



## Do DVRs Influence Sales?

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## *ABSTRACT*

The authors analyze a multimillion dollar, three-year field study sponsored by five firms to assess whether enabling skipping of advertisements using Digital Video Recorders (DVRs) impacts consumers' shopping behavior for advertised and private label goods. A large sample of households received an offer for a free DVR and service and close to 20% accepted. Each household's shopping history is observed for 48 consumer packaged goods categories during the 13 months prior and the 26 months following the DVR offer. The authors fail to reject the null of no DVR treatment effect on household spending on advertised branded or private label goods, either 1 or 2 years after the DVRs are shipped. The predicted DVR effect is tightly centered around 0, suggesting the data may have sufficient power to identify a true null effect. Using advertising exposure information for seven of the brands in the study, the authors offer suggestive evidence that ad-skipping occurs for a relatively small fraction of the total television content viewed. Other potential explanations for the lack of a DVR effect are also discussed.

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

## OVERVIEW

TiVo pioneered the new Digital Video Recorder (DVR) devices used to record and playback television content. Debuted in March of 1999, the DVR<sup>1</sup> market has grown quickly. 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. According to Jupiter Media, 19% of U.S. households with televisions had DVRs by 2007. Enders Analysis similarly forecasted DVR penetration in the UK to increase to a remarkable 80% by 2012 (Andrews 2008). In response to the diffusion of DVRs, Forrester predicted in 2004 that households would be watching 15% fewer commercials by 2007 (Economist 2004). In fact, one Jupiter Media report found that 47% of surveyed DVR users indicated skipping commercials “most of the time.” *Crain’s New York Business* recently declared that the television advertising “industry is in deep doo doo,” citing a precipitous decline in advertising viewing as a result of DVRs (Block 2008) while the *Wall Street Journal* declared “traditional TV advertising is losing luster as viewers get savvier about skipping commercials (Worden 2009).”

Advertisers have been scrambling to respond to these changes. 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)<sup>2</sup> 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<sup>3</sup>; Nielsen Media Research 2008<sup>4</sup>). 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. We begin with the conventional wisdom of the consumer goods industry and network television 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 four of IRI's Behaviorscan sample markets were offered a free DVR and subscription to TiVo. 1588 households accepted the offer, a number sufficiently small to offset any practical concerns about competitive reactions in the market place by CPG manufacturers or retailers (i.e. we do not expect adjustments to 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 because 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 7 of the brands in our sample during the post-treatment period. For these 7 brands, we can determine the frequency with which households were exposed to advertisements and the frequency with which, conditional on exposure, households skipped ads.

We focus on two household outcome variables. 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. Akerberg 2001). Once again, we would expect to see expenditures on new products decline under the null of a moderating effect of TiVo.

Surprisingly, we find no statistical evidence for a TiVo effect on purchase behavior during the year following the issuance of DVRs. Our difference-in-differences estimate of the TiVo treatment effect is found to be statistically insignificant for all outcome variables. This finding is robust to controls for self-selection based on time-varying unobservables and to self-selection based on time-varying observables. While we are unable to test for a specific cause of the lack of a DVR effect, we provide evidence that households watch relatively little recorded television content and that, conditional on watching recorded content, exhibit modest skipping levels.

The remainder of the paper is organized as follows. In the next section we describe the nature of the DVR market and its immediate implications for a household's ability to skip advertisements. Then we describe the design and implementation of the field study after which we outline our analysis and report our findings. Next, we overview potential reasons for the lack of a TiVo effect and provide some evidence that the actual level of advertisement skipping is lower than that reported in many self-report surveys. Finally, we conclude.

### *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) conditions. 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.<sup>5</sup> 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, 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 comprises 819 TiVo treated households and 4,059 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 next.

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## *DATA DESCRIPTION*

As noted in the preceding section, 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 summarize the use of each data set in Table 1, with a more detailed discussion in the sub-sections below.

[Insert Table 1 About Here]

### *IRI Behavior Scan 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,  $PL_{mci}$ .<sup>6,7</sup> These measures are then time-aggregated, by household and category, into 3 periods. The first period consists of the 55-week pre-treatment period,  $(S3_{mci}^0, PL_{mci}^0)$ . The second period consists of the subsequent 56-week short-run post-treatment period,  $(S3_{mci}^1, PL_{mci}^1)$ . The third and final period consists of

the subsequent 56-week long-run post-treatment period,  $(S3_{mci}^2, PL_{mci}^2)$ . 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). We exclude observations for a household-category pair if the household never purchases in the category during the entire 39 month sample period.

