision).

Endogeneity in the focal brand's show-targeting decision arises when the brand strategically places its linear TV ad buys. For example, households that frequently order food delivery may also tend to watch more live sports. Aware of this pattern, the focal brand might increase ad placements during sports programming, generating a spurious correlation between ad exposures and purchases. Similarly, during holidays—when households may be more likely to cook at home or dine out, and thus less likely to order food delivery—the brand may reduce its ad buys, introducing another source of spurious correlation. More generally, the focal brand may act on information about shows and time periods that

[4] This pattern is evident in our empirical setting. Our model estimates indicate a daily carryover of approximately 0.7, which is substantially lower than the weekly carryover of 0.9 (equivalent to a daily carryover of 0.985) reported in prior studies on CPGs (Shapiro et al. 2021).

correlate with households' baseline purchase propensities, even if such information is unobservable to analysts. We refer to this focal brand-induced source of endogeneity as "targeting bias."

Even if the focal brand made its linear TV ad buys randomly—allocating ad placements across shows or days nonstrategically—endogeneity could still arise from the household's show-viewing behavior. For instance, all else equal, a household that watches more TV is a priori more likely to be exposed to focal brand ads. If factors that affect TV viewing—such as time spent at home or overall busyness, which may be unobservable to analysts—also influence baseline demand for food delivery services, this could generate a spurious correlation between ad exposures and purchases. We refer to this household-induced source of endogeneity as "activity bias."

A common approach to mitigating targeting and activity biases is to incorporate control variables into an ad response model.[5] However, it is impossible to account for all the "unknown unknowns" using control variables, particularly time-varying confounders at the household level. For example, if someone unexpectedly has to work an extra hour on a given day, they are likely to (a) watch less TV and thus be less likely to view a targeted show (or only a smaller portion of it), thereby reducing their chance of exposure to a focal brand ad, and (b) have less time to cook, thereby increasing their likelihood of ordering food delivery. Such household- and time-specific unobservables that affect both ad exposures and purchases pose an endogeneity threat that control variables alone cannot adequately address.

Fortunately, in linear TV, a household's exposure to a focal brand ad during a targeted show is not solely determined by the focal brand's show-targeting decision and the household's show-viewing behavior. For a household that watches part of a targeted show, focal brand ad exposure also depends on whether the viewed segment(s) overlap, at least partially, with the time slot of the focal brand ad. In other words, the network's within-show ad airing time decision introduces an exogenous shock to a

[5] Observed household characteristics, along with random or fixed household effects, can be included as controls when a household's preference for targeted shows and its baseline demand for the focal brand are both correlated with these characteristics. Similarly, observed temporal factors, along with random or fixed time effects, can be included as controls for time-varying unobservables that affect all households in a similar manner.

household's treatment status when a targeted show is watched partially. As we will show, this shock can be extracted nonparametrically to construct an IV that is valid under two identifying assumptions.

3.2. Data-Generating Process

We begin by formalizing the DGP to clarify the sources of endogeneity and exogeneity in observed household focal brand linear TV ad exposures and to lay the groundwork for our identification strategy. Let $A_{is} \in \{0,1\}$ indicate whether household *i* receives a focal brand ad exposure during linear TV show *s*. Define A_{it} as the total number of focal brand ad exposures household *i* receives on day *t*: $A_{it} = \sum_{s \in S_t} A_{is}$, where S_t denotes the set of available shows on day *t*.

Household Purchase Decision (Y_{it}). Setting aside long-term effects for ease of exposition, the focal brand purchase decision of household *i* on day *t*, $Y_{it} \in \{0,1\}$, can be expressed as a function of same-day focal brand ad exposure A_{it} as follows:

$$Y_{it} = f(\alpha + \beta A_{it} + \gamma X_{it} + u_{it}) \tag{1}$$

where $f(\cdot)$ is a generic link function (e.g., probit or logit), β captures the causal effect of same-day ad exposures, X_{it} includes observed characteristics of household *i* on day *t*, and u_{it} denotes an unobserved (to the analyst) demand shock, with $E(u_{it}) = 0$.

Endogeneity arises when A_{it} in Equation (1) is correlated with u_{it}. Our goal is to obtain an unbiased estimate of β. Since A_{it} is aggregated over A_{is}, it is sufficient to describe the DGP of A_{is}.

Focal Brand Show Targeting Decision (A_s^b). For each linear TV show *s*, the focal brand *b* decides whether to make an ad buy, denoted by $A_s^b \in \{0,1\}$. Without loss of generality, A_s^b can be represented by an indicator function that depends on observed show characteristics X_s and unobserved (to the analyst) show characteristics u_s, with $E(u_s) = 0$. Formally, we have:

$$A_s^b = f_b(X_s, u_s) \tag{2}$$

We remain agnostic about the exact form of $f_b(\cdot)$. It suffices to assume that for all $s \in S_t$, u_s comprises a vector of $\{u_{is}\}$, where u_{is} denotes an expanded set of confounders, one of which may be the demand shock u_{it} in Equation (1). Consequently, Equation (2) accommodates targeting bias, as u_{it} can introduce a spurious correlation between Y_{it} and A_{it} by affecting both the household's focal brand purchase decision and the brand's show targeting decision A_s^b.

Household Show Viewing Decision ($\boldsymbol{View_{is}}$). With 0 and 1 denoting the start and end of show *s*, household i decides whether to watch the show and, if so, which segment(s), denoted by $View_{is} \subseteq [0,1]$. For example, $View_{is} = \{[0,0.1],[0.5,1]\}$ if household *i* watches the first 10% and the last 50% of show *s*. We assume $View_{is}$ is a function of observed household characteristics X_{is} and unobserved characteristics u_{is}, as follows:

$$View_{is} = f_v(X_{is}, u_{is}) \tag{3}$$

where $f_v(\cdot)$ maps $\{X_{is}, u_{is}\}$ to viewing segment(s) $View_{is} \subseteq [0,1]$, with two extreme cases: $View_{is} = \emptyset$ if the household does not watch the show, and $View_{is} = [0,1]$ if the household watches the entire show.

Like $f_b(\cdot)$ in Equation (2), we remain agonistic about the exact form of $f_v(\cdot)$ in Equation (3). It suffices to assume that $View_{is}$ can be influenced by u_{is}, the expanded set of confounders that may include the demand shock u_{it}. As such, the DGP for $View_{is}$ accommodates activity bias, as u_{it} can introduce a spurious correlation between Y_{it} and A_{it} by affecting both the household's focal brand purchase decision and its show viewing behavior $View_{is}$, and, in turn, its likelihood of focal brand ad exposure.

Network Within-Show Ad Airing Time Decision ($\boldsymbol{T_s^b}$). We assume that the network broadcasting show *s* sets the within-show ad airing time for focal brand *b* if the brand makes an ad buy for the show, as follows:

$$T_s^b \sim l_s(x), \text{ with } x \in [0,1] \tag{4}$$

where $l_s(\cdot)$ denotes a probability density function (PDF) that is nonzero only on the interval $[0,1]$, and the within-show ad airing time T_s^b is drawn from the distribution defined by $l_s(\cdot)$.[6] The value of T_s^b is observed only when focal brand *b* targets show *s*, i.e., when $A_s^b = 1$; when $A_s^b = 0$, $T_s^b \notin [0,1]$.

[6] For ease of exposition, T_s^b is treated as a point rather than an interval.

Household Ad Exposure A_{is}. Given the DGPs for the focal brand's show targeting decision A_s^b, the household's show viewing behavior $View_{is}$, and the network's within-show ad airing time decision T_s^b, household *i*'s focal brand ad exposure status in show *s*, A_{is}, can be expressed as:

$$A_{is} = A_s^b \times I(T_s^b \in View_{is}) \tag{5}$$

Equation (5) implies that $A_{is} = 1$ if and only if: (a) $A_s^b = 1$, indicating that the focal brand targets show *s*; and (b) $I\left(T_s^b \in View_{is}\right) = 1$, indicating that the network broadcasting show *s* airs the focal brand ad during the portion of the show viewed by the household. Here, $I(\cdot)$ denotes the indicator function.

Identifying Assumptions. Two identifying assumptions are implicit in the DGP described above.

First, *Quasi-Random Ordering Assumption* ($T_s^b \sim l_s(x)$). Because $l_s(\cdot)$ is indexed by neither brand nor household, the implicit assumption is that the network broadcasting show *s* determines the within-show ad airing time T_s^b according to the same process across brands and households. In other words, although we remain agnostic about the exact form of $l_s(\cdot)$, we assume the network does not tailor its within-show ad airing time assignment process to the focal brand or to any specific household.

If this assumption holds, analysts can infer $l_s(\cdot)$ from the observed distribution of within-show ad airing times. The assumption would be violated if, for example, the focal brand systematically secured a specific ad slot in advance (e.g., the first position in a commercial pod), or if the network adjusted the focal brand's ad airing time based on individual household characteristics (e.g., through addressable TV). However, in the context of linear TV, such a quasi-random ordering assumption generally holds and can be subjected to a falsification check by comparing the empirical distributions of within-show ad airing times for focal versus non-focal brands. If the distributions differ significantly, it would warrant caution in relying on the quasi-random ordering assumption.

Second, *Non-Strategic Viewership Assumption* ($View_{is} \perp T_s^b$). As specified in Equation (3), $View_{is}$ is not a function of a household's within-show focal brand ad exposure status. The implicit assumption is that the household's show viewership would be the same regardless of whether the focal brand's ad airing time T_s^b happens to fall within the segment(s) of the show watched by the household.

This assumption would be violated if a household alters its viewing behavior specifically in response to a focal brand ad—for instance, by changing the channel or ceasing to watch—in a manner that differs from its response to ads of other brands. Crucially, merely skipping the focal brand's ad does not constitute a violation if such behavior is consistent with the household's general ad-skipping patterns.

Empirically, this assumption can be assessed through a falsification check by comparing ad-skipping rates for focal versus non-focal brands. In Section 4.3, we provide evidence demonstrating that both the quasi-random ordering and non-strategic viewership assumptions pass their respective falsification checks in our empirical application.

3.3. Proposed Instrumental Variable

We now formalize our proposed instrument based on the DGP and the identifying assumptions outlined in Section 3.2. To facilitate exposition, we begin by introducing the notation for a key construct, P_{is}.

Let $P_{is} \in [0,1]$ denote the probability that a focal brand's ad, if aired within show s, would be aired at a time T_s^b that happens to fall within $View_{is}$. Mathematically, this is given by:

$$P_{is} \equiv \Pr\left(T_s^b \in View_{is} \mid View_{is}\right) = \int_{View_{is}} l_s(x)\, dx \tag{6}$$

where $l_s(\cdot)$ is the PDF governing the within-show ad airing time assignment, which, in practice, can be inferred from the within-show ad airing time distribution observed for the network broadcasting show *s*.

Equation (6) implies that $P_{is} = 0$ when household *i* does not watch any part of show *s* (i.e., $View_{is} = \emptyset$), and $P_{is} = 1$ when the household watches the entire show (i.e., $View_{is} = [0,1]$). When the household watches the show partially (i.e., $View_{is} \subset [0,1]$), we have $0 < P_{is} < 1$.

Equation (6) also offers an intuitive interpretation of P_{is}: it can be viewed as a weighted viewing duration, where the weights are determined by the PDF $l_s(\cdot)$. When $l_s(\cdot)$ follows a uniform distribution over $[0,1]$, P_{is} equates to the normalized viewing duration of household *i* for show s, denoted by V_{is}. More generally, P_{is} is expected to be positively correlated with V_{is}.

Finally, for a show targeted by the focal brand, the realized value of $I(T_s^b \in View_{is})$, as first defined in Equation (5), is effectively a Bernoulli draw with expected value $E\big(I(T_s^b \in View_{is})\big) = P_{is}$, i.e., $I\big(T_s^b \in View_{is}\big) \sim \text{Bernoulli}(P_{is})$.

Identification Strategy. Our approach to causal inference hinges on decomposing the observed household focal brand ad exposure A_{is} into two components: an endogenous part potentially correlated with the confounder u_{is}, and an exogenous part that is not. To achieve this, we leverage P_{is}, as defined in Equation (6), and re-express A_{is} from Equation (5) as follows:

$$\begin{aligned} A_{is} &= A_s^b \times I\big(T_s^b \in View_{is}\big) = A_s^b \times \big(P_{is} + I\big(T_s^b \in View_{is}\big) - P_{is}\big) \\ &= \underbrace{A_s^b P_{is}}_{expected\ treatment} + \underbrace{A_s^b\big(I(T_s^b \in View_{is}) - P_{is}\big)}_{proposed\ instrument} \end{aligned} \tag{7}$$

In Equation (7), the term $A_s^b P_{is}$ combines two endogenous determinants of A_{is}: the focal brand's show targeting decision A_s^b, and the household's show viewing behavior $View_{is}$, which—through Equation (6)—determines P_{is}, the household's probability of focal brand ad exposure within show *s* if the show is targeted. $A_s^b P_{is}$ thus represents the expected treatment of household *i* during show *s*, conditional on A_s^b, $View_{is}$, and $l_s(\cdot)$, the within-show ad airing time assignment process. The realized treatment A_{is} deviates from $A_s^b P_{is}$ depending on the realized T_s^b, the actual within-show focal brand ad airing time.

As we will elaborate, provided the identifying assumptions hold, the deviation of the realized treatment from the expected treatment, i.e., $A_{is} - A_s^b P_{is}$, is correlated with A_{is} and uncorrelated with the confounder u_{is}, and therefore can serve as an instrument for A_{is}.

