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Abstract

The conventional wisdom amongst advertising practitioners holds that digital video recorders (DVRs) enable consumers to skip television advertising, thereby reducing aggregate exposure to advertising and its demand-shifting impact on branded goods sales. As a result, firms are allocating a lower proportion of their advertising budget to television. However, few large scale studies exist to assess the validity of this premise. We redress this consideration via a multimillion dollar, three-year field study sponsored by five firms and involving 6,421 households, each of whom received an offer for a free DVR and service (of which 968 accepted). For each household, we observe their complete shopping history for 48 consumer packaged goods (CPG) categories during the 13 months prior and the 26 months following the issuance of the DVR offer. We fail to reject the null of no DVR effect on household spending on branded goods either 0 to 13 months or 13 to 26 months after the DVRs are shipped. We also find that our predicted DVR effect is tightly centered around 0, suggesting that our data may have sufficient power to identify a true null effect. We offer evidence for, but cannot test explicitly, one potential explanation for the lack of a DVR effect: namely that there is a surprisingly low incidence of advertisement-skipping despite relatively heavy usage of viewing of recorded television content. In sum, our analysis indicates that fears about the adverse effect of DVRs on the efficacy of advertising expenditures is empirically not substantiated.

Keywords: Digital Video Recorder, advertising, field study, brand, consumer packaged goods

1 Overview

TiVo pioneered the new Digital Video Recorder (DVR) devices used to record and playback television content. Debuted in March of 1999, the DVR market has quickly grown. According to Jupiter Media, 19% of households with televisions had DVRs by 2007. Moreover, the advent of digital television has led many cable and phone companies to offer DVRs as part of their service and adoption rates are expected to rise to 35% by 2012. By enabling users to fast forward through advertisements, the DVR has unleashed a wave of anxiety that the device will curtail the advertising revenues of network television. Consistent with this view, Forrester predicted in 2004 that households would be watching 15% fewer commercials by 2007 (Economist 2004). Surveys of TiVo users routinely reported high self-reported levels of ad-skipping. One Jupiter Media report found that 47% of surveyed users indicated skipping commercials "most of the time."

Advertisers have been scrambling to respond to these findings. A 2006 survey by the Association of National Advertisers found that 60% of advertisers intended to decrease television advertising budgets in response to DVRs and that 70% believed DVRs and video-on-demand (VOD) would reduce or destroy the effectiveness of the 30-second TV advertising spot. Similarly, a 2004 survey conducted by the Advertising Research Foundation found that 76% of advertisers believed that DVRs would change the advertising market place. These dire predictions have led the popular press to question the future of US network television advertising revenues, an industry in which the 6 top English-language national broadcast networks (CBS, ABC, FOX, NBC, UPN and WB)[1] garnered more than $2.5 billion in 2006 (Business Week 2006). Based on its survey, Jupiter Media concluded (p. 4),

> "ad skipping by DVR users poses a significant threat to advertising spending. In response, advertisers and television programmers must devise new strategies for combating the potentially disastrous effects of ad skipping."

Industry experts have partially blamed DVR-enabled ad-skipping for the decline in television advertising in the U.S between 2006 and 2007; with network television falling 1.5% and spot TV decreasing 5.1% (PBS[2]; Nielsen Media Research 2008[3])). Branded goods manufacturers have responded to the

[1] UPN and WB have since merged to form the CW network.
[2] http://www.pbs.org/mediashift/2008/03/the-new-rules-of-media091.html
[3] http://www.reuters.com/article/pressRelease/idUS194788+31-Mar-2008+PRN20080331

perceived threat by seeking ways to creating advertise content that is immune to skipping (Miller 2007). In short, the conventional wisdom seems to be that DVRs present a formidable threat to the television advertising model.

Surprisingly, besides self-reports, there is no hard evidence that DVRs have generated a decline in *actual* advertising viewing, nor is there any evidence that DVRs have had any material impact on the *actual* sales performance of advertising-heavy consumer branded goods or the product categories in which they sell (Wilbur 2008). Accordingly, our goal herein is to analyze household panel shopping data to test for a DVR effect on actual purchase behavior for goods supported by television advertising. Following the conventional wisdom of the consumer goods industry and network television, the underlying theory is that a DVR's ad-skipping functionality reduces a household's exposure to advertising. Under the maintained assumption that "advertising stimulates demand," DVRs would therefore reduce demand for advertised goods ceteris paribus. In turn, one would expect DVR usage to reduce the relative share of advertised versus unadvertised brands.

Our data arise from a multi-million dollar field study conducted in conjunction with IRI, TiVo, and a consortium of major consumer packaged goods (CPG) manufacturers. A total of 13,946 households in IRI's Behaviorscan sample were offered a free DVR and subscription to TiVo. The sample yielded a meaningful set of DVR adopters (i.e., in the end 1588 households adopted the offer) but was small enough to offset concerns of competitive reactions in the market place (i.e. CPG firms would not adjust their prices and/or promotions in response to the incremental DVR usage). The DVR usage data were then matched with each household's shopping behavior from 47 CPG categories for 13 months prior to the DVR offer and 26 months after. These data differ markedly from the self-report surveys used in previous DVR studies inasmuch as they reflect actual skipping behaviors collected in an unobtrusive manner.To assess whether a household skipped advertisements, we supplemented the DVR usage data with a complete network advertising schedule for 9 of the brands in our sample during the post-treatment period. For these 9 brands, we can determine the frequency with which households were exposed to advertisements and the frequency with which, conditional on exposure, households skipped ads. A survey was also conducted to collect information on each household's technology ownership and lifestyle characteristics.

To measure the DVR effect, we consider three behaviors. First, we consider expenditures on private label products. If a DVR truly moderates the effectiveness of advertising, then we would expect to see an increase in expenditures on unbranded alternatives as consumers shift their purchasing away from advertising-supported (i.e. branded) goods. Second, we analyze the expenditures on the most heavily advertised brands in each category. Here too we expect a decline in sales under the null hypothesis that DVRs enhance the consumers' ability to avoid their advertisements. Third, we explore consumer expenditures on new products. Past research has routinely documented positive and statistically significant advertising effects on demand for new goods (c.f. Ackerberg 2001). Once again, we would expect to see expenditures on new products decline under the null of a moderating effect of TiVo.

Ideally, a true randomized field experiment would be conducted whereby the overall set of IRI households would be assigned by randomization to treatment (DVRs) and control conditions. Unfortunately, the small size of the overall set of IRI households (13,946 households) coupled with low DVR adoption rates (less than 20%) are unlikely to generate treatment and control samples with sufficient statistical power. Since IRI cannot compel a households to adopt a DVR, one could at best expect a decent-sized sample to estimate the "intent-to-treat" effect (i.e. average causal effect of the opportunity to receive a free DVR). But, one would not expect to obtain a sufficient-sized sample to estimate the effect of "treatment-on-the-treated" (i.e. the average causal effect of actually adopting and using a DVR).