We also measure a household's expenditures on new products  $(NP_{mci}^1, NP_{mci}^2)$ , as designated by 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. Table 2 reports summary statistics for all of these measures.

[Insert Table 2 About Here]

### *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.

### *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 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 household and the specific time that a fast forward occurred into a show as well as its duration.

For each household, we construct the following two variables summarizing the usage of the DVR: 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. To determine the relative degree of fast forward behavior, we compute the

ratio of fast forwards to total keystrokes,  $\frac{FF_i}{KS_i}$ . 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 brand advertising data subsection below, we discuss the 7 brands for which we observe a complete advertising schedule and, hence, for which we can assess actual household-level advertisement exposures. Merging the fast-forwards with these exposures allows us to infer the degree of actual advertising-skipping by panelists for these 7 brands.

#### *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. We assume that these historic adoption decisions are independent of current unobserved innovations to the household expenditure variables defined above. 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 the first two analysis subsections of this paper. In addition, a separate survey was used to assess household demographics. Statistics for these data are also presented in these sections.

#### *Brand Advertising Broadcast Data*

In theory, the treatment effect of TiVo on brand buying behavior is based on the extent to which 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. Unfortunately, TiVo log files do not provide information on actual advertisement-skipping as they do not track the specific advertisements broadcast during a show.

We supplement the TiVo log file data with the network advertising schedule for nine advertising campaigns across seven 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 eight 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 eight 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 seven 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 eight 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.

#### *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. For example, Arkes and Blumer (1985) find that users who pay nothing tend to underuse a product. 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 separately the effect of TiVo on expenditures in the short run (first 12 months) and long run (second 12 months).

### *ANALYSIS*

In this section, we report the findings from our analysis. The identification of the average treatment effect of DVRs on a household's spending behavior is complicated by the lack of a randomized assignment of subjects into treatment and control conditions. To offset the endogeneity associated with self-selection of our sample into the TiVo treatment group, we resort to several quasi-experimental approaches. Our baseline case consists of exploiting the panel structure of our data to obtain a difference-in-differences estimate of the average treatment effect. 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. Several robustness

checks are conducted to control for heterogeneity in the treatment effect both across consumers and across product categories. In addition, we check the robustness of our baseline difference-in-differences estimates to any remaining, time-varying sources of self-selection into the treatment group. In general, we 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. Table 3 overviews our analytical strategy in the remainder of this section:

[Insert Table 3 About Here]

#### *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.

In Table 4, 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.

[Insert Table 4 About Here]

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 48 categories. Recall from Table 2 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 5 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 levels on both highly advertised brands and on private labels. The finding of different distributions for treatment and control households respectively suggests that we cannot infer the treatment effect of DVRs by comparing expenditure levels across the two groups. But, by exploiting the panel structure of the data and differencing each panelist's expenditures over time, we 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 the null of equal distributions for the cross-time differences in expenditure levels, even with our relatively large sample size. Therefore, for the remainder of our TiVo analysis, we will work with the first differences in expenditures as our dependent measure.

[Insert Table 5 About Here]

*Results for the Baseline Expenditure Model*

We briefly outline our estimation scheme for the DVR treatment effect. We index the households by  $i = 1, \dots, I$ , categories by  $c = 1, \dots, C$ , the markets by  $m = 1, \dots, 3$ , 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:

$$(1) \quad Y_{imct} = \alpha_{imc} + \alpha_{mt} + \alpha_{ct} + \gamma \text{DVR}_i + \tau_t \text{DVR}_i + \varepsilon_{imct}$$

where  $\text{DVR}_i$  indicates whether household  $i$  is in the DVR treatment group. The parameter  $\tau_{2004} = 0$  by design of the field study. In equation (1),  $\gamma$  measures how the treated sample differs from the untreated sample, and  $\tau$  measures the treatment effect during years  $t = 2005, 2006$  of receiving a DVR unit at the start of 2005. As no DVR is distributed in the pre-treatment period it follows that  $\tau_{2004} = 0$ . In the model 1, the DVR effect is assumed to be constant across categories.