To further simplify notation, let $\Delta_{is} \equiv I(T_s^b \in View_{is}) - P_{is}$, and recall the definition of P_{is} in Equation (6). Our proposed instrument in Equation (7) can be re-expressed as:

$$IV_{is} \equiv A_{is} - A_s^b P_{is} = A_s^b \Delta_{is} = \begin{cases} 0, if\ A_s^b = 0, or\ A_s^b = 1\ and\ View_{is} \in \{\emptyset, [0,1]\} \\ I\big(T_s^b \in View_{is}\big) - \int_{View_{is}} l_s(x)\,dx \neq 0, otherwise \end{cases} \tag{8}$$

Before formally presenting the proposition that establishes the validity of IV_{is} (Proposition 1), we highlight the core idea underlying our identification strategy. For targeted shows, Δ_{is}—the deviation between realized and expected within-show focal brand ad exposure—is driven by the quasi-random ordering mechanism employed by linear TV networks in allocating within-show ad slots across advertisers. Intuitively, this source of stochasticity in the ad airing time assignment process can be viewed as natural experiments conducted by linear TV networks during the broadcast of targeted shows. We therefore refer to Δ_{is} as the "network-induced within-show ad exposure shifter," or simply, the "network-induced shifter." As we will demonstrate, this shifter constitutes an exogenous shock that is orthogonal to the demand shock.

Properties of the Proposed IV. Two properties implied by Equation (8) are worth emphasizing.

First, *Nonzero IV under Partial Targeted Show Viewership Only*. Note that $IV_{is} = 0$ when a household watches a show not targeted by the focal brand ($A_s^b = 0$), or when a household watches either 0% of a targeted show ($A_s^b = 1$ and $View_{is} = \emptyset$, thus $\Delta_{is} = 0 - 0 = 0$) or 100% ($A_s^b = 1$ and $View_{is} = [0,1]$, thus $\Delta_{is} = 1 - 1 = 0$). This implies that $IV_{is} \neq 0$ only when a household watches a targeted show partially ($A_s^b = 1$ and $View_{is} \subset [0,1]$), thereby allowing the network to introduce a nonzero shifter between realized and expected focal brand ad exposure (i.e., $\Delta_{is} \neq 0$).

In turn, this property implies that for IV_{is} to serve as an effective instrument in empirical applications, there must be a sufficient number of incidences where $A_s^b = 1$ and $View_{is} \subset [0,1]$ to ensure adequate statistical power for identification. Moreover, the fact that $IV_{is} \neq 0$ only when a household watches a targeted show partially also suggests that, for our identified ad effects to be generalizable to all households, those engaging in partial show viewing (at least occasionally) should exhibit similar ad responsiveness to those who never do.

Second, *Mean Zero for Nonzero IV*. When $A_s^b = 1$ and $View_{is} \subset [0,1]$, $IV_{is} = \Delta_{is} \neq 0$, and the value of Δ_{is} is effectively drawn from a two-point distribution: $\Delta_{is} \sim \begin{cases} 1 - P_{is}, with\ probability\ P_{is} \\ -P_{is}, with\ probability\ 1 - P_{is} \end{cases}$.

This follows directly from $\Delta_{is} \equiv I(T_s^b \in View_{is}) - P_{is}$, and $I(T_s^b \in View_{is}) \sim$ Bernoulli(P_{is}).

The above distributional property of nonzero Δ_{is} implies that nonzero IV_{is} has a mean of zero, because: $E(IV_{is}|IV_{is} \neq 0) = E(\Delta_{is}|A_s^b = 1, View_{is} \subset [0,1]) = (1 - P_{is})P_{is} - P_{is}(1 - P_{is}) = 0$. This also implies that the overall mean of IV_{is}, combining both zero and nonzero values, is zero as well.

This mean-zero property of IV_{is}, which holds under our identifying assumptions, suggests that the proposed instrument can be subjected to a falsification check in empirical applications. If its mean differs significantly from zero, it would warrant caution regarding the validity of the constructed IV.

Validity of the Proposed IV. Formally, Proposition 1 and Corollary 1 establish that IV_{is} can serve as a valid household-show-level instrument for A_{is}, and $IV_{it} = \sum_{s \in S_t} IV_{is}$ can serve as a valid household-day-level instrument for $A_{it} = \sum_{s \in S_t} A_{is}$, provided the identifying assumptions hold. Proofs of Proposition 1 and Corollary 1 are relegated to Online Appendix A for expositional brevity.

Proposition 1: *Under the assumption that the DGP of A_{is} follows Equations (1)–(5), it holds that $corr(IV_{is}, A_{is}) > 0$ and $corr(IV_{is}, u_{is}) = 0$, thereby satisfying the relevance condition and the exclusion restriction, respectively, for IV_{is} to be a valid instrument for A_{is}.*

Corollary 1: *Under the assumption that the DGP of A_{is} follows Equations (1)–(5), it holds that* $corr(IV_{it}, A_{it}) > 0$ and $corr(IV_{it}, u_{it}) = 0$*, thereby satisfying the relevance condition and the exclusion restriction, respectively, for IV_{it} to be a valid instrument for A_{it}.*

Because u_{is} is by definition unobservable to analysts, we cannot directly test whether the exogeneity condition $corr(IV_{is}, u_{is}) = 0$ holds in an empirical application. However, we can derive falsification checks based on observable quantities: $corr(\widehat{IV}_{is}, A_s^b \hat{P}_{is})$, $corr_{(i,s)}(\widehat{IV}_{is}, A_s^b)$, and $corr(\widehat{IV}_{is}, \hat{P}_{is})$, where $\hat{P}_{is}$ is an estimate of P_{is}, and $\widehat{IV}_{is} = A_{is} - A_s^b \hat{P}_{is}$.[7]

Conceptually, if $\widehat{IV}_{is}$ is truly orthogonal to the confounder u_{is}—as claimed in Proposition 1—then it should exhibit no correlation with any endogenous components of the DGP. These include: (1) the focal brand's show targeting decision A_s^b; (2) household i's probability of within-show focal brand ad exposure,

[7] In Section 4.3, we demonstrate how $\hat{P}_{is}$ can be obtained nonparametrically and present descriptive statistics of the proposed instruments in our empirical setting, along with supporting evidence of their validity.

P_{is}, which is a function of the household's show viewership $View_{is}$; and (3) household i's expected treatment, $A_s^b P_{is}$, which incorporates endogenous variation from both (1) and (2). Significant correlation with any of these three variables would challenge the exogeneity of $\widehat{IV}_{is}$, thereby warranting careful re-evaluation of the instrument's validity. Formally, we have:

Proposition 2: *Under the assumption that the DGP of A_{is} follows Equations (1)–(5), it holds that* $corr_{(i,s)}\left(IV_{is}, A_s^b\right) = 0$, $corr(IV_{is}, P_{is}) = 0$, and $corr\left(IV_{is}, A_s^b P_{is}\right) = 0$.

The proof of Proposition 2 is relegated to Online Appendix A for expositional brevity. For both Propositions 1 and 2 to hold, the core requirement is that, for targeted shows ($A_s^b = 1$) and partial household show-viewing ($0 < P_{is} < 1$), the network-induced ad exposure shifter Δ_{is} follows a two-point distribution with an expected value of zero: $\Pr[\Delta_{is} = 1 - P_{is}] = P_{is}$, and $\Pr[\Delta_{is} = -P_{is}] = 1 - P_{is}$.

As long as this distributional property of Δ_{is} holds, IV_{is} satisfies the exogeneity condition and is uncorrelated with the endogenous determinants of A_{is}, including A_s^b, P_{is}, and their product $A_s^b P_{is}$.

3.4. Illustrating the Proposed IV with a Stylized Numerical Example

Unlike typical IVs that are directly observable, our household-show-level IV is constructed indirectly from observables by subtracting a household's expected treatment $A_s^b P_{is}$ from its realized treatment A_{is}. In empirical applications, P_{is} is estimated nonparametrically, conditional on the observed $View_{is}$ and $l_s(\cdot)$, which can be approximated using the empirical distribution of within-show ad airing times.

Given our novel approach to IV construction, i.e., $\widehat{IV}_{is} = A_{is} - A_s^b \hat{P}_{is}$, we present a stylized numerical example in which the DGP is known and satisfies the identifying assumptions, before turning to a more complex real-world application, for three reasons. First, it provides a concrete demonstration of how the IV can be constructed from observables. Second, because the identifying assumptions hold in our numerical example, we can verify whether the constructed IV satisfies both the relevance condition and the exclusion restriction. Third, knowing the ground truth about the ad effect allows us to assess the IV's effectiveness in correcting for targeting and activity biases by comparing ad effect estimates with and without the IV. In short, the stylized numerical example serves as a pedagogical proof of concept, clarifying intuition and building confidence in the core of our identification strategy.

3.4.1. Data-Generating Process of the Numerical Example

Consider an inventory of *M* = 1,000 linear TV shows, each with *N* = 100 households in its potential audience, where 0 and 1 denote the start and end of the show, respectively. For ease of exposition, assume there is no overlap in potential audiences across shows; that is, each household belongs to the potential audience of one and only one show. The DGP for our numerical example is specified as follows.

Observables. As analysts, we observe the following from the DGP: for each show *s*, focal brand *b*'s targeting decision A_s^b; for each targeted show ($A_s^b = 1$), the within-show focal brand ad airing time $T_s^b \in [0,1]$, as well as the mechanism by which it is determined by the network; and for each household *i*, its show viewership duration $V_{is} \in [0,1]$, the segment(s) of the show it watches $View_{is} \subseteq [0,1]$, its focal brand ad exposure status A_{is}, and its purchase decision Y_{is}.

Purchase Decision ($\boldsymbol{Y_{is}}$). The purchase decision Y_{is} is a function of ad exposure A_{is}, a demand shock u_{is}, and an idiosyncratic term e_{is}, specified as:

$$Y_{is} = f(\beta A_{is} + u_{is} + e_{is}) \tag{9}$$

where $f(\cdot)$ is the link function, and e_{is} is drawn i.i.d. from the standard normal distribution.

The link function takes one of two forms: $f^L(x) = x$ (a linear model), or $f^P(x) = I(x > 0)$ (a probit model). The true causal effect of ad exposure on purchase is set at $\beta = 0.5$.

The demand shock u_{is}, unobserved to the analyst, is drawn i.i.d. from a uniform distribution:

$$u_{is} \sim \text{Uniform}[-0.5 + u_s, 0.5 + u_s] \tag{10.1}$$

$$u_s \sim \text{Uniform}[-0.5, 0.5] \tag{10.2}$$

where u_s represents the average demand shock at the show level, itself drawn from a uniform distribution. This implies that $u_{is} \in [-0.5 + u_s, 0.5 + u_s] \subset [-1,1]$.

Show Targeting Decision ($\boldsymbol{A_s^b}$). The focal brand's show-targeting decision A_s^b is drawn from a Bernoulli distribution defined as:

$$\Pr\left(A_s^b = 1\right) = \frac{1+u_s}{2};\ \Pr\left(A_s^b = 0\right) = \frac{1-u_s}{2} \tag{11}$$

This specification captures scenarios in which TV advertisers are more likely to place ad buys in shows whose audiences, on average, exhibit higher baseline demand, thereby introducing targeting bias.

Show Viewing Decision (V_{is} and $View_{is}$). Household i in the potential audience of show s decides both the viewing duration $V_{is} \in [0, 1]$ and the segment(s) of the show to watch $View_{is} \subseteq [0, 1]$, if any. The viewing duration V_{is} is determined as follows:

$$V_{is} = \begin{cases} 0, & if\ V_{is}^* \leq 0 \\ V_{is}^*, & if\ 0 < V_{is}^* < 1 \\ 1, & if\ V_{is}^* \geq 1 \end{cases} \tag{12.1}$$

$$V_{is}^* \sim \text{Uniform}[u_{is}, u_{is} + 1] \tag{12.2}$$

where $V_{is}^* \in [u_{is}, u_{is} + 1] \subset [-1, 2]$ denotes a latent variable positively correlated with the household demand shock u_{is}. This specification ensures that $V_{is} \in [0, 1]$, with point masses at 0 and 1 corresponding to households that do not watch the show at all and those that watch it in full, respectively. Because V_{is}^* is positively correlated with both u_{is} and V_{is}, Equation (12) captures scenarios in which households with higher baseline demand also tend to watch more TV, thereby introducing activity bias.

Given the realized viewing duration V_{is}, the specific viewing segment(s) $View_{is}$ are randomly drawn from the show's duration, subject to the constraint that their total duration equals V_{is}.

Within-Show Ad Airing Timing (T_s^b). For each targeted show, its network determines the focal brand's ad airing time T_s^b, which is drawn uniformly over the show's duration: $T_s^b \sim \text{Uniform}[0, 1]$.

Focal Brand Ad Exposure (A_{is}). The focal brand ad exposure status of household i during show s is jointly determined by A_s^b, $View_{is}$, and T_s^b: $A_{is} = A_s^b \times I\left(T_s^b \in View_{is}\right)$, as specified in Equation (5).

Given the DGP outlined above, our numerical example comprises 100,000 households in total—that is, 1,000 shows, each with 100 households in its potential audience. Key descriptive statistics of the data generated under the DGP are as follows.

50.5% of shows are targeted by the focal brand ($A_s^b = 1$). The correlation between a household's demand shock u_{is} and whether its show is targeted is 0.41, indicating a strong targeting bias. On average, among the potential audience of a targeted show, 16.9% do not watch it at all ($V_{is} = 0$), 16.4% watch the show in full ($V_{is} = 1$), and the remaining 66.7% watch only part of it ($0 < V_{is} < 1$). The correlation between a household's demand shock u_{is} and its show viewing duration V_{is} is 0.78, reflecting a strong activity bias. Overall, 31.2% of households are exposed to a focal brand ad ($A_{is} = 1$). Among exposed households, 39.9% watch the targeted show in its entirety ($V_{is} = 1 | A_{is} = 1$), while 60.1% view only part of it. Among those exposed, 77.8% make a purchase in the probit model ($Y_{is} = 1 | A_{is} = 1$), compared to a purchase rate of 44.7% among unexposed households ($Y_{is} = 1 | A_{is} = 0$).

Given the above, our objective is to recover the true causal effect of A_{is} on Y_{is} (i.e., $\beta = 0.5$ in Equation 9). We next illustrate how our proposed IV can be constructed from the observables.