Instead, we use standard econometric techniques to restore a quasi-experimental structure to the data. Exploiting the panel structure of the data, we "difference out" household-specific effects (c.f. Meyer 1995). A DVR treatment effect can then be identified by comparing differences across households that adopt DVR (treatment group) and households that do not (control group). This difference in differences approach helps to control for time specific effects common to both groups (e.g., increases in Internet penetration might lead to fewer advertisements allocated to television). Of course, difference-in-differences do not control for group-specific time effects. Therefore, we further explore the robustness of our difference-in-differences estimates to an instrumental variables procedure that uses household technology ownership to instrument for the potential correlation between DVR adoption and unobserved innovations in a household's spending.

Surprisingly, we find no statistical evidence for a TiVo effect on purchase behavior one year after the issuance of DVRs. After differencing our three behavioral measures at the household level, we find no effect of TiVo ownership on expenditures on private labels or on heavily advertised branded goods. Likewise, TiVo ownership does not affect the degree to which consumers buy new products. Given that TiVo is a new technology, it is possible that households require a learning period to experiment with the ability to skip ads. However, we are also unable to detect a statistically significant TiVo effect on expenditures even 27 months after the TiVo units were distributed. Therefore, contrary to the conventional wisdom, we do not find an impact of TiVo usage on shopping behavior for branded CPG products.

Several possible explanations exist for our inability to detect a treatment effect in our field study. First, the DVR treatment effect may be quite small and, thus, difficult to detect. The empirical advertising literature has typically found very small advertising elasticities of sales (c.f. Assmus et al. 1981 and Lodish et al. 1995). Recent advertising experiments on the Internet have required extremely large sample sizes of more than 1 million subjects in order to generate sufficient power to detect a significant effect of advertisements (c.f. Lewis and Reiley 2008). While low power might be a limitation of our sample, we note that the confidence intervals around our TiVo point estimates are fairly tight around zero. This fact is suggestive that there may simply not be an economically meaningful TiVo effect.

A different line of reasoning attributes the lack of a TiVo effect to the manner in which households use the technology. We explore this argument by analyzing the TiVo usage log files of our treated households. Among those with DVRs, only 4.5% of the total television viewing occasions consist of recorded, as opposed to live, television. Even for the recorded television viewing, households with DVRs fast-forward only 71% of the commercial breaks. Therefore, our sample households watch considerably more live television (where fast-forwarding is not possible) than recorded television. Even during recorded television, households skip less than three quarters of the advertisements they see. These low ad-skipping rates are consistent with other studies. Pearson and Barwise (2007) report fast forward rates of 68% in recorded television content using ethnographic methods wherein they observed persons watching shows to ascertain how often they fast forwarded through the advertisements. Recently, Nielsen Media Research reported that DVR users skipped only 60% of the commercials in a usage study spanning one week. It is even possible to watch an advertisement several times when viewers watch recorded shows repeatedly. In short, recent usage-based studies of ad-skipping indicate that DVRs may not lead to as much fast-forwarding of advertisements than previous studies based on self-reports. Consequently, it is possible that the lack of a DVR treatment effect arises from the fact that DVRs do not induce as strong a manipulation on advertising exposure as is currently conjectured by practitioners.

Irrespective of the amount of ad-skipping, it is also possible that fast-forwarding an advertisement need not eliminate the exposure effect. Mandese (2004) finds that 67% of DVR viewers always (or sometimes) notice the advertisements through which they fast-forward, suggesting that, even when forwarded, advertisements can be effective. Recent research by Millward Brown (du Plessis 2007) indicates that fast forward exposures, relative to regular speed exposures, attenuates advertising recall by only a few percentage points. Similar findings are reported by Siefert et al. (2008) who find evidence that subjects watch advertisements as they fast forward.

Another explanation regarding the negligible effect of DVRs on brand sales is that they can increase overall television viewership. Specifically, DVRs facilitate the viewing experience by allowing the viewer to match their favorite TV content with their leisure time. TiVo therefore increases the effective content quality, and may lead to more TV consumption.

The remainder of the paper is organized as follows. In section 2, we describe the nature of the DVR market and its immediate implications for a household's ability to skip advertisements. In section 3, we describe the design and implementation of the field study. We then outline our analysis and report our findings in section 4. Finally, we conclude in section 5 by summarizing our findings and offering future research directions.

2 Field Study Design

The data for this field study were collected by Information Resources, Incorporated and TiVo and was sponsored by three major consumer packaged goods firms. The study was conducted in 4 of IRI's Behaviorscan markets (http://usa.infores.com): Eau Claire, Pittsfield, Cedar Rapids and Midlands. IRI first constructed a sub-sample of 13,946 households deemed to be "potential DVR purchasers." The sample was constructed based on the two conditions that a panelist not already own a DVR (information obtained from pre-treatment surveys) and that a panelist agree to remain an active member of the IRI panel.

The initial objective was to construct the treatment and control conditions via a randomization. In September 2004, IRI randomly assigned each of the sample households to intention-to-treat

(3,064) and control (10,882). The intent-to-treat condition consisted of an offer to receive a free DVR from TiVo Inc, as well as a subscription to TiVo's service. DVRs were scheduled to be delivered to those households that accepted the offer at the beginning of 2005.[4] Initial acceptance was low and generated too small a treatment group to obtain any statistical power. In October 2004, IRI extended the offer to all eligible Behavior Scan households, eliminating the randomization and, consequently, abandoning the experimental design of the data. After this subsequent solicitation, a total of 1,587 panelists (11.4% of the sample) accepted the offer. In our analysis below (section 4), we outline how we work around the self-selection of households into TiVo treatment and non-TiVo control conditions.

A technology ownership survey was also issued to each household to determine whether a household owns a DVR and to assess a household's ownership of various other consumer technologies such as cell phones and DVD players. A total of 8,786 households (63% of the sample) responded to the survey. Of these survey respondents, 1,222 were in the TiVo treatment condition (conditional response rate of 77%) and 7,564 were in our non-TiVo, or control, condition (conditional response rate of 61%). For our analysis, we exclude the 1,282 (17%) control households that reported already owning a DVR.

Finally, the survey data were matched with demographic files and panelist shopping histories for the CPG products in 48 categories over the 55 weeks (14 4-week periods) pre treatment and 112 weeks (28 4-week periods) post treatment. These purchase records were subsequently limited to three markets after the 28th 4-week period (the 14th 4-week period of post-treatment) because data collection in the Midlands market ceased. All totaled, the resulting interlaced sample size is comprised of 819 TiVo treated households and 3,585 control households across the four dozen categories excluding the Midlands market, and 968 treatment households and 5,453 households inclusive of Midlands. We describe these data and our dependent measures in more detail in section 3, below.