To obtain a consistent estimate of  $\tau$  that controls for household heterogeneity, we “difference out” the household-specific intercepts,  $\alpha_{imc}$ . That is, we run the following two regressions in first-differences to estimate the DVR treatment effect

$$(2) \quad \begin{aligned} \Delta Y_{imc,2005} &= \Delta \alpha_{m,2005} + \Delta \alpha_{c,2005} + \tau_{2005} \text{DVR}_i + \Delta \varepsilon_{imc,2005} \\ \Delta Y_{imc,2006} &= \Delta \alpha_{m,2006} + \Delta \alpha_{c,2006} + \Delta \tau_{2006} \text{DVR}_i + \Delta \varepsilon_{imc,2006} \end{aligned}$$

where  $\Delta$  is the difference operator across adjacent years. In our analysis below, we will study both the difference between 2005 and 2004,  $\Delta Y_{imc,2005} = Y_{imc,2005} - Y_{imc,2004}$  and the difference between 2006 and 2005,  $\Delta Y_{imc,2006} = Y_{imc,2006} - Y_{imc,2005}$ . The parameter  $\tau_{2005}$  captures the DVR treatment effect on sales in 2005 whereas the parameter  $\Delta \tau_{2006}$  captures the change in the DVR treatment between from 2005 to 2006. The latter parameter enables us to test for potential “learning” whereby the usage of DVRs evolves over time. Trends in expenditures are captured by  $\Delta \alpha_{mt}$  for market  $m$ , and  $\Delta \alpha_{ct}$  for category  $c$ . The latter term ensures that our estimates of the

average DVR treatment effects are robust to time-varying demand shocks that are common across groups.

The model in (2) “differences out” the nuisance parameters,  $\alpha_{icm}$ . 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 validating the control subsection, differencing restores the equality of the treatment and control groups. Second, first-differencing corrects the standard errors for the heteroskedasticity associated with potential heterogeneity.

We report the regression results in Table 6, omitting the category and market fixed effects to conserve space. In addition to reporting the treatment effect from 2005 versus 2004,  $\tau_{2005}$ , we also report the change in the treatment effect between 2005 and 2006,  $\Delta\tau_{2006}$ . 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). The results indicate a statistically insignificant TiVo treatment effect on sales, in 2005, and a statistically insignificant change in the effect of TiVo on sales between 2005 and 2006. That is, even two years after receiving a DVR and learning how to use it, we do not detect a statistically significant difference between the change in sales of treated versus un-treated households. Note that if we compute the 2006 treatment effect,  $\tau_{2005} + \Delta\tau_{2006}$ , we have a small and insignificant total effect for both advertised goods and private labels.<sup>8</sup>

[Insert Table 6 About Here]

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 2005 DVR effect lies roughly between -0.6 and 0.6 cents for both advertised goods expenditures and private label goods expenditures. Referring back to Table 2, the average weekly expenditures on advertised goods, across categories, in 2005 is roughly 25 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 -2.5% to 2.5% of expenditures. Given that only 17% of our sample households adopted the DVR (i.e. 819 of 4,878 households), our results imply that total expenditures in a given category would be influenced by roughly between -0.4% and 0.4%, amounts which are economically small and of little managerial significance. Similarly, the average weekly expenditures on private labels is roughly 3 cents and, hence, the total DVR treatment effect on total private label expenditures is roughly between -3 and 3%. Finally, we observe a very small confidence interval for the changes in the TiVo effect when we look at 2006. 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.

#### *Observed Household 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. Since our estimation sample consists of a single cross-section of expenditure differences for each household, we cannot identify unobserved heterogeneity in the treatment effect. Instead, we explore two forms of observed heterogeneity. First, we look at whether the TiVo effect varies with the degree of fast-forwarding, a measure of the intensity of usage of the technology. Second, we look at whether the TiVo effect varies with observable household characteristics.

To ascertain the role of fast forward behavior on our dependent measures, we specify and estimate the following regression

$$(3) \quad \Delta Y_{itmc} = \alpha_m + \alpha_c + \beta_1 \left( \frac{FF_i}{KS_i} \right) + \Delta \delta_{itmc}$$

where  $KS_i$  is the number of keystrokes and  $FF_i$  is the number of fast forwards (we also consider key strokes as a measure of DVR usage and obtain similar insights). 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 3.