3.4.2. Constructing and Validating the Proposed IV in the Numerical Example

Recall that $P_{is} \equiv \Pr\left(T_s^b \in View_{is} \mid View_{is}\right) = \int_{View_{is}} l_s(x)\, dx$ denotes the probability that household *i* is exposed to a focal brand ad during show *s* in the event the show is targeted. It is straightforward to verify that when the PDF $l_s(\cdot)$ is $\text{Uniform}[0, 1]$, as in our numerical example, $P_{is} = V_{is}$, the household's viewing duration of show *s*. Based on Equation (8), we construct IV_{is} as follows:

$$\widehat{IV}_{is} = A_{is} - A_s^b \hat{P}_{is} = A_{is} - A_s^b V_{is} \tag{13}$$

As discussed in Section 3.3, under the identifying assumptions, the distributional properties of IV_{is} give rise to several falsification checks on its validity. Specifically, $\widehat{IV}_{is}$ should exhibit zero mean and be uncorrelated with (a) the show targeting decision A_s^b; (b) P_{is}, which equals the household's show viewing duration V_{is} when the within-show ad airing time distribution is $\text{Uniform}[0, 1]$, as in our example; and (c) $A_s^b P_{is}$, which equals $A_{is}^b V_{is}$ in our example.

Based on our data, the mean of $\widehat{IV}_{is}$ is 0.001, $corr_{(i,s)}\left(\widehat{IV}_{is}, A_s^b\right) = 0.005$, $corr\left(\widehat{IV}_{is}, V_{is}\right) = -0.003$, and $corr\left(\widehat{IV}_{is}, A_{is}^b V_{is}\right) = -0.0003$, indicating that $\widehat{IV}_{is}$ passes all the falsification checks, consistent with Proposition 2. Furthermore, because the demand shock u_{is} is known in our example, we can directly verify whether $\widehat{IV}_{is}$ satisfies both the relevance condition and the exclusion restriction. It does: $corr\left(\widehat{IV}_{is}, A_{is}\right) = 0.53$ and $corr\left(\widehat{IV}_{is}, u_{is}\right) = 0.001$, consistent with Proposition 1.

3.4.3. Estimating Ad Effect with the Proposed IV in the Numerical Example

Equipped with $\widehat{IV}_{is}$, we estimate the linear model (where $f = f^L$ in Equation 9) using both the 2SLS and CF approaches. The probit model (where $f = f^P$ in Equation 9) is estimated using only the CF approach, as 2SLS is not appropriate for discrete choice models. In the CF approach, A_{is} is first regressed on $\widehat{IV}_{is}$, and the resulting residual, denoted by CF_{is}, is then included as a control variable in the second-stage estimation of either the linear or probit model.

The results in Table 3 indicate that, with $\widehat{IV}_{is}$, the true causal effect of A_{is} on Y_{is} can be recovered with high confidence for both the linear and probit models. In contrast, without the instrument, the estimated effects are substantially overstated due to the presence of strong targeting bias ($corr(A_{is}^b, u_{is}) = 0.41$) and activity bias ($corr(V_{is}, u_{is}) = 0.78$).

Table 3. Estimation Results from the Numerical Example

	True Value	Linear Model			Probit Model	
		OLS	2SLS	CF	Probit	Probit + CF
Intercept	0	-0.149***	-0.007	-0.008	-0.134***	-0.005
		(0.004)	(0.006)	(0.006)	(0.005)	(0.006)
Ad Effect (β)	0.5	0.957***	0.502***	0.502***	0.899***	0.492***
		(0.007)	(0.014)	(0.014)	(0.009)	(0.017)
Control Function (CF)				0.631***		0.577***
				(0.016)		(0.020)

Notes. Standard errors are reported in parentheses. Standard errors for "Probit + CF" are derived from 1,000 bootstrapped samples. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

4. Empirical Application

We implement the proposed identification strategy in a household-day-level response model using data from a leading U.S. food delivery platform. Sections 4.1 describes the dataset, Section 4.2 outlines the ad response model, and Sections 4.3–4.4 detail the construction, validation, and implementation of the IV.

4.1. Data and Model-Free Evidence

Our data come from two sources: TV viewing data provided by LGADS and customer purchase data from the focal brand, a U.S. food delivery platform with a dominant market position at the time of the study. Customers can place orders through the focal brand's mobile app or website, and purchases from both channels are included in our dataset.[8] Each customer in the purchase data and each household in the TV viewing data is identified by a unique, privacy-compliant meshed IP address, which is used to merge the two data sources. The resulting panel dataset tracks 1,401,902 households over 133 days, from November 15, 2020, through March 28, 2021.[9]

LGADS collects TV viewing data through Automatic Content Recognition (ACR) technology from a large, opt-in panel of U.S. smart TV households. These ACR data capture second-by-second exposure to both shows and ads during each household's viewership. Notably, all observed ad airings from the focal brand occurred on linear TV throughout the study period. Accordingly, our study focuses exclusively on linear TV ads, which are targeted at the show level rather than the household level.

During the study period, the focal brand aired approximately 700 linear TV ads per week, with fewer airings during holidays such as Thanksgiving, Christmas, and New Year's. Most ads were placed in sitcoms (12.9%), comedies (7.1%), animated sitcoms (5.6%), reality shows (5.5%), and reality comedies (5.1%), targeting audiences inclined toward these genres. The focal brand ran only national TV ads during this period, featuring various creatives that emphasized the quality of the delivery experience, humorous interactions with celebrities, or collaborations with new restaurant partners.

Table 4 summarizes household TV viewing and purchase behavior in our data. On average, a panel household watched 4.4 hours of TV per day and was exposed to 0.14 ads from the focal brand per day. For each purchase (i.e., a food delivery order via the focal brand's platform), we observe the customer ID, meshed IP address, purchase time, and a binary indicator denoting whether it was the household's first-ever transaction with the focal brand. At the start of the study period, none of the households in our data were existing customers. Over the course of the study, 53,618 households (3.8%)

[8] While we have device IDs for each order, we cannot distinguish between mobile app and web browser orders. The data provider indicated that most orders were placed via mobile apps. Additionally, information on paid membership status is not available in the dataset.

[9] Our data overlap with the COVID-19 pandemic, during which both food delivery demand and TV viewing habits changed, potentially affecting consumer responsiveness to the focal brand's ads. The pandemic led to an increase in online shopping and convenience-seeking behaviors, which may have enhanced ad effectiveness. Conversely, economic challenges such as job losses may have made consumers more cautious, thereby reducing responsiveness to ads for non-essential goods. These factors suggest a caveat in generalizing our findings to the post-pandemic era.

made their first purchase (i.e., converted). Converted households made a total of 126,077 purchases, averaging 2.4 purchases per household, with an average interpurchase time of 11.3 days.[10]

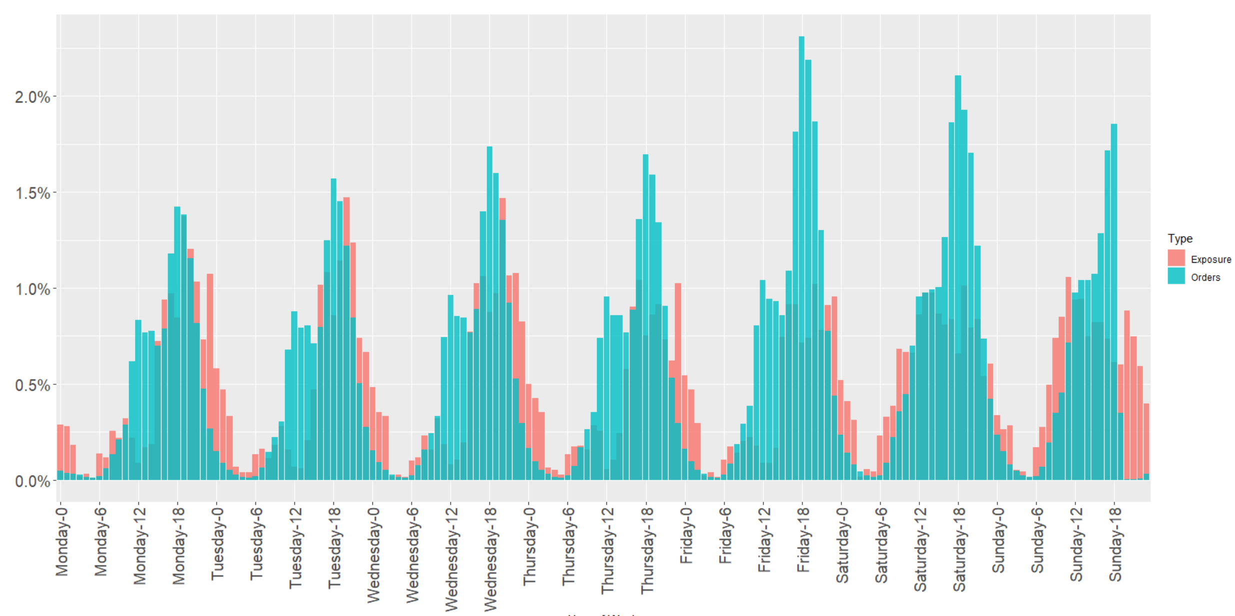
Table 4. Descriptive Statistics

	Mean	Std. Dev.	5%	25%	50%	75%	95%
TV Viewing per Household per Day (Hours)	4.35	5.66	0.00	0.00	1.79	7.04	17.38
Targeted Show Viewing per Household per Day (Hours)	0.11	0.49	0.00	0.00	0.00	0.00	0.66
Number of Focal Brand Ad Exposures per Household per Day	0.14	0.56	0.00	0.00	0.00	0.00	1.00
Purchase Frequency per Converted Household	2.35	3.43	1.00	1.00	1.00	2.00	8.00
Interpurchase Time per Converted Household (Days)	11.26	15.22	1.00	2.00	6.00	14.00	43.00

Notes. TV viewing and purchase behavior for 1,401,902 households, November 15, 2020, through March 28, 2021.

Figure 1 presents the distributions of focal brand purchases and TV ad exposures across the hours of the week. Purchases follow a bimodal daily pattern, peaking around lunch (11:00–13:00) and dinner (17:00–19:00), with higher activity on Fridays and weekends. While the focal brand's TV ad exposures are more evenly distributed throughout the day, their timing broadly aligns with peak purchase periods.

Figure 1. Distribution of Focal Brand Purchases and TV Ad Exposures Across Hours of the Week



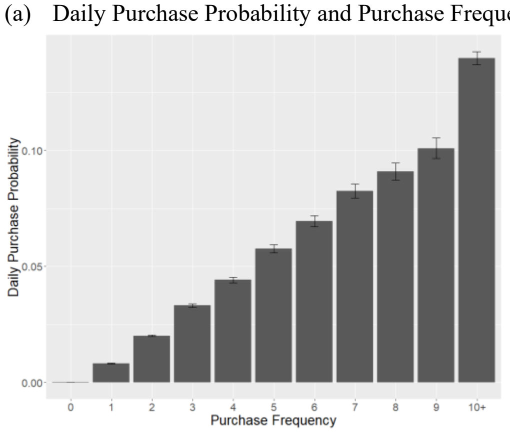
Notes. The distributions of focal brand purchases (blue) and TV ad exposures (red) across the hours of the week.

[10] Among households that received at least one focal brand ad exposure during the study period, over 90% of those that converted made their first purchase after their first exposure to a focal brand ad.

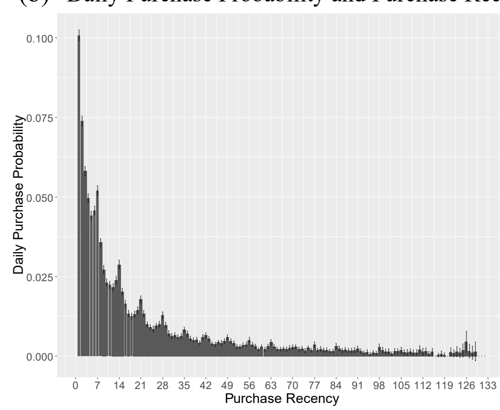
Figure 2 plots the relationships between daily purchase probability and past purchase frequency, recency, and same-day focal brand ad exposures. Figure 2(a) shows that a household's daily purchase probability increases with the number of prior purchases. Figure 2(b) illustrates that daily purchase probability is negatively associated with recency (i.e., number of days since the last purchase). Additionally, we observe regular spikes in daily purchase probability when recency values correspond to multiples of 7 (e.g., 7 and 14), suggesting that households tend to repeat purchases on the same day of the week as their last purchase. Figure 2(c) reveals a slightly positive relationship between same-day TV ad exposures and purchase probability, suggesting a potential positive treatment effect. Together, these model-free patterns motivate our modeling choices described next.

Figure 2. Model-Free Relationship Between Daily Purchase Probability and Past Purchase Frequency, Recency, and Same-Day Ad Exposures

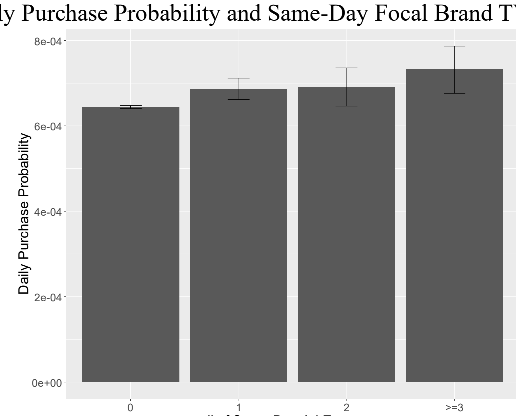
(a) Daily Purchase Probability and Purchase Frequency



(b) Daily Purchase Probability and Purchase Recency



(c) Daily Purchase Probability and Same-Day Focal Brand TV Ad Exposures



Notes. The Y-axis represents the average purchase probability on a given day, with a 95% confidence interval, conditional on a household's prior purchase frequency, recency, or same-day ad exposures.