[4]Hereafter, we refer to the time period from 2005 onwards as the treatment period, and the time period prior to 2005 as the pre-treatment period.

3 Data

As noted in Section 2, the data for this study comprise several files: (1) IRI Behaviorscan panel data containing household level purchase information; (2) TiVo log files summarizing each treated household's TiVo usage; (3) TNS advertising data regarding the annual advertising expenditure of each brand in our sample in each of the 4 geographic markets; (4) survey data on household demographics and technology usage; and (5) the network advertising schedule for nine of the brands in our sample. We now describe in detail each of these databases along with the measures we compute with them.

3.1 IRI Behaviorscan Household Panel Data

The IRI panel data contain the entire purchase history for each of the households in the treatment and control groups across 48 different CPG categories in four IRI markets. The data span the time period from the last month of 2003 to the end of the first quarter of 2007, yielding a 112-week post-treatment period and a 55-week pre-treatment period. For each market, m, category, c and household, i, we compute the following two measures for each observed shopping trip in the panel data: (1) the total dollar sales of the top three advertised brands in the category, $S3_{mci}$, and (2) the total dollar sales of the private label brands, PL_{mci}.[5] These measures are then time-aggregated, by household and category, into 3 periods. The first period consists of the 55-week pre-treatment period, $(S3^0_{mci}, PL^0_{mci})$. The second period consists of the subsequent 56-week short-run post-treatment period, $(S3^1_{mci}, PL^1_{mci})$. The third and final period consists of the subsequent 56-week long-run post-treatment period, $(S3^2_{mci}, PL^2_{mci})$. The use of two post-TiVo data periods enables us to ascertain whether learning about TiVo operation or other time-based effects lead to a change in the DVR effect over time. For comparability, we normalize total period expenditures by the number of weeks in the period (though the periods are quite close in length). For a household that never purchases in a given category during the 39 months, we treat its data as missing for that category, as opposed to treating it as zero expenditure (i.e., we exclude the household from regressions for that category).

We also measure a household's expenditures on new products (NP^1_{mci},NP^2_{mci}), as designated by

[5] We describe the advertising data used to determine the largest selling brands in section 3.2.

the 2005 IRI New Product Pacesetters list, which includes UPCs for 132 food and 120 non-food brands. According to IRI's web site, "to qualify for the list, a brand must have been introduced between February 2004 and January 2005, so that it had a full 52 weeks of sales data by December 2005, and must have achieved at least $7.5 million in first-year retail sales in the food, drug and mass outlets excluding Wal-Mart. For each new product, we again time-aggregate a household's expenditures into 3 periods, pre-treatment, post-treatment short-run and post-treatment long-run.

		2004			2005			2006		
Variable	Sample	Obs	Mean	SD	Obs	Mean	SD	Obs	Mean	SD
$S3_{mci}$	DVR	15666	28.65	70.89	15564	30.41	73.63	15211	30.40	72.56
	Non-DVR	43898	26.71	64.97	43057	29.00	69.53	42213	28.65	13.67
PL_{mci}	DVR	15666	3.74	11.97	15564	4.20	13.66	15211	4.29	14.69
	Non-DVR	43898	3.95	13.34	43057	4.44	14.85	42213	4.49	14.85
NP_{mci}	DVR				1389	12.64	17.69	1505	15.15	25.94
	Non-DVR				3026	12.78	20.96	3268	15.52	28.02

Table 1: Summary Statistics (Cents per Category per Week per Household)

3.2 Advertising Data

To determine which products in our IRI panel data are supported by television advertising, we use TNS AdSpender data for 2004 and 2005. This service records dollar expenditures for a wide array of brands, including consumer packaged goods. Matching the TNS and IRI data is complicated by the lack of consistency in the manner in which each company identifies a brand. Owing to different categorization and naming conventions for brands and items, merging had to be done manually and, for this reason, we focus on the top 3 brands (based on their share of category expenditures) that advertise in each category. Since total category advertising expenditures are typically highly concentrated amongst these top 3 brands, we do not expect our analysis to be very sensitive to this truncation at the top 3 brands.

3.3 TiVo Log Files

TiVo log files track each TiVo-treated household's moment-by-moment usage of the DVR. For each machine, these files are sent nightly to a central server where they are stored. We can match each IRI panelist's machine id, and hence log file, with their purchase data. The TiVo log files are available from July 1, 2005 to the end of July 4, 2007; although the actual distribution of DVRs to households began at the start of 2005.

The TiVo log files record all television content viewed on the TiVo, including live, "near-live" and recorded content from the TiVo's hard drive. "Near live" content pertains to live shows that were paused and, possibly, followed by accelerated viewing. The log files also contain the Tribune Media Services identifier of each show that was watched and the channel on which it aired. The DVR records the time of the viewing and the offset into the show in which the view started but it does not explicitly indicate when a viewing ended. In addition, we observe all keystrokes, such as fast-forwarding and pausing, and the time these keystrokes were entered. From these data, we can infer the fraction of a treated household's total television content that is viewed live versus recorded, how often the DVR is used (as defined by keystrokes) and the amount of fast-forwarding done by a households and the specific time that a fast forward occurred into a show as well as its duration.

For each household, we construct several variables summarizing the usage of the DVR. In particular, we compute the total number of keystrokes executed by a household i, KS_i, and the number of fast forwards, FF_i. The former captures the intensity with which a DVR is used. While the latter is related to the amount of advertising skipping, it is confounded with fast-forwarding through non-advertising content. We further construct the ratio of fast forwards to total keystrokes to determine the relative degree of fast forward behavior. This ratio controls for the possibility that those who fast forward often also happen to watch TV more often and therefore be exposed to more advertisements. The average number of keystrokes per DVR is 119,092 (standard deviation = 127,115) and the average number of forwards is 15,899 (standard deviation = 24,121).

The DVR log files do not contain descriptive information pertaining to advertisements broadcast during a show. However, in section 3.5, below we discuss the 9 brands for which we observe complete advertising schedule and, hence, for which we can assess actual household-level advertisement exposures. Merging the fast-forwards with these exposures will allow us to infer the degree of actual advertising-skipping by panelists for these 9 brands.