Table 7 reports the results for the fast-forward effect,  $\beta_1$ , on expenditures for advertised brands and private labels in 2005 and 2006. As before, we omit 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.

[Insert Table 7 About Here]

We next estimate a model in which we interact the TiVo effect with various household demographic variables to check for heterogeneity in the treatment. In particular, we interact the TiVo treatment effect with a household's technology ownership index, a household's income, and a primary household shopper's education and age. Table 8 presents the estimates of the TiVo treatment effect,  $\tau$ , and the interactions with household characteristics.

[Insert Table 8 About Here]

As indicated in Table 8, the TiVo treatment effect and interactions are found to be small and statistically insignificant. The change in the treatment effect between 2005 and 2006 is found to be insignificant for private labels and marginally significant for advertised goods amongst the highest-income households. Note that the 2006 TiVo treatment effect,  $\tau_{2005} + \Delta\tau_{2006}$ , is small and insignificant for both advertised goods and private labels.

### *DVRs and New Products*

In a large-scale set of split-sample television advertising field experiments, Lodish et al. (1995) found the largest advertising effects among recently-launched brands. Recently-launched branded goods should constitute an ideal context in which to test for a DVR treatment effect on product trials. Unfortunately, this test would constitute an inherently between-subjects comparison of trial levels and, hence, the treatment effect of a DVR in this context is not identified. Hence, we use our difference-in-differences estimator to recover the change in treatment effect of a DVR on the change in new brand sales from 2005 to 2006. A change in treatment effect implies the DVR's effect on new brand sales differs between the first year of brand launch and the second. This might arise if advertising effects are greater in the first year of a brand's lifecycle than the second.<sup>9</sup>

Table 9 reports the change in treatment effect for newly-launched brands, omitting fixed category and market effects to conserve space. As in the previous sections, the change in DVR treatment effect is statistically insignificant. However, due to the smaller sample of new brands, our estimate of the change in treatment effect is less precise. The OLS 95% confidence interval ranges from -1 cents to 4 cents. Given that average weekly expenditures for new brands are, on average, only about 15 cents in 2006, we cannot rule out that the change in DVR treatment effect might be as large as 27% in absolute value.

[Insert Table 9 About Here]

### *Heterogeneity in the Treatment Effect Across Categories*

We also look at the distribution of TiVo treatment effects on a category-by-category basis to explore the sensitivity of our findings to any potential aggregation bias across categories. We estimate the same difference-in-differences regressions as specified above, but estimate them separately for each category. Consistent with the pooled regressions, the category-specific TiVo

treatment effects for expenditure on advertised brands are found to be insignificant in all but one category in 2005, and the change in treatment effect is not significant for all but two categories in 2006. Similarly, category-specific TiVo treatment effects for the private label expenditures are found to be significant at 5% in only three categories in 2005, and in none of the categories in 2006. We would expect similar outcomes by chance. Hence, we conclude that our findings from the pooled analysis extend to the category-by-category regressions.

#### *Results for DVR Selection Model*

In this subsection, we check the robustness of the results from our baseline first-differences regression, in baseline expenditure model subsection, to any potential remaining biases from self-selection into the treatment group. The difference-in-differences estimator removes any self-selection associated with a persistent unobserved household effect. In fact, in that subsection, we established that the distribution of expenditures for treated households becomes statistically indistinguishable from the distribution for untreated households after taking first differences. However, we still want to control for the possibility that self-selection might arise due to time-varying unobserved household effects. 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. We control for this form of self-selection in two ways: selection on observables and selection on unobservables.

*Selection on observables.* We first consider the case of selection on observables, whereby we assume that we can control for any time-varying sources of self-selection using observed household variables. We use propensity score matching (Rosenbaum and Rubin 1983). In the first stage, we estimate each household's probability, or "propensity," of receiving the TiVo treatment as a function of observed variables

$$\text{Prob}(\text{DVR}_i = 1) = \text{Prob}(S + Z_i\gamma + \eta_i \geq 0)$$

where  $Z_i$  is a vector of observed household-specific characteristics and  $\eta_i$  is distributed Type I extreme value. The latter assumption implies that the probability of treatment has the Logit formulation.