4.2. Ad Response Model

Building on the data described previously, we specify a household daily ad response model to quantify the effect of linear TV advertising on focal brand purchase. Let Y_{it} denote whether household *i* makes a purchase on day *t* ($Y_{it} = 1$) or not ($Y_{it} = 0$). The purchase probability is determined by the utility U_{it} that household *i* derives from making a purchase from the focal brand on day *t*:

$$U_{it} = \alpha_{it} + \beta_{it} AS_{it} + \gamma X_{it} + u_{it} \tag{14}$$

$$Y_{it} = I(U_{it} > 0) \tag{15}$$

where AS_{it} is the ad stock that incorporates household *i*'s same-day and discounted past exposures to focal brand ads. X_{it} is a set of control variables, including fixed effects for month, day of the week, and holidays (covering both the day before and the day of Thanksgiving, Christmas, and New Year's), an indicator for the first-month post-conversion promotion, major competitors' TV ad spend,[11] and a set of dummies defined as the remainder of purchase recency divided by 7 to account for the observed weekly spikes in daily purchase probability shown in Figure 2(b). u_{it} is an unobserved (to the analyst) demand shock that may be correlated with $AS_{it,}$ and $I(\cdot)$ denotes an indicator function.

We further specify the ad stock AS_{it} as follows:

$$AS_{it} = \sum_{l=0}^{t-1} (\lambda_A)^l A_{i,t-l} \tag{16}$$

where A_{it} is the number of focal brand TV ad exposures household *i* receives on day *t*, and $\lambda_A \in (0,1)$ is a daily decay parameter that captures advertising carryover effects over time.[12]

In Equation (14), α_{it} and β_{it} are household-specific and time-varying, capturing household *i*'s baseline purchase propensity and ad responsiveness on day *t*. We characterize α_{it} and β_{it} as follows:

$$\alpha_{it} = \alpha_0 + \alpha_1 Z_i + I(Freq_{it} \geq 1) \times f(Freq_{it}, Rec_{it}, \alpha_2) + \omega_i^\alpha \tag{17}$$

[11] We account for the top two competitors' TV advertising by acquiring DMA-day-level TV ad spend data from Kantar and calculating ad spend per capita for each DMA-day as a control variable in the ad response model.
[12] To flexibly capture nonlinear ad response while preserving the linear ad stock term necessary for our control function correction, one may specify the ad exposure variable using piecewise linear regressors. This approach allows the marginal effect of ad exposure to vary across predefined intervals while maintaining compatibility with our IV identification strategy.

$$\beta_{it} = \beta_0 + \beta_1 Z_i + I(Freq_{it} \geq 1) \times f(Freq_{it}, Rec_{it}, \beta_2) + \omega_i^{\beta} \tag{18}$$

$$f(Freq_{it}, Rec_{it}, \alpha_2) = \alpha_{20} + \alpha_{21}\log(Freq_{it}) + \alpha_{22}(\log(Freq_{it}))^2 + \alpha_{23}\log(Rec_{it}) + \alpha_{24}(\log(Rec_{it}))^2 \tag{19}$$

$$f(Freq_{it}, Rec_{it}, \beta_2) = \beta_{20} + \beta_{21}\log(Freq_{it}) + \beta_{22}(\log(Freq_{it}))^2 + \beta_{23}\log(Rec_{it}) + \beta_{24}(\log(Rec_{it}))^2 \tag{20}$$

We allow α_{it} and β_{it} to vary by Z_i, a vector of observed, time-invariant household characteristics. Z_i includes average TV viewing time, focal brand ad and targeted show completion rates,[13] and the allocation of TV viewing time across show genres (e.g., sports, reality, news) and dayparts (e.g., daytime, prime time, weekends).[14] All elements of Z_i are calibrated using data from the month prior to the study period and are standardized to have a mean of zero and a standard deviation of one.

To capture unobserved, time-invariant heterogeneity in household baseline purchase propensity and ad responsiveness, ω_i^{α} and ω_i^{β} are assumed to follow a bivariate normal distribution with correlation ρ, such that: $\begin{pmatrix}\omega_i^{\alpha}\\ \omega_i^{\beta}\end{pmatrix} \sim \text{MVN}\left(\begin{matrix}0\\0\end{matrix}, \begin{bmatrix}\sigma_1^2 & \rho\sigma_1\sigma_2\\ \rho\sigma_1\sigma_2 & \sigma_2^2\end{bmatrix}\right)$.

In Equations (17) and (18), $Freq_{it}$ denotes purchase frequency (i.e., number of past purchases) and Rec_{it} denotes purchase recency (i.e., number of days since the last purchase).[15] The indicator function $I(Freq_{it} \geq 1)$ equals one if household *i* made at least one purchase before day *t*, allowing baseline purchase propensity and ad responsiveness to differ between prospective and existing customers. For converted households (i.e., $Freq_{it} \geq 1$), we allow α_{it} and β_{it} to evolve as functions of purchase

[13] Ad completion rate is defined as the total duration of the focal brand's ads watched by a household divided by the total length of all focal brand ads the same household encountered (with at least one second of exposure). Targeted show completion rate is defined as the total duration of targeted shows watched by a household divided by the total length of all targeted shows the same household encountered (with at least one minute of exposure).
[14] We do not have access to demographic variables such as age and gender due to our data provider's data-sharing policy. Instead, we use households' show-viewing behavior from one month prior to the study period as a proxy to capture time-invariant household characteristics.
[15] In the CRM literature, in addition to recency (R) and frequency (F), total monetary value (M) is often found to be predictive of an existing customer's future purchase behavior. However, given that frequency and total monetary value are highly correlated in our data, we focus solely on recency and frequency in this study.

frequency and recency, represented by $f(Freq_{it}, Rec_{it}, \alpha_2)$ and $f(Freq_{it}, Rec_{it}, \beta_2)$, respectively. The log- and squared-log terms in the function $f(\cdot)$ allow baseline purchase propensity and ad responsiveness to vary nonlinearly with purchase frequency and recency.[16]

4.3. Operationalization and Validation of the Proposed IV

4.3.1. Falsification Checks on Identifying Assumptions

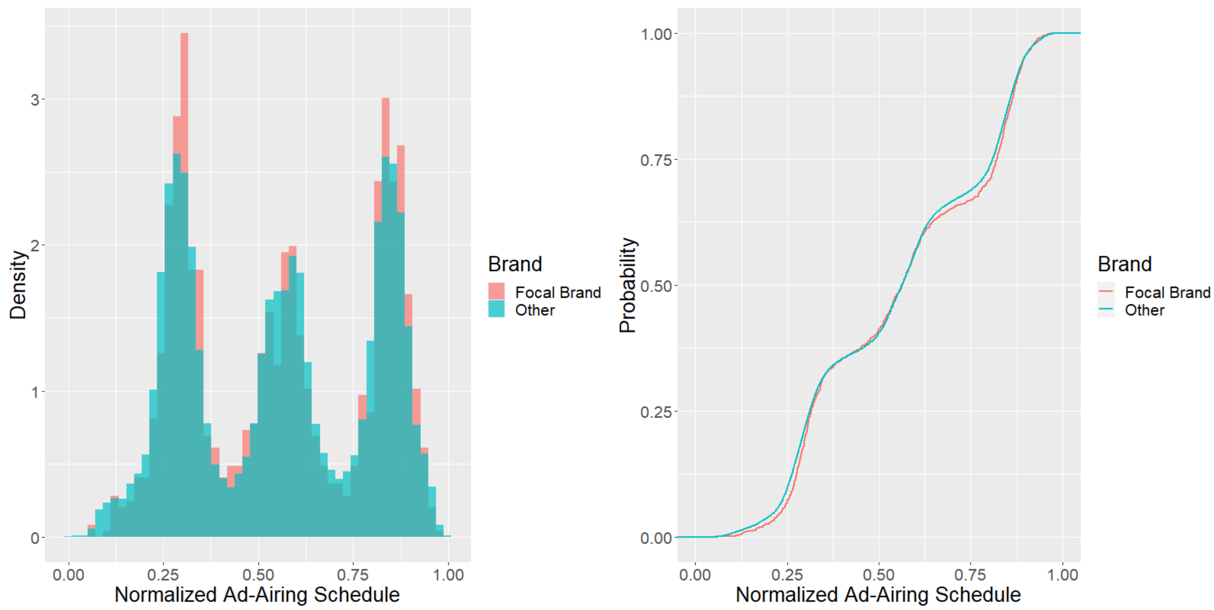
The validity of our proposed instrument relies on two identifying assumptions outlined in Section 3.2. The first assumption is that the exact time at which a focal brand ad is aired within a targeted show is quasi-randomly assigned by the network broadcasting the show. This aligns with current industry practices: in the U.S., networks typically sell linear TV ad slots based on the show and airing date, schedule these slots within a show approximately 7 to 10 days in advance (Bollapragada and Garbiras 2004), and implement an equitable rotation of advertisers' ads across ad slots within a show to ensure fairness (Wilbur et al. 2013, McGranaghan et al. 2022).

In our empirical application, the quasi-random ordering assumption can be assessed through a falsification check by comparing within-show ad airing times for the focal brand with those of non-focal brands across targeted networks. As an illustration, Figure 3 presents the distributions of focal versus non-focal brand ad airing times across shows on MTV, a major network targeted by the focal brand.

Indeed, the within-show airing times for focal and non-focal brand ads exhibit similar distributions in both the PDFs and cumulative density functions (CDFs). MTV appears to schedule both focal and non-focal brand ads according to a tri-modal distribution. The Kolmogorov-Smirnov test confirms that the two distributions are statistically indistinguishable (p = 0.545), suggesting that MTV scheduled focal brand ad airings similarly to other ad airings. In Online Appendix B, we extend this analysis to all major networks and continue to find no significant differences in the timing distributions between focal and non-focal brand ads, indicating that the threat of a violation of the quasi-random ordering assumption is minimal in our setting for all practical purposes.

[16] We compare four specifications in Online Appendix C.1: (1) linear, (2) linear + squared, (3) log, and (4) log + squared log. The log + squared-log specification outperforms the other three based on log-likelihood, AIC, and BIC.

Figure 3. PDFs and CDFs of Within-Show Ad Airing Time Distributions on MTV



Notes. Within-show ad airing times are normalized by show duration, with zero representing the start and one representing the end of a show.

The second identifying assumption, non-strategic viewership, requires that a household's viewership of a targeted show is independent of the within-show airing time of the focal brand ad. This assumption implies that households do not skip or watch the focal brand ad in a systematically different manner than they do ads of other brands.

At first glance, this may appear to be a relatively strong assumption, as households might strategically watch or skip a focal brand ad when it airs, implying that being treated could influence which segments of the show are viewed. Nevertheless, in our empirical setting, 95.5% of focal brand ad exposures observed in our data were watched in their entirety by treated households.[17] Such a high ad completion rate is not significantly different from that of non-focal brand ads (95.7%, $p = 0.12$). Moreover, focal brand ad completion rates are consistent across households regardless of their focal brand conversion status: 95.5% for converted households and 95.6% for unconverted households ($p = 0.20$).

[17] In our empirical implementation, a household is considered treated if it is exposed to any part of a focal brand ad, regardless of the duration of the exposure.

These high and consistent ad completion rates indicate that the threat of strategic, focal-brand-specific ad-skipping behavior is minimal in our setting for all practical purposes.

Taken together, the evidence supports the plausibility of both identifying assumptions, thereby providing empirical justification for estimating a household's probability of focal brand ad exposure within a targeted show based on its actual show viewership.

4.3.2. Operationalizing the Proposed IV

With both identifying assumptions passing their respective falsification checks, we now operationalize the proposed IV. Recall Equation (8): our household-show-level instrument is given by $IV_{is} = A_{is} - A_s^b P_{is}$. While A_{is} (i.e., whether household *i* is exposed to a focal brand ad within show *s*) and A_s^b (i.e., whether show *s* is targeted by the focal brand) are both directly observable from our data, the construction of IV_{is} hinges on the estimation of P_{is}—the probability that household *i* is exposed to a focal brand ad during show *s* in the event the show is targeted.

Recall Equation (6): $P_{is} = \int_{View_{is}} l_s(x)\, dx$. $View_{is}$ is directly observable from the household TV viewing data. We approximate $l_s(\cdot)$ using the empirical within-show ad airing time distribution of each targeted show's network. Based on this approximation, we compute $\hat{P}_{is}$, our estimate of P_{is} as defined in Equation (6). Additional operationalization details are provided in Online Appendix C.2.

Given $\hat{P}_{is}$, we compute the household-show-level IV as $\widehat{IV}_{is} = A_{is} - A_s^b \hat{P}_{is}$. Since focal brand purchase is observed at the daily level, we aggregate the household-show-level IV across all shows broadcast on day *t* to obtain the household-day-level IV: $\widehat{IV}_{it} = \sum_{s \in S_t} \widehat{IV}_{is}$.

One property of our household-show-level IV, as noted in Section 3.3, is that it is nonzero only when a household partially watches a targeted show. This implies that our identification strategy relies on variation in ad exposure status across observations in which a household watches a targeted show but does not complete it. In our data, among households that watched at least one targeted show during the study period, only 1.8% completed all the targeted shows they watched (i.e., "always completers"), indicating that the remaining 98.2% watched at least one targeted show partially.

Moreover, at the household-show level, only 7.6% of observations correspond to shows completed in full, while 51.1% lasted less than 10% of the show's duration, 33% lasted between 10% and 90%, and the remaining 8.3% lasted between 91% and 99%.

Taken together, the small share of always completers (1.8%) and the large portion of partially completed household-show observations (92.4%) suggest that our identification strategy benefits from ample nonzero, network-induced exogenous shocks, enabling the estimation of the average treatment effect for the general population of smart TV households with sufficient statistical power.[18]

4.3.3. Summary Statistics of Operationalized IV and Falsification Checks

Table 5 presents summary statistics for realized treatment A_{is}, expected treatment $A_s^b \hat{P}_{is}$, and instrument $\widehat{IV}_{is} = A_{is} - A_s^b \hat{P}_{is}$ across all household-show observations in which a household watched any portion of a targeted show (i.e., $A_s^b = 1$ and $View_{is} \neq \emptyset$). Table 5 also reports summary statistics for the corresponding household-day-level aggregates, i.e., $A_{it} = \sum_{s \in S_t} A_{is}$, $\sum_{s \in S_t} A_s^b \hat{P}_{is}$, and $\widehat{IV}_{it} = \sum_{s \in S_t} \widehat{IV}_{is}$.