3.4 Survey Data

The survey data were initially collected to screen-out households that already own a DVR. However, we use the additional survey information about technology ownership to construct as an instrument to resolve the self-selectivity of households into the TiVo treatment condition. The underlying intuition is that households that already own other media-related technology are more likely to accept the offer for a free DVR and TiVo service. The survey provides information regarding the ownership of 17 devices such as DVD players, PDAs and Satellite Radio. We construct a technology ownership index by computing the fraction of surveyed devices owned by each household. For the TiVo households, the mean index value is 34% (standard deviation = 0.14) while the corresponding mean for the non-TiVo households is 21% (standard deviation = 0.14). Hence, those that voluntarily accept the TiVo offer are more prone to be adopters of technology. We discuss the implications of this difference further in sections 4.1 and 4.2. In addition, a separate survey was used to assess household demographics. Statistics for these data are also presented in section 4.1.

3.5 Brand Advertising Broadcast Data

As discussed above, TiVo log files do not provide information on actual advertisement-skipping as they do not track the specific advertisements broadcast during a show. In theory, the prediction of a TiVo effect on brand buying behavior is predicated on the belief that TiVo users skip ads. We seek to ascertain whether the self-reported advertising skipping rates of nearly 50% by the Jupiter study are consistent with actual behavior. If the rates are smaller (larger) than persons self-report, it stands to reason that the TiVo effect might be smaller (larger) than industry experts believe.

We supplement the TiVo log file data with the network advertising schedule for nine of the brands in our sample. In principle, one could study advertising exposure simply by matching the time stamps on a household's DVR log file with the calendar of pre-contracted times during which each advertisement was scheduled to air. Such data are problematic since many advertisements are switched across pods, meaning the advertisements are not broadcast at the pre-scheduled times. Instead, IRI manually collected the complete television advertising broadcast schedule for nine of the brands in the sample for the four Behavior Scan markets during the period from April 16, 2005 to June 30, 2006. IRI staffers audited play lists provided by the networks and cable channels. In total, advertisements for the nine brands were aired in our test markets 2,661 times. Due to the labor-intensive method for data collection, our advertisement sample is limited to nine brands, which could reduce the "representativeness" of the sample. These brands comprise primarily household cleaning and personal grooming products. As such, the shows they target are not likely a representative sample of all shows. For instance, these nine goods were rarely advertised during sporting events.

A different sample of advertisements could yield different viewing rates.

By matching these data with the TiVo log files, we can determine whether advertisements for these 9 brands were aired during a household's viewing time and whether the household used the fast forward function during the time of these advertisements. Consequently, we can measure the extent of ad-skipping for the 9 brands.

3.6 The Representativeness of Free DVR Usage

A potential concern with the issuance of a free DVR is that a household may not use it the same way as a household that purchases a DVR. Several steps are taken to verify that our TiVo households exhibit similar DVR usage as typical DVR buyers. First, we contrast various aspects of DVR usage in our treatment group to aggregate usage measures provided by TiVo from its national sample of users during 2005. The national sample watched, on average, 5:24 hours of television per day during the first six quarters of the post-treatment period while the IRI panel watched 5:29 per day, a difference of only 5 minutes. Thus, television usage appears similar. However, we do notice some differences in the TiVo usage for the IRI panel as one might expect because they are novices with regard to using the technology. During the first quarter of 2005, 4 months after the issuance of DVRs, our treated households spent 11% of their viewing time watching recorded content. In contrast, the national TiVo sample spent 25% of their viewing time watching recorded content. We also observe an evolution in the treatment group's usage of their DVRs over time. By the second quarter of 2006, 18 months after the issuance of DVRs, the treated households spent 15% of their viewing time watching recorded content. In contrast, the national TiVo sample spent 22% of their viewing time watching recorded content. We observed a similar evolution in the use of the fast-forward function. In the field study TiVo sample, the use of fast forwards increased from 8.9 per day in Q1 of 2005 to 11.3 per day in Q2 of 2006. Over those same periods, the national TiVo panel's use of fast forwards was 15.3 per day and 14.6 per day respectively. Despite some differences in viewership patterns between our experimental sample and the actual national TiVo sample early on, we do observe a trend towards convergence in usage over time. In spite of the convergence over time, we note an important initial "learning" period for our TiVo sample. In our analysis, we will therefore analyze separate the effect of TiVo on expenditures in the short run (first 12 months) and long run (second 12 months).

4 Analysis

In this section, we report the findings from our analysis. First, we use a year of pre-treatment data to assess the quasi-experimental validity of first-differencing. Our goal is to show that the distribution of expenditures in the treatment and control groups are the same after taking differences and netting out persistent heterogeneity. Second, we assess the average DVR treatment effects on advertised brands and private label brand sales via an OLS regressions on their first-differences. Third, several robustness checks are conducted. We check the robustness of our estimated treatment effects to self-selection into the treatment group. We also explore several potential sources of heterogeneity in the treatment effect that might arise from differences in how intensely a household utilizes the DVR technology. As a last robustness check, we focus only on recently-launched products for which the potential effect of advertising may be more sizable. In general, we will fail to detect a statistically significant TiVo effect in any of these analyses. Moreover, our point estimates are quite tightly-distributed around zero, which is suggestive that TiVo may not have a qualitatively important impact on shopping behavior. Fourth, to elaborate upon the small size of the DVR effectswe look at the DVR log files and document low amounts of recorded television viewing and consequently a low incidence of advertisement-skipping. This low overall recording rate could explain why DVRs have no detectable impact on buying behavior.

4.1 Validating the control sample

We first compare the two sub-samples of households, those that adopt the DVR offer and those that do not. Of particular interest is whether the non-adopters can be used as a valid control sample for measuring the counter-factual expenditures of the TiVo households had they not adopted a TiVo. To the extent these groups are similar, the likelihood that exogenous unobserved differences between groups explain potential differences in behavior across the groups is mitigated. In order to eliminate any confounds pertaining to TiVo, we use only the pre-treatment period, 2004, to assess potential differences across the groups. Though the two samples differ in terms of their demographic composition, we find little difference in their shopping behavior (expressed as differences) pre-treatment.

In Table 2, we report the demographic composition of the two groups. Several notable differences between the two groups emerge. The TiVo households are more likely to have children, earn over \$45,000 and hold white collar jobs. In contrast, the non-TiVo households are more likely to be older couples, over 45 and retired. Moreover, the TiVo adopters have higher average technology ownership scores, meaning that they own more consumer household electronics.