In the second stage, a matching estimator is constructed that matches households with similar propensities. In particular, we matched each treated household with the 5 most similar non-treated households in terms of propensity.<sup>10</sup>

Results for the first-stage Logit propensity score model are reported in Table 10. As expected, we find a positive and significant effect of technology ownership and female household head education on TiVo adoption. We also find a negative and significant effect of female household head age on adoption.

[Insert Table 10 About Here]

The propensity score estimator was applied to household-level differences in expenditures for a design of 48 (categories) by 2 (variables: expenditures on advertised goods, expenditures on private labels) by 2 (years: 2005, 2006). As above for the category-specific difference-in-differences estimator, the TiVo treatment effect is found to be small and statistically insignificant in almost every category, for both variables and both time periods. Only 4 of the categories generate a statistically significant DVR treatment effect out of  $2 \times 48$  for expenditures on advertised goods. None of the categories generates a statistically significant treatment effect for expenditures on private labels. This small number of insignificant effects is consistent with chance.

*Selection on unobservables.* In general, it is unlikely to expect that our observed household characteristics will completely capture all forms of self-selection. To control for selection on unobservables, we follow the convention in the treatment effects literature and cast our estimation problem as a linear latent index model (c.f. Heckman and Robb 1985). We effectively

run an instrumental variables regression that controls for the binary nature of our potentially endogenous treatment variable. Note that none of our households literally has a zero probability of receiving the treatment (i.e. of self-selecting into the TiVo condition) and, hence, we can only identify a local average treatment effect after controlling for self-selection in this manner (e.g. Imbens and Angrist 1994).

As in the previous section, 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  $\text{cov}(\eta_i, \Delta \varepsilon_i) = \sigma$ . 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:

$$(4) \quad DVR_i = \begin{cases} 1 & \text{if } D_i^* > 0 \\ 0 & \text{if } D_i^* \leq 0 \end{cases}$$

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  $\alpha_i$ . Thus,  $Z_i$  act as exogenous instruments for  $DVR_i$ . Note that we are also implicitly assuming that  $Z_i$  are uncorrelated with  $\Delta \varepsilon_i$ , the unobserved component of a household's *change* in expenditures between a year  $t$  and a year  $t - 1$ . Thus  $Z_i$  acts as an instrument that contains exogenous variation in  $DVR_i$ . Estimation is carried out using a two step approach (Heckman 1979).

We begin with the results from the first-stage DVR adoption model, (4). Our instruments consist of technology ownership and household demographics (age, education and income):  $Z_i = [\text{Tech}_i, \text{Age}_i, \text{Edu}_i, \text{Inc}_i]$ . To assess the quality of these instruments, we use a test based on the

“relative bias” of a two-stage least squares estimator versus that of OLS. Cragg and Donald (1993) derive an F-statistic that characterizes this relative bias in terms of the power of the instruments themselves. One can then test whether this relative bias exceeds some threshold (e.g. 10%) and use the critical values provided in Stock and Yogo (2004). We report the results of this test for weak instruments in Table 11 and easily reject the hypothesis of weak instruments for several threshold values.

[Insert Table 11 About Here]

Results for the second stage outcome equation, corrected for selection, are computed for each category. As above, we summarize the estimations by reporting the number of categories in which we found a significant treatment effect. Beginning with the estimated covariance parameter between the selection model and the outcome equation regressions,  $\sigma$ , we find only mild evidence of selection. In most categories,  $\sigma$  is found to be small and in 90% of cases it is insignificant. Similarly, we find the estimates of  $\sigma$  to be small and, in most categories, insignificant for expenditures on private labels. Thus, we conclude that there appears to be little evidence for selection.

We now turn to our estimates of  $\tau$ , 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 and on the change in advertised goods expenditures are very similar to the regression results in baseline model subsection, albeit “noisier.” For private label good expenditures, only 3 categories generate a statistically significant treatment effect in 2005, and 2 categories in 2006. For advertised goods expenditures, only 6 categories generate a statistically significant treatment effect in 2005 and 4 categories in 2006. In short, even after

controlling for selection, we are unable to detect a statistically significant effect of DVRs on expenditures.