Table 5. Summary Statistics of Realized Ad Exposure, Expected Ad Exposure, and IV

	Mean	Std. Dev.	5%	25%	50%	75%	95%
Household-show-level							
A_{is}	0.29	0.45	0.00	0.00	0.00	1.00	1.00
$A_s^b \hat{P}_{is}$	0.29	0.38	0.001	0.01	0.06	0.56	1.00
$\widehat{IV}_{is}$	-0.002	0.25	-0.38	-0.04	-0.004	0.00	0.49
Household-day-level							
A_{it}	0.51	0.84	0.00	0.00	0.00	1.00	2.00
$\sum_{s \in S_t} A_s^b \hat{P}_{is}$	0.51	0.77	0.00	0.01	0.15	0.82	2.00
$\widehat{IV}_{it}$	-0.004	0.39	-0.61	-0.11	-0.01	0.00	0.75

Notes. Summary statistics are based on 41,407,672 household-show-level and 23,985,341 household-day-level observations in which households watched any portion of a targeted show. A_{is} represents realized focal brand ad exposure within a targeted show, $A_s^b \hat{P}_{is}$ denotes expected ad exposure, and $\widehat{IV}_{is}$ is the household-show-level IV. A_{it}, $\sum_{s \in S_t} A_s^b \hat{P}_{is}$, and $\widehat{IV}_{it}$ are the corresponding household-day-level aggregates.

At the household-show level, the mean of $A_{is} = 0.29$ closely aligns with the mean of $A_s^b \hat{P}_{is}$, indicating that our estimate of expected treatment is unbiased. This, in turn, yields a household-show-

[18] Our identification strategy may lead to a downward bias in the estimated ad effects if households that consistently watch entire shows are also more ad-responsive.

level $\widehat{IV}_{is}$ with a mean close to zero (-0.002). A similar pattern emerges at the household-day level, where the mean of $A_{it} = 0.51$, the mean of $A_s^b \hat{P}_{is} = 0.51$, and the mean of $\widehat{IV}_{it} = -0.004$. These results indicates that both $\widehat{IV}_{is}$ and $\widehat{IV}_{it}$ pass the mean-zero falsification check, as discussed in Section 3.3.

Conceptually, our estimate of expected treatment, $A_s^b \hat{P}_{is}$, resembles a propensity score. However, unlike traditional propensity score estimation—which typically involves calibrating a predictive model using the realized treatment status (in our case, A_{is}) as the dependent variable—we obtain $A_s^b \hat{P}_{is}$ nonparametrically and without knowledge of A_{is}. This fundamentally different approach to expected treatment or propensity score estimation underscores that achieving equality between the mean of A_{is} and the mean of $A_s^b \hat{P}_{is}$ is nontrivial and not mechanically guaranteed.

Moreover, $corr\left(A_{is}, A_s^b \hat{P}_{is}\right) = 0.89$, indicating that our expected treatment estimate is not only unbiased, but also highly correlated with the realized treatment, as expected. Most notably, $corr\left(\widehat{IV}_{is}, A_{is}\right) = 0.42$, $corr_{(i,s)}\left(\widehat{IV}_{is}, A_s^b\right) = -0.001$, $corr\left(\widehat{IV}_{is}, \hat{P}_{is}\right) = -0.001$, and $corr\left(\widehat{IV}_{is}, A_s^b \hat{P}_{is}\right) = -0.009$. These results indicate that our instrument simultaneously achieves (a) a high correlation with realized treatment—thus satisfying the relevance condition, consistent with Proposition 1—and (b) near-zero correlation with the show-targeting decision (A_s^b), the estimated probability of focal brand ad exposure within a targeted show ($\hat{P}_{is}$), and the expected treatment estimate ($A_s^b \hat{P}_{is}$)—thus passing the falsification checks on exogeneity, consistent with Proposition 2.[19]

Statistically, achieving both (a) and (b) is nontrivial. It requires that two highly correlated variables—A_{is} and $A_s^b \hat{P}_{is}$—yield a difference, $\widehat{IV}_{is} = A_{is} - A_s^b \hat{P}_{is}$, that is highly correlated with one (A_{is}) but uncorrelated with the other ($A_s^b \hat{P}_{is}$).

Similar to the pattern observed at the household-show level, at the household-day level, we find that $corr\left(A_{it}, \sum_{s \in S_t} A_s^b \hat{P}_{is}\right) = 0.92$, $corr\left(\widehat{IV}_{it}, A_{it}\right) = 0.36$, $corr_{(i,t)}\left(\widehat{IV}_{it}, \sum_{s \in S_t} A_s^b\right) = -0.006$, $corr\left(\widehat{IV}_{it}, \sum_{s \in S_t} \hat{P}_{is}\right) = -0.007$, and $corr\left(\widehat{IV}_{it}, \sum_{s \in S_t} A_s^b \hat{P}_{is}\right) = -0.009$. These results indicate that our

[19] In Online Appendix C.3, we compare the correlation in focal brand ad exposures with the correlation in network-induced shifters across targeted shows to provide further support for the validity of the proposed IV.

proposed household-day-level instrument $\widehat{IV}_{it}$ also satisfies the relevance condition and passes the falsification checks on exogeneity.

4.4. Control Function Approach

Because each household's daily purchase decision is modeled as a discrete choice (Equations 14 and 15), we incorporate the proposed instrument into the ad response model using the control function approach (Petrin and Train 2010, Wooldridge 2015, Ebbes et al. 2016). In the first stage of the control function approach, we regress each household's daily focal brand ad exposures A_{it} on its instrument $\widehat{IV}_{it}$ and other variables entering the utility function. Formally, we have:

$$A_{it} = \alpha_{it}^{f} + \gamma^{f} X_{it} + \varphi^{f} \widehat{IV}_{it} + \varepsilon_{it}^{f} \tag{21}$$

$$\alpha_{it}^{f} = \alpha_{0}^{f} + \alpha_{1}^{f} Z_i + I(Freq_{it} \geq 1) \times f(Freq_{it}, Rec_{it}, \alpha_{2}^{f}) + \omega_{i}^{f} \tag{22}$$

where α_{it}^{f} (with superscript "*f*" denoting "first-stage") is a household-specific, time-varying intercept specified analogously to α_{it} in Equation (17), and $\omega_{i}^{f} \sim N(0, \sigma_3^2)$.

We retain the residual from the first-stage regression (Equations 21 and 22), denoted by $\hat{\varepsilon}_{it}^{f}$, and include it in the second stage of the control function approach to correct for potential endogeneity bias in the ad response model. To align with the ad stock formulation of household daily focal brand ad exposures (i.e., $AS_{it} = \sum_{l=0}^{t-1} (\lambda_A)^l A_{i,t-l}$), we adopt a similar specification to construct a "control function stock," denoted by CFS_{it}, which is expressed as:

$$CFS_{it} = \sum_{l=0}^{t-1} (\lambda_{CF})^l \hat{\varepsilon}_{i,t-l}^{f} \tag{23}$$

where $\lambda_{CF} \in (0,1)$ is a decay parameter to be determined empirically.

Taken together, in the second stage of the control function approach, we estimate a probit model with CFS_{it} entering the utility function as an additional control:

$$U_{it} = \alpha_{it} + \beta_{it} AS_{it} + \gamma X_{it} + \delta CFS_{it} + \varepsilon_{it}^{s} \tag{24}$$

where ε_{it}^{s} (with superscript "*s*" denoting "second-stage") is i.i.d. standard normal and is no longer correlated with AS_{it}, because CFS_{it} conditions out the variation in the demand shock u_{it} in Equation (14) that is correlated with AS_{it}. This yields an endogeneity-corrected estimate of β_{it}. A test of the null hypothesis $\delta = 0$ also serves as a test of exogeneity for AS_{it} (Wooldridge 2015, Ebbes et al. 2016).

5. Results

5.1. Evidence of Bias Correction by the Proposed Identification Strategy

The results from the first-stage regression (Equations 21 and 22), reported in Table 6, show that $\widehat{IV}_{it}$ has the expected positive and significant effect on focal brand ad exposure (φ^f = 0.970, p < 0.01). Also, the instrument is unlikely to be weak, as indicated by a univariate F-statistic of 1,902,996 (p < 0.001). All variables related to purchase history—such as frequency, recency, and the indicator for the first month post-conversion promotion—are uncorrelated with focal brand ad exposure once we condition on $\widehat{IV}_{it}$. Although top competitors' ad spend is positively associated with focal ad exposure, the effect size is negligible: a one standard deviation increase in competitors' ad spend corresponds to only 0.008 additional focal brand ad exposures. Finally, we observe substantial unobserved heterogeneity across households, as reflected in the significant standard deviation of the random intercept.

Table 6. Parameter Estimates from the First-Stage Model

Model Component	Parameter	Estimate	SE
Intercept	α_0^f	0.060***	0.001
$\widehat{IV}_{it}$	φ^f	0.970***	0.001
Post Conversion	$\alpha_{2,0}^f$	0.005	0.004
Frequency (log)	$\alpha_{2,1}^f$	0.0003	0.003
Frequency (log) sq.	$\alpha_{2,2}^f$	-0.0003	0.002
Recency (log)	$\alpha_{2,3}^f$	-0.002	0.003
Recency (log) sq.	$\alpha_{2,4}^f$	0.0001	0.001
First Month Post-Conversion Promotion	γ_1^f	-0.001	0.002
Competitor Ad Spend	γ_2^f	0.008***	0.0004
Month FE	-	Yes	
Day-of-Week FE	-	Yes	
Holiday FE	-	Yes	
Household Characteristics	α_1^f	Yes	
Std. Dev. of Random Intercept	σ_3	0.012***	0.001

Notes. The DV is daily household focal brand ad exposures A_{it}. ***p < 0.01; **p < 0.05; *p < 0.1.

The second-stage household-daily ad response model is a random-coefficient probit estimated using simulated maximum likelihood (SML) with Halton draws (Train 1999). To assess how key parameter estimates vary across model specifications, we estimate several simplified versions for comparison. The results are presented in Table 7.

Column 1 presents the simplest specification, which assumes no ad carryover ($\lambda_A = 0$), no control function ($\delta = 0$ and $\lambda_{CF} = 0$), no moderation of ad responsiveness by time-invariant household characteristics ($\beta_1 = 0$) or purchase history ($\beta_2 = 0$), and no unobserved heterogeneity in baseline purchase propensity or ad responsiveness ($\sigma_1 = 0$, $\sigma_2 = 0$, and $\rho = 0$).

Column 2 extends Column 1 by introducing a control function term, allowing δ to be estimated while fixing the decay parameter $\lambda_{CF} = 0$, such that $CFS_{it} = \hat{\varepsilon}_{it}^{f}$. We observe that the ad effect estimate (β_0) decreases from 0.023 ($p < 0.01$) in Column 1 to 0.010 ($p < 0.1$) in Column 2, highlighting the debiasing effect of including the control function term $\hat{\varepsilon}_{it}^{f}$. The positive and significant coefficient for CFS_{it} ($\delta = 0.015$, $p < 0.05$) further confirms that observed household daily ad exposures are endogenous, exhibiting positive spurious correlation with the household's daily purchase decision.

Columns 3 and 4 extend Columns 1 and 2, respectively, by allowing the decay parameters in AS_{it} and CFS_{it} (i.e., λ_A and λ_{CF}) to be empirically determined through a grid search (Danaher et al. 2020, Shapiro et al. 2021, Tsai and Honka 2021). We do not constrain λ_A and λ_{CF} to be equal. Instead, we vary them independently from 0 to 0.99 in increments of 0.05 and select the value combination that maximizes out-of-sample fit.[20] The best-fitting daily decay parameters are $\lambda_A = 0.7$ and $\lambda_{CF} = 0.95$.

The control function term remains positive and significant ($\delta = 0.003$, $p < 0.01$).[21] Comparing the β_0 estimates in Columns 3 and 4, we observe that including the control function term CFS_{it} reduces the estimated ad effect by over 50%, from $\beta_0 = 0.016$ ($p < 0.01$) to $\beta_0 = 0.007$ ($p < 0.05$). Additionally, the significant and positive coefficient of CFS_{it} ($\delta = 0.003$, $p < 0.01$) further supports the presence of endogeneity, consistent with the pattern observed in the comparison between Columns 1 and 2.

[20] For each decay parameter combination, we estimate the ad response model using data from the first 120 days of the study period and evaluate the model's log-likelihood on data from the remaining 13 days.
[21] The correlation between AS_{it} and CFS_{it} is 0.72. To assess potential multicollinearity, we calculate the variance inflation factors (VIFs) for AS_{it} and CFS_{it} in the second-stage model, which are 2.42 and 2.27, respectively—well below the conventional threshold of 10.