	TiVo Households			Non-TiVo Households		
variable	Obs	Mean	St. Dev.	Obs	Mean	St. Dev.
Technology Ownership	968	0.34	0.14	5375	0.21	0.14
Income $>$ \$45,000	968	0.61	0.49	5375	0.41	0.49
Children	968	0.34	0.48	5375	0.18	0.39
Family Size	968	2.86	1.30	5375	2.31	1.20
Households With Younger Children	968	0.13	0.34	5375	0.06	0.25
Older Singles	968	0.10	0.30	5375	0.23	0.42
Female Head $>$ 45years	921	0.71	0.45	5043	0.85	0.36
Male Head $>$ 45 years	760	0.71	0.46	3699	0.85	0.35
Male Head White Collar	760	0.49	0.50	3699	0.31	0.46
Female Head White Collar	760	0.51	0.50	3699	0.41	0.49
Female Head Retired	921	0.15	0.35	5043	0.33	0.47
Male Head Retired	760	0.14	0.35	3699	0.35	0.48

Table 2: Demographics for the TiVo versus non-TiVo Households

In light of these demographic differences, we compare actual shopping behavior for CPG products to assess potential differences across the two populations that might affect our analysis. We begin by testing for differences in the distribution of annual household expenditures in each of the two groups. We focus on each group's total 2004 spending on advertised and private label CPG products in the 47 categories. Recall from Table 1 that the mean expenditure level before the TiVo treatment is higher in the treatment group than in the control group, a potentially worrisome difference between the groups. Our approach consists of testing for a difference in the distribution across households for the treated group versus that of the control group. We use the Wilcoxon rank-sum test, which is a non-parametric test for assessing whether two samples of observations come from the same distribution (Mann and Whitney 1947; Wilcoxon 1945).

Table 3 reports the results from the Wilcoxon rank-sum test on several expenditure variables aggregated across categories. The test rejects the null of equal distributions for expenditure (late versus early 2004) levels on highly advertised brands and fails to reject the null of equal distributions for the difference in expenditures on private label brands. The inequality of distributions suggests that it would not be prudent to compare purchase behaviors of the TiVo and non-TiVo groups directly. However, by exploiting the panel structure of the data and differencing each panelist's expenditures over time, one can control for unobserved fixed-effects differences in group compositions. We break the pre-TiVo (2004) data into two equal time intervals and construct the cross-time difference in expenditure for each panelist. We fail to reject equality of distributions on the differences, even with our relatively large sample size. Therefore, for the remainder of our TiVo analysis, we will work with first differences in expenditures as our dependent measure.

Dependent variable	z	Obs	p-value
Total advertised expenditures	-6.68	6340	$<$0.00
Change in total advertised expenditures	-1.63	6340	0.10
Private label expenditures	-0.69	6340	0.49
Change in private label expenditures	0.18	6340	0.86

Table 3: Wilcoxon Rank-sum Tests for Equality of Distributions in the TiVo versus non-TiVo Groups in 2004 (pre-TiVo only)

4.2 Estimation

We briefly outline our estimation scheme for the DVR treatment effect. We index the households by $i = 1, ..., I$, categories by $c = 1, ..., C$, the markets by $m = 1, ..., 3$, and the time periods by $t \in \{2004, 2005, 2006\}$. We denote a household's outcome variable (i.e. expenditure in category c by household i, living in market m during year t) as Y_{imct}. We begin with the baseline model:

$$Y_{imct} = \widetilde{\alpha}_{icm} + \widetilde{\alpha}_{mt} + \widetilde{\alpha}_{ct} + \widetilde{\gamma}\mathrm{DVR}_i + \widetilde{\tau}_t \mathrm{DVR}_i I_t + \epsilon_{imct} \tag{1}$$

where DVR_i indicates whether household i is in the DVR treatment group, $I_t \in \{I_{2005}, I_{2006}\}$ indicates whether period t is the post-DVR period in 2005 or 2006, and $\widetilde{\epsilon}$ is a random normal error disturbance. Our main interest lies in estimating the DVR effect on purchases, $\widetilde{\tau}$. Since the model 1 pools expenditures across categories, $\widetilde{\tau}$ measures the average effect across categories and households of DVR treatment on expenditures during the post-treatment period.

To obtain a consistent estimate of $\widetilde{\tau}$ that controls for household heterogeneity, we "difference out" the household-specific intercepts, $\widetilde{\alpha}_{icm}$. That is, we use a first-differences estimator of the DVR treatment effect:

$$\triangle Y_{imct} = \triangle\widetilde{\alpha}_{mt} + \triangle\widetilde{\alpha}_{ct} + \triangle\widetilde{\tau}_t \mathrm{DVR}_i + \Delta\widetilde{\epsilon}_{imct},\ t = 2005,\ 2006,\ k = 1, 2 \tag{2}$$

where $\triangle$ is the difference operator (i.e. $\triangle Y_{imct} = Y_{imc,post} - Y_{imc,pre}$). In our analysis below, we will study both the difference between 2005 and 2004, $\triangle Y_{imc,2005} = Y_{imc,2005} - Y_{imc,2004}$, and the difference between 2006 and 2005, $\triangle Y_{imc,2006} = Y_{imc,2006} - Y_{imc,2005}$. Our main parameter of interest, $\triangle\widetilde{\tau}$, captures the difference in differences in sales between the DVR and non-DVR groups for 2005 and 2006 respectively. The total difference in differences between 2004 to 2006 can be computed as $\triangle\widetilde{\tau}_{2005} + \triangle\widetilde{\tau}_{2006}$. The $\triangle\widetilde{\alpha}_{mt}$ capture the market-m-specific trend in expenditures, and the $\triangle\widetilde{\alpha}_{ct}$ capture the category-c-specific trend in expenditures. The latter term ensures that our estimates of the average DVR treatment effect, $\triangle\widetilde{\tau}$, is robust to time-varying demand shocks that are common across groups. The first difference equation (2) can be rewritten more succinctly as $\triangle Y_{imct} = \alpha_{mt} + \alpha_{ct} + \tau_t \mathrm{DVR}_i + \epsilon_{imct}$and this forms the basis of our estimation equation and the parameters reported in the ensuing tables.

Note that the model 2 "differences out" the nuisance parameters, α_{icm}. While these parameters are not estimated, they are nevertheless implied by the model. In light of this, differencing accomplishes two goals. First, it removes any persistent household-specific effects from the data that, if ignored, could introduce endogeneity bias due to the self-selection of a household into the DVR treatment condition. As we showed in section 4.1, differencing restores the equality of the treatment and control groups. Second, first-differencing corrects the standard errors for the heteroskedasticity associated with potential heterogeneity. More importantly, the model 2 implicitly controls for household-specific heterogeneity in expenditure behavior in each category, α_{icm}.

We report the regression results in Table 4, omitting the category and market fixed effects to conserve space. First, we look at changes in consumer spending between 2005 versus 2004. Second, we look at 2006 versus 2004, to allow for a potential "learning period" with the new technology. Results are reported for differences in expenditures on advertised goods and for differences in expenditures on private label brands. In each case, we normalize the data to a dollar per week basis (e.g., average dollar sales per week). We are unable to detect any statistically significant DVR treatment effect. That is, on average, we do not detect a significantly different change in expenditures over time for households that adopt a DVR versus households that do not.