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## *DISCUSSION OF THE DVR NULL EFFECT*

In contrast to widespread trade press conjecture about the adverse effects of DVRs on sales and the belief by 70% of manufacturers that DVRs reduce or destroy the effectiveness of the television commercials, our analysis has consistently found no statistical support for a TiVo treatment effect on expenditures. In this section we discuss potential explanations for this outcome. These potential explanations include (1) a low overall rate of advertising skipping, (2) the potential that advertising has no effect on sales to begin with, meaning there would also be no adverse effect of DVRs on sales, (3) a positive effect of DVRs on television consumption that might in part offset its effects on advertisement skipping, (4) the facility with which non-DVR users can also avoid advertisements and (5) the fact that advertising forwards may still have some value in terms of advertising exposure. We discuss each point in turn.

### *Advertisement Skipping*

One potential explanation for the lack of a DVR effect is that advertisements are not skipped as often as is commonly believed. There are two factors that can influence the number of potential skips. First, an advertisement must be recorded to be forwarded. Second, conditioned on recording, a user must decide to skip.

In self-reports, ad-skipping behavior appears to be quite prevalent. An ABC network survey found that 71% of surveyed individuals self-reported fast forwarding through advertisements (Loughney 2007). In an ethnographic study of individuals observed during their viewing of recorded television, Pearson and Barwise (2007) reported fast forward rates of 68% during recorded television content. Similarly, Nielsen Media Research reported that DVR users skipped

only 60% of the commercials in a usage study spanning one week. In short, skip rates are roughly 60 to 70%.

Even with high skip rates, the opportunities to fast-forward advertisements will be limited if households do not watch a high proportion of recorded television content. Several prior studies document the relatively low viewing of recorded, as opposed to live, television content, with numbers ranging from 13.8% (Wearn 2007) to under 10% (Zigmond et al. 2009) of total viewing time. Coupling the 10% recording rate with a 70% forwarding rate implies that there is only a 7% reduction in total exposures as a result of DVRs. If so, the small reduction in advertising viewing would yield a correspondingly small DVR effect.

Using our advertising exposure data and TiVo log files, we measure skip rates and the recorded television viewing rates for our sample. 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 2,661 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.<sup>11</sup>

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.
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.

[Insert Figure 1 About Here]

Figure 1 summarizes the skipping behavior discussed above. Of the 46,620 total exposures, only 3,034 advertisements are fast-forwarded (6.5%), considerably lower than the 47% self-reported skipping rate in the Jupiter survey. Interestingly, the results are consistent with the 7% skipping rate reported in the DISH network data (Zigmond et al. 2009). It is possible that the low skipping rates contribute to the lack of a DVR effect.

#### *Other Literature*

The prior literature on DVRs exposts several additional reasons that might underpin our DVR null effect. First, the small magnitude of the marginal effect of advertising on sales may

require a very large sample to identify empirically. For instance, Lewis and Reiley (2009) estimate the effect of Internet display ads on sales using a sample of more than 1 million web users. However, we do find a positive correlation between changes in advertising and changes in sales for two of the seven brands for which we observe advertising campaign data.<sup>12</sup> This fraction is consistent with Assmus et al. (1981) and Lodish et al. (1995) who also find that one in three brands have positive advertising elasticities. To the extent our finding generalizes across brands, the lack of a DVR treatment effect may not merely reflect ineffective advertising.

Second and contrary to the expectations of industry experts, DVRs could, enhance the advertising effectiveness by allowing the viewer to match their favorite TV content with their leisure time, resulting in greater television consumption. According to our personal communications with IRI, total television viewership is 6% higher in DVR households than non-DVR households. Further, the chief researcher of CBS noted the following in November 2009:

“The best preseason estimate for [the lift in ad view ratings due to DVRs in] the current season ... was about a 1 percent increase from playback over the live program for the networks combined. Instead, many are in the range of 7 to 12 percent, with some shows having increases of more than 20 percent when DVR ratings are added. The four networks together are averaging a 10 percent increase. It’s the magnitude that’s really surprising us.” Carter (2009)

This increase in viewership could increase advertising exposures and further offset any potential adverse DVR effects. In our data, we observe a few instances where a household watches the same recorded content repeatedly, increasing potential ad exposures even further.