Table 7. Second-Stage Estimation Results for the Ad Response Model

		No Carryover or Moderation Effects w/o CF (1)		No Carryover or Moderation Effects w/ CF (2)		No Moderation Effects w/o CF (3)		No Moderation Effects w/ CF (4)		Full Model w/o CF (5)		Full Model w/ CF (6)	
	Parameter	Estimate	SE	Estimate	SE	Estimate	SE	Estimate	SE	Estimate	SE	Estimate	SE
Intercept	α_0	-3.338***	0.005	-3.368***	0.005	-3.370***	0.005	-3.368***	0.006	-3.430***	0.006	-3.429***	0.006
Ad Stock (AS_{it})	β_0	0.023***	0.003	0.010*	0.005	0.016***	0.002	0.007**	0.003	0.018***	0.003	0.012***	0.003
Control Function (CFS_{it})	δ	-	-	0.015**	0.007	-	-	0.003***	0.001	-	-	0.003***	0.001
Post Conversion	$\alpha_{2,0}$	1.611***	0.010	1.611***	0.010	1.611***	0.010	1.611***	0.009	1.499***	0.012	1.498***	0.013
Frequency (log)	$\alpha_{2,1}$	0.516***	0.007	0.516***	0.007	0.516***	0.007	0.516***	0.007	0.459***	0.008	0.459***	0.008
Frequency (log) sq.	$\alpha_{2,2}$	-0.045***	0.003	-0.045***	0.003	-0.045***	0.003	-0.045***	0.003	-0.061***	0.003	-0.061***	0.003
Recency (log)	$\alpha_{2,3}$	-0.120***	0.006	-0.120***	0.006	-0.120***	0.006	-0.120***	0.006	-0.103***	0.008	-0.103***	0.008
Recency (log) sq.	$\alpha_{2,4}$	-0.027***	0.001	-0.027***	0.001	-0.027***	0.001	-0.027***	0.001	-0.028***	0.002	-0.028***	0.002
Adstock × Post Conversion	$\beta_{2,0}$	-	-	-	-	-	-	-	-	0.001	0.013	0.004	0.013
Ad Stock × Freq (log)	$\beta_{2,1}$	-	-	-	-	-	-	-	-	0.017*	0.009	0.019*	0.009
Ad Stock × Freq (log) sq.	$\beta_{2,2}$	-	-	-	-	-	-	-	-	-0.010*	0.005	-0.010*	0.005
Ad Stock × Rec (log)	$\beta_{2,3}$	-	-	-	-	-	-	-	-	-0.011	0.011	-0.011	0.011
Ad Stock × Rec (log) sq.	$\beta_{2,4}$	-	-	-	-	-	-	-	-	0.004**	0.002	0.004**	0.002
First Month Post-Conversion Promotion	γ_1	0.200***	0.005	0.200***	0.006	0.200***	0.005	0.200***	0.005	0.189***	0.006	0.189***	0.006
Competitor Ad Spend	γ_2	0.014***	0.005	0.014***	0.004	0.014***	0.005	0.014***	0.006	0.014**	0.005	0.014***	0.005
Month FE	-	Yes		Yes		Yes		Yes		Yes		Yes	
Day-of-Week FE	-	Yes		Yes		Yes		Yes		Yes		Yes	
Holiday FE	-	Yes		Yes		Yes		Yes		Yes		Yes	
Carryover	λ_A/λ_{CF}	0/0		0/0		0.7/0		0.7/0.95		0.7/0		0.7/0.95	
Household Characteristics	α_1	Yes		Yes		Yes		Yes		Yes		Yes	
HH Char. × Adstock	β_1	-		-		-		-		Yes		Yes	
Std. Dev. (Intercept)	σ_1	-		-		-		-		0.197***	0.004	0.198***	0.005
Std. Dev. (Adstock)	σ_2	-		-		-		-		0.003	0.015	0.015	0.012
Rho	ρ	-		-		-		-		-0.553	0.446	-0.538	0.351

Notes. Columns (1) and (2) assume no carryover of ad effect, no evolution of ad responsiveness, and no unobserved heterogeneity. Columns (3) and (4) assume no evolution of ad responsiveness and no unobserved heterogeneity. Columns (5) and (6) incorporate all model components. Estimates in Columns (2), (4), and (6) are obtained using the control function approach, with standard errors derived from 50 bootstrapped samples. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Finally, Columns 5 and 6 extend Columns 3 and 4 by allowing ad responsiveness to be moderated by time-invariant household characteristics ($\beta_1 \neq 0$) and purchase history ($\beta_2 \neq 0$), as well as by introducing unobserved heterogeneity in both baseline purchase propensity and ad responsiveness ($\sigma_1 \neq 0$, $\sigma_2 \neq 0$, and $\rho \neq 0$). Once again, incorporating CFS_{it} corrects the substantial upward bias in the naïve ad effect estimate, as evidenced by the decline in β_0 from 0.018 ($p < 0.01$) in Column 5 to 0.012 ($p < 0.01$) in Column 6.[22]

The naïve ad effect estimate suggests that an average unconverted household with one focal brand ad exposure is approximately 7.5% more likely to convert than one without exposure. Under the proposed model, this estimated lift decreases to 4.6%. For the remainder of the paper, we focus on the endogeneity-corrected estimates from the full model presented in Column 6 of Table 7.

To contextualize our ad effect estimates within the existing literature, we compute short- (same-day) and long-term (30-day) ad elasticities for an average household in our panel. With endogeneity correction, the short- and long-term elasticities are 0.045 and 0.143, respectively, compared to 0.072 and 0.222 without correction—indicating an overstatement of 55%. These naïve estimates closely align with the mean elasticities of 0.12 (short-term) and 0.24 (long-term) reported by Sethuraman et al. (2011), highlighting the risk of substantially inflated estimates without proper endogeneity correction.

Our endogeneity-corrected long-term elasticity of 0.143 remains notably higher than the 0.023 reported by Shapiro et al. (2021), likely reflecting differences in brand maturity. Shapiro et al. (2021) primarily examined mature brands, for which the literature consistently finds lower ad elasticities (Lodish et al. 1995, Sethuraman et al. 2011).

5.2. Baseline Purchase Propensity and Purchase History

The α_2 estimates in Column 6 of Table 7 capture how a household's baseline purchase propensity changes with its purchase history. There is a significant increase in baseline purchase propensity after a

[22] As a robustness check of the identified positive effect of linear TV advertising, we conduct a placebo test by replacing the focal brand's ad exposures with those from a major automobile manufacturer and re-estimating the proposed model. The null effect of auto ads on household purchases of the focal brand suggests that the identified positive effect of focal brand ads is unlikely to be coincidental (see Online Appendix C.4 for details).

household makes its first purchase ($\alpha_{2,0}$ = 1.498, p < 0.01), indicating a substantial boost in daily purchase probability: an unconverted household has a 0.01% daily probability of making a purchase, whereas a newly converted household has a 1.5% daily probability.

One possible explanation for this increase is the initial setup cost associated with using the focal brand's platform. Before placing the first order, a household must download the app (or access the website), create a user account, and provide a payment method. As these steps are not required for subsequent purchases, the resulting reduction in friction naturally leads to a higher baseline purchase propensity following the first transaction.

Once a household is converted, prior purchase frequency has a positive but diminishing effect on baseline purchase propensity ($\alpha_{2,1}$ = 0.459, p < 0.01; $\alpha_{2,2}$ = −0.061, p < 0.01), suggesting self-reinforcing habit formation, particularly during the early stages of repeat purchasing. In contrast, baseline purchase propensity declines as recency increases ($\alpha_{2,3}$ = −0.103, p < 0.01; $\alpha_{2,4}$ = −0.028, p < 0.01), indicating that households become less likely to repurchase the longer they wait. This pattern aligns with the recency-trap effect documented in other contexts (Neslin et al. 2013).

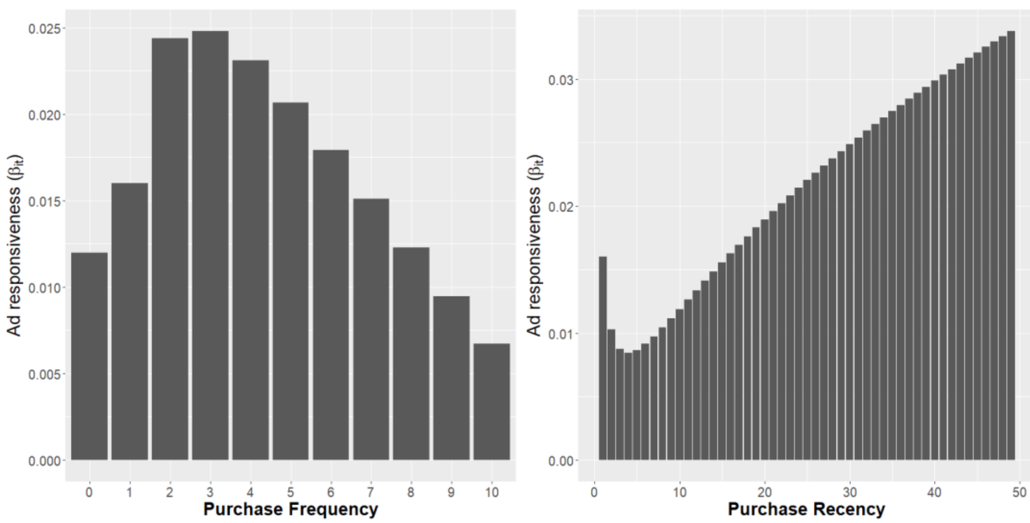
Taken together, our estimates show that baseline purchase propensity continues to evolve with purchase frequency and recency, beyond the initial post-conversion increase. For example, households with one past purchase have a 1.5% daily probability of purchasing, which rises to 4.0% after three purchases and to 5.5% after five. Conversely, a household that purchased yesterday has a 1.5% probability of purchasing today, which declines to 0.9% after one week and to 0.3% after four weeks.

5.3. Ad Responsiveness and Purchase History

We next examine how a household's responsiveness to the focal brand's TV ads varies with its purchase history, as captured by the β_2 estimates in Column 6 of Table 7. Since $\beta_{2,0}$ is statistically insignificant (p > 0.1), there is no evidence that a household becomes either more or less responsive to the focal brand's TV ads immediately following its trial purchase. To better interpret how ad responsiveness evolves with prior purchases, Figure 4 visualizes the nonlinear patterns implied by the β_2 estimates.

The left panel of Figure 4 depicts an inverted U-shaped relationship between ad responsiveness and purchase frequency. Households are most responsive to the focal brand's TV ads after making two to four prior purchases—that is, during the early stages of repeat purchasing. Beyond this peak, ad responsiveness declines as households become more frequent purchasers, reflecting a reduced impact of TV advertising on habitual buyers. In contrast, the right panel of Figure 4 illustrates a U-shaped relationship between ad responsiveness and purchase recency. Households are least responsive to the focal brand's TV ads three to five days after a purchase.

Figure 4. Ad Responsiveness by Purchase Frequency and Recency



Notes. The moderating effects of purchase frequency and recency on ad responsiveness (β_{it}) indicate that frequency exhibits an inverted U-shaped relationship with ad responsiveness, whereas recency follows a U-shaped pattern.

These patterns are also reflected in the short-term ad elasticity estimates. For an average household with one prior purchase, same-day ad elasticity increases from 0.040 to 0.056 after the second purchase but declines to 0.046 after the fifth. Similarly, for a typical household with one prior purchase, same-day ad elasticity decreases from 0.040 to 0.027 one week after the last purchase but rises to 0.078 four weeks later.

While these patterns suggest that the focal brand's TV ad effectiveness varies nonmonotonically with prior purchase frequency and recency, the underlying mechanisms remain unclear. Future research is needed to better understand the factors driving these dynamics. One potential explanation is that, with each additional purchase, the effectiveness of TV ads may shift due to changes in their informational versus emotional roles (Tellis 1988, Deighton et al. 1994, Ackerberg 2001, 2003).

Regardless of the mechanisms, the complex dynamics of baseline purchase propensity and ad responsiveness with respect to purchase frequency and recency offer important managerial implications for behaviorally targeted TV advertising. First, to capture the full impact of a TV ad, it is essential to account for state-dependence effects—such as habit formation acceleration and recency trap avoidance—in addition to same-day and carryover effects. Second, when formulating targeting strategies, advertisers should consider incorporating prior purchase frequency and recency as a basis for segmentation (e.g., prospective vs. existing customers, early repeat vs. habitual purchasers, and recent vs. lapsed purchasers). For instance, the focal brand may benefit from targeting lapsed households—those who have not purchased in a long time—given their substantially higher ad responsiveness. However, because baseline purchase propensity declines sharply with increasing recency, timely intervention is essential to prevent households from falling into a self-reinforcing recency trap. Effective behavioral targeting should therefore balance these opposing forces by accounting for both the decline in baseline purchase propensity and the rise in ad responsiveness as recency increases.

5.4. Household Heterogeneity and Other Control Variables

Table 8 presents the parameter estimates capturing observed household heterogeneity in baseline purchase propensity and ad responsiveness.

Households that watch more TV tend to have lower baseline purchase propensity, potentially because heavy TV viewers are typically older and less engaged with food delivery services. In contrast, households that watch more sports exhibit higher baseline purchase propensities, possibly because sports audiences tend to be younger, a demographic more inclined to use food delivery services. Conversely, heavy news viewers, who skew older, may have more traditional meal preparation habits or a lower propensity to adopt digital food ordering.

We also find that households with higher sports viewership are more responsive to the focal brand's ads. In contrast, heavy TV viewers exhibit lower responsiveness, possibly due to ad saturation—

greater exposure to a wide range of advertisers may lead to ad fatigue and diminished attention to any single advertiser, including the focal brand.

These observed heterogeneities are economically meaningful. For example, a one standard deviation increase in a household's sports viewership raises short-term ad elasticity from 0.045 to 0.061, while a one standard deviation increase in general TV viewing lowers it from 0.045 to 0.030.

Table 8. Parameter Estimates for Observed Household Heterogeneity

	Baseline Purchase Propensity ($\boldsymbol{\alpha_1}$)		Ad Responsiveness ($\boldsymbol{\beta_1}$)	
	Estimate	SE	Estimate	SE
Avg. TV Viewing	-0.019^{***}	0.002	-0.004^{**}	0.002
Sports Viewing	0.014^{***}	0.002	0.004^{*}	0.002
Reality Viewing	-0.003^{*}	0.002	0.003	0.002
News Viewing	-0.012^{***}	0.002	0.002	0.002
Daytime Viewing	-0.006^{**}	0.002	-0.001	0.003
Prime Time Viewing	-0.011^{***}	0.002	0.001	0.003
Weekend Viewing	-0.005^{**}	0.002	-0.0004	0.003
Ad Completion	0.001	0.001	-0.004	0.003
Show Completion	0.003^{**}	0.001	0.003	0.002

Notes. The standard errors are derived from 50 bootstrapped samples. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Beyond observed heterogeneities, Column 6 of Table 7 also indicates substantial unobserved heterogeneity in baseline purchase propensity (σ_1 = 0.156, p < 0.01). However, unobserved heterogeneity in ad responsiveness is not statistically significant, nor is there evidence of a significant correlation between unobserved baseline purchase propensity and ad responsiveness.