While statistical insignificance alone is not conclusive of a "no TiVo effect," it is striking that the point estimates are also tightly distributed around zero. If we account for uncertainty, our 95% confidence region of the DVR effect on advertised goods expenditures lies roughly between -1 and

0.7 cents. Referring back to Table 1, the average weekly expenditures on advertised goods, across categories, in 2005 and 2006 is 28.7 cents. Thus, the average treatment effect of a DVR on a given household's advertised good expenditures is (in absolute value) within the range of -3.5% to 2.4% of expenditures. Given that only 11.4% of our sample households adopted the DVR, our results imply that total expenditures in a given category would be influenced by roughly between -0.4% and 0.3%, amounts which are economically small and of little managerial significance. The data appear to have sufficient power to conclude that the TiVo effect is not only statistically insignificant, it may in fact be marginally different from zero. Our results are similar for private label expenditures. While we find an insignificant average DVR treatment effect, the 95% confidence region lies between -0.2 cents and 0.3 cents. Thus our data support a DVR effect on a treated household of between -5% and 7% and, hence, an effect on total private label expenditures of roughly between -0.6% and 0.8%.

	2005			2006		
Model	Coeff.	t	95% Conf. Int.	Coeff.	t	95% Conf. Int.
Advertised Brand Sales						
TiVo Effect, τ	-0.0016	-0.37	(-0.010, 0.0070)	0.0045	1.01	(-0.004, 0.013)
Model Fit		$R^2 = 0.011$			$R^2 = 0.012$	
Private Label Sales						
TiVo Effect, τ	0.003	0.26	(-0.0021, 0.0027)	0.0004	0.29	(-0.0022, 0.0030)
Model Fit		$R^2 = 0.007$			$R^2 = 0.003$	
Sample Size		42,857			41,920	

Table 4: Difference-in-Differences Regressions

4.3 Results for DVR Selection Model

In this section, we check the robustness of the results from our baseline first-differences regression to any potential remaining selection on unobservables arising through correlation between the treatment variables, DVR_i, and the innovation to expenditures, $\Delta\epsilon_{imc}$. A concern is that the insignificance (both statistically and economically) of the DVR effects in Table 4 may be due to selection bias. For instance, if the growth in expenditures for households that adopt DVRs is systematically higher than for households that do not, the net effect of a DVR could appear to be zero, even if the true treatment effect is negative. Following the convention in the treatment effects literature, we cast our estimation problem as a linear latent index model (c.f. Heckman and Robb 1985). The participation decision of household i into the DVR program is modeled as the latent index

$$D_i^* = \beta + Z_i\gamma + \eta_i$$

where Z_i is a vector of household-specific characteristics and $cov(\eta, \Delta\epsilon) \equiv \sigma$. One can think of this participation index as capturing the household's expected net present value of utility from accepting the DVR offer. The observed treatment indicator, DVR_i, is related to this index by:

$$\text{DVR}_i = \begin{cases} 1, & if\ D_i^* > 0 \\ 0, & if\ D_i^* \leq 0. \end{cases} \tag{3}$$

The main effects of the variables Z_i on Y_i are automatically differenced-out of our estimation model (2). Note that such time-invariant household characteristics would be implicitly subsumed into the household fixed-effects α_i. Thus, Z_i act as exogenous instruments for DVR_i. Note that we are also implicitly assuming that Z_i are uncorrelated with $\Delta\epsilon$, the unobserved component of a household's *change* in expenditures over time. Estimation is carried out using a two step approach (Heckman 1978).

We begin with the results from the first-stage DVR adoption model, (3). Our instruments, Z_h, consist of technology ownership and household demographics (age, education and income): $Z_i = [Tech_i, Age_i, Edu_i, Inc_i]$. Given the pre-treatment equality of the distributions of differences across groups coupled with our use of time-invariant instruments, this is a reasonable assumption. Results are reported in Table 5 under the heading "Selection Model." There appears to be systematic differences in selection of TiVos, with older and more educated persons more likely to accept the offer of a free TiVo – as well as those that are prone to adopt other types of technology. The instruments provide a strong correlation with DVR adoption. That is, a simple OLS regression of these variables on DVR adoption yields a squared correlation of 0.13.

	2005			2006		
Model	Coeff.	t	95% Conf. Int.	Coeff.	t	95% Conf. Int.
Advertised Brand Sales						
TiVo Effect, τ	0.018	1.50	(-0.005, 0.041)	-0.031	-2.61	(-0.056, -0.008)
Sel. Model Cov., σ	-0.013	1.77	(-0.028, 0.001)	0.023	2.95	(0.008, 0.038)
Model Fit	$\chi^2_{45} = 434.58$, $p < 0.0000$			$\chi^2_{45} = 570.72$, $p < 0.0000$		
Private Label Sales						
TiVo Effect, τ	0.002	0.67	(-0.004, 0.008)	0.004	1.17	(-0.003, 0.011)
Sel. Model Cov., σ	-0.001	-0.61	(-0.005, 0.003)	-0.003	-1.23	(-0.007, 0.004)
Model Fit	$\chi^2_{49} = 295.19$, $p < 0.0000$			$\chi^2_{49} = 706.24$, $p < 0.0000$		
Selection Model						
Technical Index, γ_{Tech}	2.831	49.2	(2.718, 2.944)	2.846	49.1	(2.733, 2.960)
Age, γ_{Age}	-0.121	-19.8	(-0.133, -0.109)	-0.109	-17.6	(-0.121, -0.097)
Education, γ_{Edu}	0.058	12.2	(0.048, 0.067)	0.060	11.5	(0.046, 0.064)
Income, γ_{Inc}	-0.002	-0.08	(-0.006, 0.005)	-0.001	-0.49	(-0.007, 0.004)
Model Fit	$\chi^2_4 = 5854.25$, $p < 0.0000$			$\chi^2_3 = 5502.13$, $p < 0.0000$		
Sample Size	39,290			38,423		

Table 5: TiVo Treatment Effects

Next we report the results from our second-stage difference model defined in equations. These are reported in Table 5. An asterisk by a parameter denotes that it is significant at the 5% level. We omit the fixed market and category effects to conserve space. Beginning with the estimated covariance parameter between the selection model and the outcome equation regressions, σ, we find mild evidence of selection. For the change in expenditures on advertised goods, the covariance term is positive and significant in 2006 (albeit small – the correlation coefficient is 0.056). This covariance implies that those who select TiVos are more likely to have greater increases in advertised brand purchases – perhaps reflecting greater increases in income. The 2005 estimate is significant only at the 10% level. We find no evidence of such selection in our analysis of the change in expenditures on private label goods.