Lastly, we examine the effects of two key control variables. The first-month post-conversion promotion, available only to new customers, has a positive and significant effect ($\gamma_1 = 0.189$, p < 0.01), resulting in a 58% increase in daily purchase probability. Competitor ad spend also has a positive and significant effect on focal brand purchase propensity ($\gamma_2 = 0.014$, p < 0.01). The implied same-day competitor ad elasticity is 0.016 for unconverted households, approximately 36% of the focal brand's own same-day ad elasticity. This finding aligns with prior research on TV advertising spillover effects. For example, Anderson and Simester (2013) and Sahni (2016) report positive spillover from competitor ads in field experiments. Du et al. (2019) similarly find that immediate TV ad elasticity of brand search ranges from 0.02 to 0.22 for own-brand ads and from 0.003 to 0.05 for competitor ads. These results are consistent with our finding that competitor TV ads can indirectly benefit the focal brand, potentially because the category expansion effect outweighs the share-stealing effect in a relatively young industry.

6. Concluding Remarks

The widespread adoption of ACR-enabled smart TVs and STBs has made second-by-second TV viewing data available to networks and advertisers at unprecedented scales. This high-granularity, large-scale audience measurement data is emerging as a viable contender for TV ad currency. When merged with first-party CRM data, this modern single-source data has the potential to transform the landscape of TV advertising—not only by enabling improved targeting and attribution in practice, but also by fostering methodological innovation in marketing science.

This research contributes to such methodological innovation by developing a novel IV for estimating the causal effect of linear TV advertising using household-level observational data. Our method addresses a central challenge in ad effectiveness research based on such data: the absence of a generalizable approach to causal inference that is robust to both targeting and activity biases. The key to our method lies in recognizing that, in linear TV—where ad buys are primarily targeted at the show level—networks generally assign within-show ad slots across advertisers in a quasi-random manner. This practice introduces a source of stochasticity in realized ad exposure status among households that watch only part of a targeted show.

Through mathematical proofs, a stylized numerical example, and an empirical application, we show that, provided the identifying assumptions hold, there exists exogenous, network-induced variation in linear TV ad exposure at the household-show level that can be leveraged for causal identification. Our core innovation lies in demonstrating how this exogenous variation can be extracted nonparametrically as the residual between a household's realized ad exposure and its expected treatment, and used as an instrument at either the household-show or household-day level. A notable feature of our approach to IV construction is that the expected treatment is estimated without calibrating a predictive model using the realized treatment as the dependent variable, in contrast to traditional propensity score-based approaches.

For IV-based identification, instrument exogeneity is inherently untestable in real-world applications. That said, a desirable feature of our approach to IV construction is that it enables multiple falsification checks to assess the credibility of both the identifying assumptions and the instrument itself:

- *On the quasi-random ordering assumption:* Examine whether the empirical distribution of within-show ad airing times for the focal brand is statistically indistinguishable from that of non-focal brands. Passing this check increases confidence that networks have assigned the focal brand's within-show ad slots through a quasi-random process comparable to that used for other brands.
- *On the non-strategic viewership assumption*: Examine whether ad-skipping rates for the focal brand are comparable to those for non-focal brands. Passing this check—particularly when ad-skipping rates are generally low (e.g., no greater than 4.5%, as in our data)—increases confidence that viewers do not avoid focal brand ads in a systematically different manner than they do ads of other brands.
- *On the validity of the instrument*: Examine whether the constructed IV (a) has a mean close to zero, thereby ensuring the expected treatment estimate is unbiased, (b) is strongly and positively correlated with the realized treatment, thus satisfying the relevance condition; and (c) is uncorrelated with the expected treatment estimate, thereby passing the falsification check for the exogeneity condition.

With respect to the effectiveness of our identification strategy in practice, we find that, in our empirical application, omitting the proposed IV correction for endogeneity leads to naïve ad elasticity estimates that are overstated by 55%, even after controlling for a large number of covariates and incorporating random effects. Such substantial bias correction underscores the value of our method for credible causal inference.

Substantively, we find that, in the context of food delivery services, baseline purchase propensity experiences a substantial boost after the initial purchase and continues to rise with each subsequent purchase, albeit at a diminishing rate. This pattern of positive reinforcement is indicative of a habit-formation effect. Additionally, we observe a recency-trap effect, whereby baseline purchase propensity declines progressively with each additional day without a subsequent purchase. Furthermore, we find that ad responsiveness also varies with past purchase behavior. In our empirical setting, early repeat purchasers—those with two to four prior purchases—exhibit the highest responsiveness to the focal brand’s TV ads. With respect to purchase recency, ad responsiveness initially declines following a purchase but subsequently rebounds as more days elapse without another purchase.

As modern single-source data—such as those used in this study—become increasingly available, we envision our identification approach as a portable solution that marketing researchers, advertisers, and policymakers can use to more accurately measure TV ad effectiveness with household-level observational data. While RCTs are often costly, logistically challenging, or infeasible in the context of linear TV, our method offers a practical and cost-efficient supplement—or, in many cases, an alternative—enabling robust causal inference at scale by exploiting quasi-random ordering in linear TV advertising.

Several boundary conditions of our identification strategy warrant future extension. First, the quasi-random ordering assumption may not hold in contexts where advertisers secure specific ad positions in advance or where networks employ addressable technologies that adjust ad timing based on viewer characteristics. Second, the assumption of non-strategic viewership may be violated if the focal advertiser elicits systematically different viewer responses. For example, a brand promoting a politically polarizing issue may induce higher levels of ad avoidance relative to other brands, leading to strategic skipping of the focal brand’s ads. Third, although our identification strategy addresses the two most prominent sources of endogeneity—targeting and activity biases—other sources may persist. One such source is heterogeneity in creative content across ad copies. If higher-quality ads are systematically aired during shows with audiences more inclined to purchase, failing to account for this could bias the estimated ad effects. Future research could extend our approach to address these complexities.

All in all, we hope our study stimulates further interest in leveraging the full potential of large-scale, high-granularity single-source data. As the history of marketing science has shown, each new wave of audience measurement data tends to spur advances in empirical methods and scientific discovery (Du et al. 2021). We view our work as part of this ongoing evolution and as a methodological foundation for future research on TV advertising effectiveness.

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Online Appendix A: Proofs of Propositions and Corollary

Proposition 1: *Under the assumption that the DGP of A_{is} follows Equations (1)–(5), it holds that* $corr(IV_{is}, A_{is}) > 0$ *and* $corr(IV_{is}, u_{is}) = 0$*, thereby satisfying the relevance condition and the exclusion restriction, respectively, for IV_{is} to be a valid instrument for A_{is}.*

Proof: The relevance condition, $corr(IV_{is}, A_{is}) > 0$, holds because, recalling Equation (8), $IV_{is} = A_s^b \Delta_{is}$, and $A_{is} = A_s^b P_{is} + A_s^b \Delta_{is}$. This implies that when $A_s^b = 1$, the nonzero values of Δ_{is}, which appears in both IV_{is} and A_{is}, induce a positive correlation between them.

To satisfy the exclusion restriction, we must show that $corr(IV_{is}, u_{is}) = corr\left(A_s^b \Delta_{is}, u_{is}\right) = 0$, which is equivalent to showing that $cov\left(A_s^b \Delta_{is}, u_{is}\right) = 0$.

Since $cov\left(A_s^b \Delta_{is}, u_{is}\right) = E\left(A_s^b \Delta_{is} u_{is}\right) - E\left(A_s^b \Delta_{is}\right) \times E(u_{is})$ and by definition $E(u_{is}) = 0$, it suffices to show that $E\left(A_s^b \Delta_{is} u_{is}\right) = 0$.

We apply the law of iterated expectations: $E\left(A_s^b \Delta_{is} u_{is}\right) = E_u\left(E\left(A_s^b \Delta_{is} u_{is} | u_{is}\right)\right) = \int E\left(A_s^b \Delta_{is} | u_{is}\right) u_{is} h(u_{is}) du_{is}$, where $h(\cdot)$ is the PDF of u_{is}. Therefore, it suffices to show that $E\left(A_s^b \Delta_{is} | u_{is}\right) = 0$ for all u_{is}.

Since $A_s^b \Delta_{is} \neq 0$ only when $A_s^b = 1$ and $\Delta_{is} \neq 0$, it suffices to show that $E\left(\Delta_{is} | A_s^b = 1, 0 < P_{is} < 1, u_{is}\right) = 0$, i.e., the network-induced shifter has an expected value of zero when a household watches a targeted show partially.

Given that $\Pr[\Delta_{is} = 1 - P_{is}] = P_{is}$, and $\Pr[\Delta_{is} = -P_{is}] = 1 - P_{is}$, we have: $E\left(\Delta_{is} | A_s^b = 1, 0 < P_{is} < 1, u_{is}\right) = (1 - P_{is})P_{is} + (-P_{is})(1 - P_{is}) = 0$. **Q.E.D.**

Intuitively, the core of the above proof lies in the fact that, as long as—for targeted shows ($A_s^b = 1$) and partial household show-viewing ($0 < P_{is} < 1$)—the network-induced within-show ad exposure shifter Δ_{is} follows a two-point distribution with an expected value of zero, our proposed household-show-level instrument $IV_{is} = A_{is} - A_s^b P_{is} = A_s^b \Delta_{is}$ satisfies the exclusion restriction.

The above proof also indicates that, before using our proposed IV in an empirical application, one should check whether the mean of nonzero $\widehat{\Delta}_{is}$ is close to zero.

Corollary 1: *Under the assumption that the DGP of A_{is} follows Equations (1)–(5), it holds that* $corr(IV_{it}, A_{it}) > 0$ and $corr(IV_{it}, u_{it}) = 0$*, thereby satisfying the relevance condition and the exclusion restriction, respectively, for IV_{it} to be a valid instrument for A_{it}.*

Proof: Recall that $IV_{it} = \sum_{s \in S_t} IV_{is} = \sum_{s \in S_t} A_s^b \Delta_{is}$. The relevance condition $corr(IV_{it}, A_{it}) > 0$ clearly holds because $A_{it} = \sum_{s \in S_t} A_{is} = \sum_{s \in S_t}\left(A_s^b P_{is} + IV_{is}\right) = \sum_{s \in S_t} A_s^b P_{is} + \sum_{s \in S_t} A_s^b \Delta_{is}$.

For the exclusion restriction, having $corr(IV_{it}, u_{it}) = corr\left(\sum_{s \in S_t} A_s^b \Delta_{is}, u_{it}\right) = 0$ is equivalent to $cov\left(\sum_{s \in S_t} A_s^b \Delta_{is}, u_{it}\right) = 0$. Given that $cov\left(A_s^b \Delta_{is}, u_{is}\right) = 0$ (as proven in Proposition 1) and u_{is} represents an expanded set of confounders that includes u_{it} as an element, it directly follows that $cov\left(A_s^b \Delta_{is}, u_{it}\right) = 0$. Applying the additive property of covariance, we have $cov\left(\sum_{s \in S_t} A_s^b \Delta_{is}, u_{it}\right) = \sum_{s \in S_t} cov\left(A_s^b \Delta_{is}, u_{it}\right) = 0$. **Q.E.D.**

Proposition 2: *Under the assumption that the DGP of A_{is} follows Equations (1)–(5), it holds that* $corr_{(i,s)}\left(IV_{is}, A_s^b\right) = 0$, $corr(IV_{is}, P_{is}) = 0$, and $corr\left(IV_{is}, A_s^b P_{is}\right) = 0$.

Proof: We first prove $corr\left(IV_{is}, A_s^b P_{is}\right) = 0$. Recall that $IV_{is} = A_s^b \Delta_{is}$. Showing $corr\left(A_s^b \Delta_{is}, A_s^b P_{is}\right) = 0$ is equivalent to showing $cov\left(A_s^b \Delta_{is}, A_s^b P_{is}\right) = 0$. Note that $cov\left(A_s^b \Delta_{is}, A_s^b P_{is}\right) = E\left(\left(A_s^b\right)^2 \Delta_{is} P_{is}\right) - E\left(A_s^b \Delta_{is}\right) \times E\left(A_s^b P_{is}\right)$.

Since $A_s^b \Delta_{is} \neq 0$ only when $A_s^b = 1$ and $\Delta_{is} \neq 0$, to show $E\left(A_s^b \Delta_{is}\right) = 0$, it suffices to show $E\left(\Delta_{is} | A_s^b = 1, 0 < P_{is} < 1\right) = 0$, i.e., the network-induced shifter has an expected value of zero when a household watches a targeted show partially.

Given that $\Pr[\Delta_{is} = 1 - P_{is}] = P_{is}$, and $\Pr[\Delta_{is} = -P_{is}] = 1 - P_{is}$, we have: $E\left(\Delta_{is} | A_s^b = 1, 0 < P_{is} < 1\right) = (1 - P_{is})P_{is} + (-P_{is})(1 - P_{is}) = 0$, which implies $E\left(A_s^b \Delta_{is}\right) = 0$.

Similarly, since $\left(A_s^b\right)^2 \Delta_{is} P_{is} \neq 0$ only when $A_s^b = 1$ and $\Delta_{is} \neq 0$, to show $E\left(\left(A_s^b\right)^2 \Delta_{is} P_{is}\right) = 0$, it suffices to show $E\left(\Delta_{is} P_{is} | A_s^b = 1, 0 < P_{is} < 1\right) = 0$ for all P_{is}.

Again, given that $\Pr[\Delta_{is} = 1 - P_{is}] = P_{is}$, and $\Pr[\Delta_{is} = -P_{is}] = 1 - P_{is}$, we have: $E\left(\Delta_{is} P_{is} | A_s^b = 1, 0 < P_{is} < 1\right) = (1 - P_{is})P_{is}P_{is} + (-P_{is})P_{is}(1 - P_{is}) = 0$ for all P_{is}, which implies $E\left(\left(A_s^b\right)^2 \Delta_{is} P_{is}\right) = 0$, thereby concluding the proof of $corr\left(IV_{is}, A_s^b P_{is}\right) = 0$.

As for showing $corr_{(i,s)}\left(A_s^b \Delta_{is}, A_s^b\right) = 0$, it is equivalent to showing $cov_{(i,s)}\left(A_s^b \Delta_{is}, A_s^b\right) = 0$. Note that $cov_{(i,s)}\left(A_s^b \Delta_{is}, A_s^b\right) = E\left(\left(A_s^b\right)^2 \Delta_{is}\right) - E\left(A_s^b \Delta_{is}\right) \times E\left(A_s^b\right)$.

Following the proof of $corr\left(IV_{is}, A_s^b P_{is}\right)$, similar to showing $E\left(A_s^b \Delta_{is}\right) = 0$, it is straightforward to show $E\left(\left(A_s^b\right)^2 \Delta_{is}\right) = 0$, thereby concluding the proof for $corr_{(i,s)}\left(IV_{is}, A_s^b\right) = 0$.

As for showing $corr(IV_{is}, P_{is}) = 0$, it is equivalent to showing $cov(IV_{is}, P_{is}) = 0$. Note that $cov\left(A_s^b \Delta_{is}, P_{is}\right) = E\left(A_s^b \Delta_{is} P_{is}\right) - E\left(A_s^b \Delta_{is}\right) \times E(P_{is})$.

Following the proof of $corr\left(IV_{is}, A_s^b P_{is}\right)$, similar to showing $E\left(\left(A_s^b\right)^2 \Delta_{is} P_{is}\right) = 0$, it is straightforward to show $E\left(A_s^b \Delta_{is} P_{is}\right) = 0$, thereby concluding the proof of $corr(IV_{is}, P_{is}) = 0$. **Q.E.D.**

Like Proposition 1, the core requirement for Proposition 2 to hold is also that, for targeted shows ($A_s^b = 1$) and partial household show-viewing ($0 < P_{is} < 1$), the network-induced within-show ad exposure shifter Δ_{is} follows a two-point distribution with an expected value of zero.

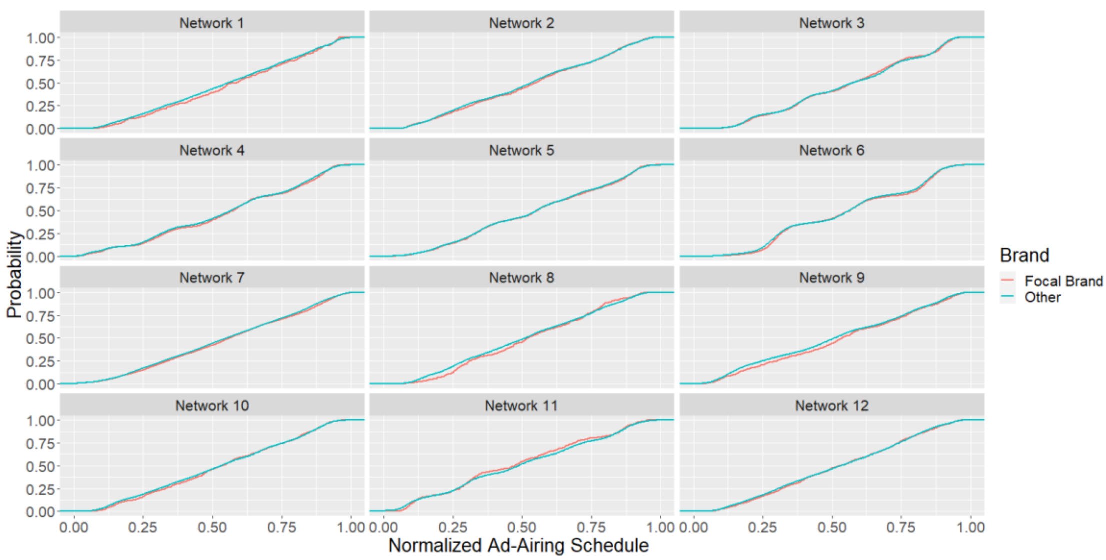
Finally, Proposition 2 and its proof also suggest that, before using our proposed IV in an empirical application, one should, in addition to checking whether the mean of nonzero $\widehat{\Delta}_{is}$ is close to zero, conduct a falsification check to assess whether $corr\left(A_s^b \widehat{\Delta}_{is}, A_s^b \widehat{P}_{is}\right)$ is close to zero. If that is the case, one would have greater confidence in the exogeneity of $IV_{is} = A_s^b \Delta_{is}$ given its empirical orthogonality to $A_s^b \widehat{P}_{is}$, the expected treatment estimate resulted from a combination of the endogenous determinants of the DGP.

Online Appendix B: Within-Show Ad Airing Time Distributions

For each TV network that aired ads from the focal brand during our study period, we examine the empirical distribution of within-show focal brand ad airing times, normalized by show duration, where zero indicates the start and one indicates the end of a show. We conduct the same analysis for all non-focal brand ad airings to enable a direct comparison of within-show ad scheduling patterns between focal and non-focal brands.

In addition to the comparison of the distributions of focal versus non-focal brand ad airing times across shows on MTV in Figure 3, Figure B1 extends this analysis to all major networks targeted by the focal brand during our study period. Without exception, across all networks, the distributions of within-show airing times are statistically indistinguishable between focal and non-focal brand ads, further supporting the notion that networks scheduled the focal brand's ads in a quasi-random rotation alongside those of other brands. In other words, while the focal brand may have targeted specific shows, it did not target specific time slots within those shows. This suggests that within-show ad airing timing provides a source of exogenous variation in determining which partial viewers of a targeted show were exposed to the focal brand's ads.

Figure B1. CDFs of Within-Show Ad Airing Time Distributions of the Top 12 Networks



Notes. Within-show ad airing times are normalized by show duration, with zero representing the start and one representing the end of a show. The analysis includes the twelve networks with the highest number of focal brand ad airings during the study period.

Online Appendix C: Additional Results and Robustness Checks

C.1. Model Fit Comparison

We assess model fit across four specifications of the function *f* in Equations (17) and (18): (1) linear, (2) linear + squared, (3) log, and (4) log + squared log. The log-likelihood (LL), AIC, and BIC values presented in Table C1 favor the fourth specification—log + squared log—which is used in our proposed model.

Table C1. Model Fit Comparison

	Linear	Linear + Squared	Log	Log + Squared Log (Proposed Specification)
LL	-759,322	-754,543	-753,716	-753,399
AIC	1,518,741	1,509,190	1,507,527	1,506,901
BIC	1,519,436	1,509,943	1,508,222	1,507,654

C.2. Estimating Within-Show Focal Brand Ad Exposure Probability

We nonparametrically estimate P_{is}—household i's focal brand ad exposure probability within show *s* in the event the show is targeted by the focal brand—in three steps.

Step 1: Constructing the network-specific ad airing time distribution. We discretize within-show time at the second level and normalize each show's duration to an interval from 0 (beginning) to 1 (end), defining an ad's airing time as the proportion of the show that has elapsed before the ad appears. For instance, an ad inserted at the 15th minute of a 30-minute show corresponds to a normalized ad airing time of 0.5.

Using all ad airings of all brands, we construct the empirical PDF of within-show ad airing time distribution for each targeted network. These empirical PDFs provide estimates of $l_{k_s}(x)$, which governs the within-show ad airing time distribution for show *s* that is broadcast by its network k_s, where $x \in [0,1]$ represents the normalized time within the show.

The left panel of Figure C1 visualizes our estimate of $l_{k_s}(x)$ for MTV, a representative targeted network in our data. The focal brand's ad airing times follow a tri-modal distribution, with concentrations just after the first and second quarters of a show and immediately before the end of the show.

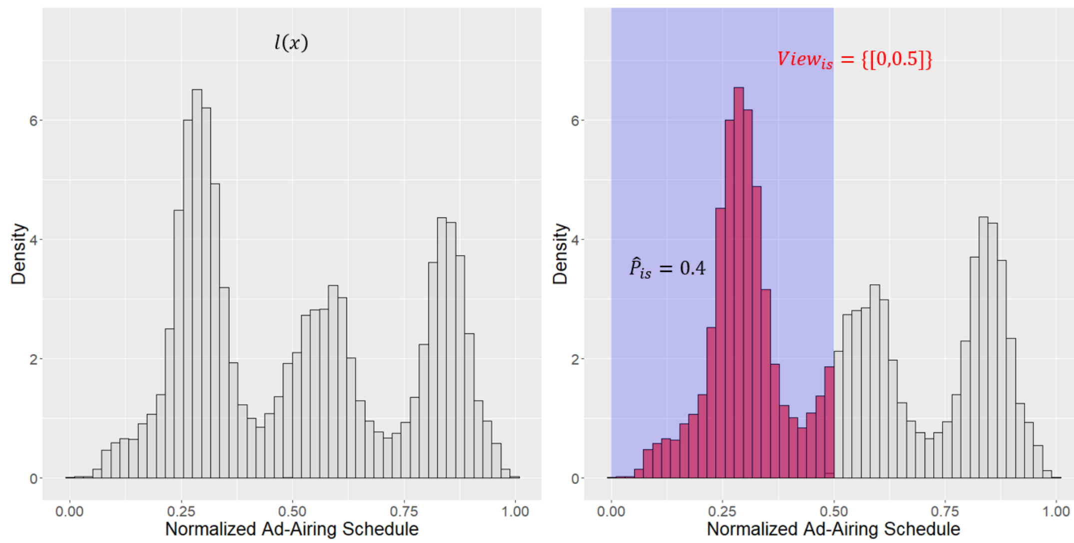
Step 2: Identifying household show viewing patterns. Next, we map each household's actual viewership of each targeted show onto the normalized show duration, based on second-by-second TV viewing data provided by LGADS. For example, if household *i* watched the first 15 minutes of a 30-minute show *s*, then $View_{is} = \{[0,0.5]\}$. This is illustrated in the right panel of Figure C1, where the shaded area represents the portion of the show that was viewed.

Step 3: Computing the within-show probability of focal brand ad exposure. Conditional on the observed household show viewership $View_{is}$, and the estimated targeted network's ad airing time PDF $\hat{l}_{k_s}(x)$, our estimate of P_{is}, denoted by $\hat{P}_{is}$, can be expressed as: $\hat{P}_{is} = \int_{View_{is}} \hat{l}_{k_s}(x)\, dx$.

The red bars in the right panel of Figure C1 visualize the calculation of $\hat{P}_{is}$: the red area under the curve (AUC) represents our nonparametric estimate of $\hat{P}_{is}$ for a household that watched the first half of a targeted show on MTV. In this example, the red AUC accounts for 40% of the total AUC, indicating a 40% probability that the household could be exposed to the focal brand's ad, given $View_{is}$ and $\hat{l}_{k_s}(x)$.

For each household *i* and show *s*, we compute the household-show-level IV as $\widehat{IV}_{is} = A_{is} - A_s^b \hat{P}_{is}$. In the illustrative example shown in Figure C1, we have $\widehat{IV}_{is} = 1 - 0.4 = 0.6$ if household *i* was actually exposed to the focal brand ad, and $\widehat{IV}_{is} = 0 - 0.4 = -0.4$ if unexposed. Hence, the expected value of this IV is $E(IV_{is}) = 0.4 \times 0.6 + 0.6 \times (-0.4) = 0$.

Figure C1. Within-Show Ad Airing Time Distribution and Focal Brand Ad Exposure Probability



Notes. X-axis represents normalized show duration from 0 (beginning) to 1 (end), and Y-axis represents the probability density of an ad airing. In the right plot, the shaded area represents the portion of the show that was viewed ($View_{is} = \{[0,0.5]\}$), and the red bars visualize the calculation of estimated within-show focal brand ad exposure probability ($\hat{P}_{is}$).

C.3. Correlation in Focal Brand Ad Exposures vs. Correlation in Network-Induced Shifters Across Targeted Shows

When household TV viewing is correlated across shows, targeting shows with similar audiences can result in positive correlations in A_{is} (focal brand ad exposures) across these shows. For example, a household that watches targeted show A and is exposed to a focal brand ad is more likely to also watch targeted show B, thereby increasing the likelihood of being exposed to the focal brand ad during show B. In contrast, if $\widehat{\Delta}_{\mathrm{is}}$, the network-induced shifter, is truly exogenous, the correlation in $\widehat{\Delta}_{\mathrm{is}}$ across targeted shows should be zero. This is empirically confirmed using the following procedure:

1. Draw a sample of targeted shows with high correlation in household viewership.
2. Calculate $\widehat{\Delta}_{is}$.
3. For each pair of sampled targeted shows, compare the between-show correlation in A_{is} with between-show correlation in $\widehat{\Delta}_{is}$.

Table C2 presents pairwise correlations in A_{is} across four targeted shows with high household viewership correlation. Table C3 reports the corresponding pairwise correlations in $\widehat{\Delta}_{is}$. The between-show correlations in A_{is} are all positive and significant ($p < 0.01$), suggesting that households exposed to a focal brand ad during one targeted show are more likely to be exposed to focal brand ads during other targeted shows as well. In contrast, the pairwise correlations in $\widehat{\Delta}_{is}$ are much smaller and statistically insignificant, providing empirical support for the exogeneity of $\widehat{\Delta}_{is}$.

Table C2. Pairwise Between-Show Correlations in Household Focal Brand Ad Exposure

	Show 1	Show 2	Show 3	Show 4
Show 1	-	0.074	0.047	0.050
Show 2	0.074	-	0.048	0.066
Show 3	0.047	0.048	-	0.099
Show 4	0.050	0.066	0.099	-

Table C3. Pairwise Between-Show Correlations in Network-Induced Shifter

	Show 1	Show 2	Show 3	Show 4
Show 1	-	-0.019	0.007	-0.007
Show 2	-0.019	-	0.006	-0.019
Show 3	0.007	0.006	-	-0.007
Show 4	-0.007	-0.019	-0.007	-

C.4. Placebo Test Using Exposures to TV Ads from Another Brand

We conduct a placebo test by replacing the focal brand's ad exposure stock in our ad response model with that of a major automobile manufacturer, adjusting the control function term accordingly. The results show no statistically significant effect for the placebo ad exposure stock ($\beta_0 =$ -0.0008, SE = 0.002) and no statistically significant effect for the corresponding control function term ($\delta =$ 0.0002, SE = 0.0007), while the other model parameter estimates remain largely unchanged. These findings suggest that the positive and significant effect estimates for the focal brand's ad exposure stock and the corresponding control function term in our proposed model are unlikely to be coincidental.