We now turn to our estimates of the DVR effect on expenditures. Given the general lack of evidence of selection, it is not surprising to see that our point estimates for the DVR effect on the change in private label expenditures are very similar to the regression results in Table 4, albeit noisier. The effect of DVRs on the change in expenditures on advertised is more interesting. As in our baseline case, the effect in 2005 is insignificant and noisier. However, we find a statistically significant effect of DVRs in 2006. As one would expect under the conventional wisdom that DVRs reduce advertising exposures, the sign of the DVR treatment effect is negative. Thus, the expenditures on advertised goods increased in 2006 over 2004 by a lower increment for treated households by an amount of roughly 3 cents. Note that 3 cents is nearly 10% of the average 2006 expenditure level on advertised goods of 29 cents. However, this effect is largely offset by the predicted decrease of 1.8 cents in 2005. The combined 2005 and 2006 DVR effect is not significant ($p < 0.05$) implying one can not reject the null of no net difference in sales between 2006 and 2004. In short, even after controlling for selection, we are unable to detect a statistically significant effect of DVRs on expenditures.

4.4 Heterogeneity in the Treatment Effect

Given the small and insignificant effects reported thus far, we next assess whether heterogeneity in the treatment effect might be a factor. In particular, we check whether there is a different non-zero treatment effect for those households who use the DVR technology the most intensely. We re-examine the effect of DVRs on changes in private label sales and changes in the leading advertised brands sales while including interactions with measures of a household's ad-skipping behavior.

To ascertain the role of DVR usage on our dependent measures, we specify and estimate the following regression

$$\triangle Y_{icm} = \alpha_m + \alpha_c + \beta_1 (FF_i / KS_i) + \Delta \epsilon_{icm} \tag{4}$$

where KS_i is the number of keystrokes and FF_i is the number of fast forwards. As skipping behavior is only observed for households that accept the DVR offer, we only use the treated households for estimation. Hence, the DVR effect is subsumed into the market and category fixed-effects in equation 4. Table 6 reports the results, once more omitting the category and market fixed effects from the table to conserve space. We fail to detect a statistically significant effect of fast forward behavior on purchase behavior for the TiVo households. Thus, we do not find any evidence of systematic differences in shopping behavior based on the intensity of DVR usage.

	% Fast Forward Coefficient	t	N	R^2
Change Private Label Expenditure	-0.0003	1.21	10,749	0.004
Change Leading Advertised Brands Expenditure	0.0010	1.12	10,749	0.015

Table 6: Moderating Effect of Fast Forward Behavior

4.5 DVRs and New Products

As a final robustness check, we investigate whether there is a different DVR effect on expenditures for recently-launched products. In Lodish et al. (1995), advertising effects are found to be largest and most prevalent among newly-launched brands. In Table 7, we present the results from both the OLS and 2-step Treatment Effects models, omitting fixed category and market effects to conserve space.[6] Even in the case of expenditures on new brands, we find the DVR effect to be insignificant. However, for new brands, we lack sufficient power in the data to assess whether there is indeed no DVR effect. Although not reported, the OLS 95% confidence interval on the DVR effect lies between -5 cents and 1 cent. Given that average weekly expenditures for new brands are, on average, only about 15 cents in 2006, we cannot rule out that the DVR effect might be as large as 33% in absolute value. Moreover, the sign of the DVR effect is consistent with what one might expect regarding the role of TiVo on new brand sales. In this instance, it is possible that with a larger sample of consumers and/or more categories with new product launches, one might be able to estimate a DVR effect that is both statistically and economically significant.

	Selection Model		OLS	
Model	Coefficient	t	Coefficient	t
Difference New Brand Sales				
TiVo Effect, τ	-0.059	-1.08	-0.019	-1.13
Selection Model Covariance, $\sigma_{\triangle}$	0.026	0.73		
Model Fit	$\chi^2_{30} = 84.79$, $p < 0.0000$		$R^2 = 0.06$	
Selection Model				
Technical Index, γ_{Tech}	2.640*	9.19*		
Age, γ_{Age}	-0.058*	-1.89		
Education, γ_{Edu}	0.051*	2.02*		
Income, γ_{Inc}	-0.007	-0.44		
Model Fit	$\chi^2_4 = 154.95$, $p < 0.0000$			
Sample Size	1,427		1,559	

Table 7: TiVo Treatment Effects for New Products (2006)

4.6 Advertisement Skipping

Our analysis in sections 4.2 to 4.5 has consistently found statistically insignificant TiVo effects on expenditures that are tightly centered around zero. The tight inference around zero suggests that

[6]The sample sizes differ between the OLS and selection models because there exist some missing values for demographic variables in the selection model.

our data do have the statistical power to infer that TiVo ownership has limited impact on CPG shopping behavior. We now examine the TiVo log files to document usage behavior that supports these findings.

As documented by Nielsen Media Research (NMR), most households only skip a small fraction of the advertisements they see on television. First, most television is still watched live. Second, contrary to self-reports, most households do not skip all the advertisements they see on recorded television. To explore this point further, we used the advertising exposure data and TiVo log files to form a preliminary assessment of the degree of advertising skipping made by the TiVo panelists. For this analysis, we broke the advertising viewership into a series of conditional decisions, graphically depicted in Figure 1.

1. Watch. We first observe whether a household watches any portion of a show during which the advertisement was broadcast. If all households watched each show in which our 2661 advertisements were aired once, we would observe 4,036,592 shows viewed. In our data, we in fact observe 70,839, total actual shows watched, or 1.7% of these potential views.

2. Record. In general, shows must be recorded in order for an advertisement to be skipped. The log files we obtained explicitly denote a recorded viewing. As a caveat, it is possible to watch non-recorded shows to the extent live viewing is paused and viewed in a phase delayed fashion without being explicitly recorded. More recently, DVR log files account for near live views as recorded, but our data do not enable us to determine this explicitly. We find that 3,486 of the 70,389 shows watched (5%) were viewed after being recorded. This statistic is important because it begins to suggest that fast forwarding might not be as endemic as advertisers expect or as reported in previous surveys.

3. Expose. Even though a show is watched, a household might channel zap during advertisements leading to no exposure. We infer an exposure whenever the advertisement appears after a person began watching a show in which the advertisement was embedded and either (1) the person watched until the end of the show or (2) a person tuned to another show prior to the advertisement. We find that 94% of recorded shows led to an advertising exposure and 65% of live or near live shows led to an exposure. This is likely the result of increased channel surfing in the context of live viewership.

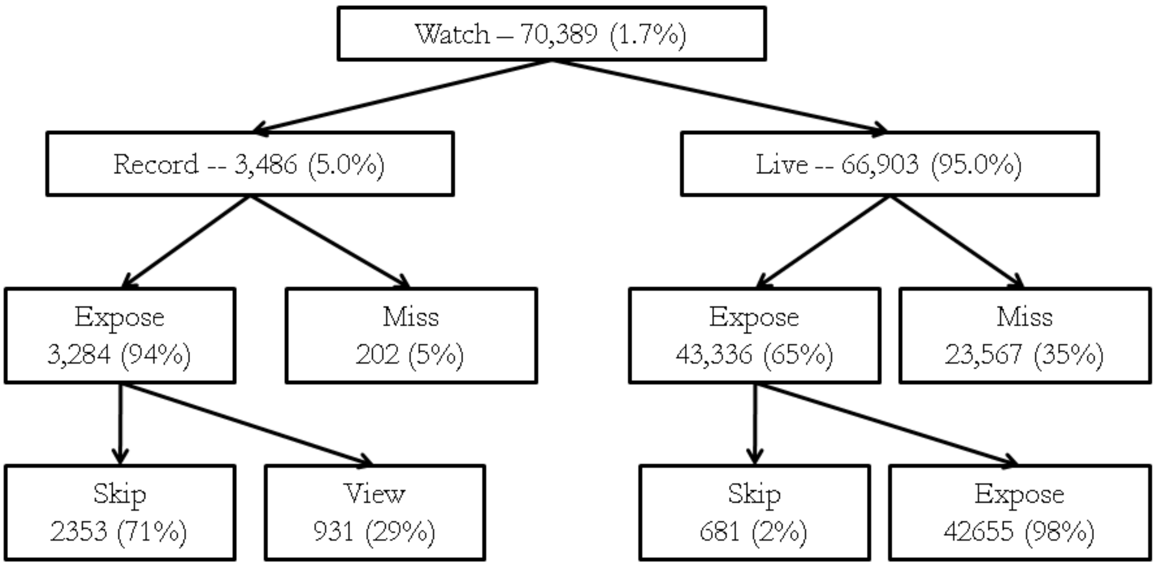

Figure 1: Fast Forward Behavior

4. Skip. We infer a skip of an advertisement if we observe a fast forward during the interval in which an advertisement is aired. Given many advertisements are only 15 seconds, it is especially critical to audit advertisement placements. According to IRI, advertisements are often shifted within and across pods relative to the published broadcast schedules provided by the network, hence such data are of limited value in inferring skipping behavior. We find that 2,353 advertisements are forwarded in the recorded condition (71%) and 681 are forwarded in the near live condition (2%). The 71% statistic is remarkably close to the 68% recorded by Pearson and Barwise, and thus show high face validity.

Figure 1 summarizes the skipping behavior discussed above. From this figure, we observe that, of the 46,620 total exposures, only 3,034 advertisements are fast forwarded (6.5%), well short of the 47% self-reported skipping rate in the Jupiter survey. The relatively low skipping rates arise because one must record or pause a show to skip, and the intensity of this type of behavior is somewhat limited. Given that our data are comprised primarily of cleaning and grooming products, the coverage of advertisements across genres is limited. For example, these products are rarely advertised in sporting events. To the extent that sporting events are more often watched live, we expect even fewer advertising skips.

5 Conclusions

In summary, we are unable to detect statistical evidence of a TiVo effect on CPG purchase behavior across a variety of measures including demand for large advertised goods, private labels and new brands. Even for households with the highest TiVo usage, we find no effects. The fact that most of our point estimates are economically small with fairly tight confidence intervals around zero suggests that there may not be a TiVo effect on CPG shopping behavior. These findings suggest that, contrary to conventional wisdom, DVRs may not present a threat to network advertising in the short run or medium (2 years) run.

There are several possible explanations for the lack of a TiVo effect. It is possible that television advertising does not have an effect on sales irrespective of TiVo. For instance, households may be skipping most advertisements even without a DVR, either by channel surfing (Zufryden et al. 1993) or by diverting their attention during advertising breaks. It is also possible that the actual marginal effect of advertising is already too small for TiVo to have much impact. Previous research (e.g. Lodish et al 1995) also documents small network advertising effects. Although our field study cannot test the effectiveness of advertising on sales (because we do not explicitly observe advertising exposures for non-TiVo households), perhaps the most interesting direction for future research is not to test whether TiVo impacts advertising, but whether advertising impacts sales in the first place. Since DVRs distribute television digitally to households, it should become increasingly easy to use this technology to conduct extensive advertising experiments.

Other explanations for the lack of a TiVo exist. For example, a negligible TiVo effect may arise from the means by which households avoid advertisements when using a DVR. Fast forwarding requires that households attend to the advertising pod as they fast forward through it, thereby entailing a modicum of brand exposure which would tend to partially attenuate the adverse consequences of advertisement avoidance. We find some evidence of this behavior in our data. Also, to skip advertisements, one must first record shows. To the extent recorded viewings are limited, the opportunities to forward are also curtailed. Another explanation for a limited effect of DVRs is that recording leads to an overall increase in viewership via increased recording. Given that a portion of recorded advertisements are viewed, the overall reduction in advertising exposure rates might not be as great as traditionally conceived. Relatedly, recorded shows can be watched repeatedly, thus increasing advertising exposures.

We view this research to be a first step toward assessing the role of TiVo on the efficacy of television advertising. A number of open issues remain. First, our analysis is a field study, not an experiment and is prone to self-selection issues. We use first-differencing to control for potential sources of endogeneity due to correlation between TiVo adoption and persistent unobserved differences between households shopping. We also construct an instrument to control for any additional endogeneity due to correlation between TiVo adoption and differences in the evolution of unobserved shopping behavior. Ideally, future work may try to run a field experiment to obtain cleaner data that does not require econometric methods to tease out the treatment effect.

A second potential limitation is that we have only two years of post-TiVo treatment data. This may be an insufficient duration for persons to learn TiVo use or for brand images to be adversely affected by a decrease in advertising. However, we offer evidence that our panel is not too discrepant from a national panel of TiVo households and a separate analysis available from the authors decomposes the post-TiVo treatment data into two consecutive nine month periods and finds little difference between these periods. Third, our analysis is limited to packaged goods and we can not make definitive conclusions about the role of TiVo in other categories.

DVR log files enable several new research directions. including the ability to track actual advertising and television show viewing. Among other things, this is useful for understanding how DVRs can be used to target advertisements (i.e., contextual advertising) more effectively and how such advertisements should be priced. DVR data might also be useful for explaining the advertising and show timing and placement decisions on the part of advertisers and networks. Finally, the ability to match television and advertising viewing with purchases could permit a better analysis of the welfare implications of advertising. We hope this paper helps to lay the groundwork for this future research.

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