A third factor that might mitigate the DVR treatment effect is the facility with which non-DVR households can also avoid advertisements. For example, households can readily skip ads by

channel surfing (Zufryden et al. 1993). Zigmond et al. (2009) find that 15% of households with non-DVR set-top boxes channel-zap (i.e. switch channels). Non-DVR households can also avoid advertisements by leaving the room. Survey research by ABC finds that 48% of non-DVR users report leaving the room or ignoring the commercials (Loughney 2007). In contrast, only 13% of DVR users report leaving the room or ignoring commercials during recorded advertising breaks. These forms of advertising avoidance by non-DVR households could constitute another explanation for the lack of a DVR effect.

Finally, several studies have also shown that DVR users can be more attentive to advertisements when fast-forwarding through them (Mandese 2004, Du Plessis 2007, Siefert et al. 2008, Brasel and Gips 2008, and Goode 2007). For example, Mandese (2004) finds that two thirds of DVR viewers notice the advertisements they forward. Further, Brasel and Gips (2008) and Goode (2007) find advertisements are more effective in some cases when viewed at an accelerated rate. Recall that our previous discussion of the log file analysis of exposures on sales also showed no coincident effect of fast-forwards on sales. This result might be related to the fact that forwarded exposures are not equivalent to a non-exposures.

## *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. Exploratory analyses of our data indicate that only a modicum of advertisements are actually forwarded in our data, and that even when they are they do not have an adverse effect on sales. These findings expand on some previous research that suggest similar outcomes. In light of the negligible effect of DVRs on sales, it is interesting to speculate whether the DVR effect could even *increase sales* if the technology enhances the ability of advertisers to target their message more finely (Ansari and Mela 2003; Gal-Or et al. 2006).

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.

This research also suggests a number of future avenues for research. For example, it may be useful to understand how DVRs can be used to target advertisements (i.e., contextual advertising)

more effectively and how networks should price such advertisements to firms. Also of interest are the attendant implications for consumer welfare. Given the increasing ubiquity of DVR technology and its potential to reshape the advertising landscape, we hope this paper helps to lay the groundwork for this and other future research.

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*TABLES*

Table 1

OVERVIEW OF DATA

Data Set	Period	Use
IRI Panel Data	3/2003-3/2007	Dependent Sales Measures
IRI Demographics	3/2003-3/2007	Instruments for Selection Model
IRI Technology Survey	2005-2006	Instruments for Selection Model
IRI New Product Pacesetters	2004	Analysis for New Brands
TNS Advertising Data	2004-2005	Analysis for Advertised Brands
Nine Advertising Campaigns	4/2005-2/2006	Determine Advertising Broadcast Schedules
TiVo Log Files	7/2005-7/2007	Assess if Advertisements are Skipped

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Table 2

## SUMMARY STATISTICS (CENTS PER CATEGORY PER WEEK PER HOUSEHOLDS)

Year	Sample	$S3_{mci}$			$PL_{mci}$			$NP_{mci}$		
		Obs	Mean	St.Dev.	Obs	St.Dev.	St.Dev.	Obs	Mean	St.Dev.
2004	DVR	15666	0.24	0.64	20587	0.03	0.11	2402	0.012	0.064
2004	NON-DVR	37391	0.23	0.59	47544	0.03	0.12	5249	0.016	0.081
2005	DVR	15564	0.26	0.66	20587	0.03	0.12	2402	0.073	0.148
2005	NON-DVR	36614	0.24	0.63	47544	0.04	0.13	5249	0.074	0.171
2006	DVR	15211	0.25	0.65	20587	0.03	0.13	2402	0.095	0.218
2006	NON-DVR	35893	0.24	0.61	47544	0.03	0.13	5249	0.097	0.234

Table 3:

OVERVIEW OF ANALYSIS

Subsection	Analysis	Objective
1	Compare treated and control	Validating the control
2	Baseline expenditure regression	Test for TiVo effect
3	Expenditure regression with: i) Demographic interactions ii) DVR usage interactions	Control for heterogeneity
4	Expenditure regression on new items	Control for prior exposure
5	Category-level expenditure regression	Control for category heterogeneity
6	Category expenditure regression with: i) Selection on unobservables ii) Selection on observables	Control for endogeneity

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Table 4:

## DEMOGRAPHICS FOR THE TIVO VERSUS NON-TIVO HOUSEHOLDS

Variable	TiVo Households			Non-TiVo Households		
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Technology Ownership	819 <sup>a</sup>	0.335	0.139	3240	0.208	0.142
Income > \$45,000	819	0.626	0.484	3240	0.451	0.498
Children	819	0.342	0.475	3240	0.154	0.361
Family Size	782	2.870	1.295	3015	2.247	1.159
Households With Younger Children	819	0.132	0.339	3240	0.057	0.232
Older Singles	819	0.059	0.235	3240	0.030	0.170
Female Head > 45years	783	0.716	0.451	3061	0.869	0.338
Male Head > 45 years	656	0.720	0.450	2307	0.874	0.332
Male Head White Collar	656	0.462	0.499	2307	0.274	0.446
Female Head White Collar	656	0.474	0.500	2307	0.352	0.478
Female Head Retired	783	0.138	0.345	3061	0.340	0.474
Male Head Retired	656	0.143	0.351	2307	0.340	0.474

<sup>a</sup>The number of observations can change as a result of missing values.

Table 5

WILCOXON RANK-SUM TESTS FOR EQUALITY OF DISTRIBUTIONS  
 IN THE TIVO VERSUS NON-TIVO GROUPS IN 2004 (PRE-TIVO ONLY)

Dependent Variable	z	Obs	p-value
Total advertised expenditures	-19.567	4059	0.000
Change in total advertised expenditures	-0.190	4059	0.849
Private label expenditures	-11.412	4059	0.000
Change in private label expenditures	1.432	4059	0.152

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Table 6

## DIFFERENCE IN DIFFERENCE REGRESSIONS

Model	$T_{2008}$			$\Delta T_{2008}$		
	Coeff.	Std. Err.	95% Conf. Int.	Coeff.	Std. Err.	95% Conf. Int.
Advertised Brand Sales						
TiVo Effect	0.0002	0.003	(-0.0057, 0.0061)	-0.0004	0.003	(-0.0007, 0.0026)
Model Fit	R <sup>2</sup> =0.010			R <sup>2</sup> =0.011		
Private Label Sales						
TiVo Effect	0.0010	0.0008	(-0.0063, 0.0054)	-0.0001	0.0009	(-0.0019, 0.0017)
Model Fit	R <sup>2</sup> =0.006			R <sup>2</sup> =0.002		
Sample Size	62,332					

Table 7

MODERATING EFFECT OF FAST FORWARD BEHAVIOR

N=17,691	<del><math>\beta_{1.000}</math></del>	t	R <sup>2</sup>	<del><math>\beta_{1.000}</math></del>	t	R <sup>2</sup>
Change Private Label Expenditure	-0.002	-0.220	0.010	-0.172	-1.820	0.003
Change Leading Advertised Brands Expenditure	0.022	0.700	0.012	0.093	0.930	0.012

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Table 8

## MODERATING EFFECT OF HOUSEHOLD CHARACTERISTICS

Model	2008			2009		
	Coeff.	Std. Err.	95% Conf. Int.	Coeff.	Std. Err.	95% Conf. Int.
<b>Advertised Brand Sales</b>						
TiVo Effect	-0.031	0.017	(-.063, .002)	0.017	0.016	(-.015, .049)
Tech*TiVo	0.022	0.020	(-.018, .061)	-0.017	0.02	(-.056, .022)
Age*TiVo	-0.0003	0.002	(-.005, .004)	-0.0005	0.002	(-.005, .004)
Education*TiVo	0.002	0.002	(-.001, .006)	-0.004	0.002	(-.008, -.0007)
Income*TiVo	0.001	0.001	(-.001, .003)	0.002	0.001	(-.0003, .004)
Model Fit		R <sup>2</sup> = 0.010			R <sup>2</sup> = 0.012	
<b>Private Label Sales</b>						
TiVo Effect	-0.002	0.005	(-.0115, .007)	-0.008	0.005	(-.017, .002)
Tech*TiVo	0.0004	0.006	(-.011, .011)	0.005	0.006	(-.007, .017)
Age*TiVo	-0.0001	0.0006	(-.001, .001)	-0.0003	0.0006	(-.002, .001)
Education*TiVo	0.0002	0.0005	(-.0008, .0012)	0.0008	0.0005	(-.0002, .0019)
Income*TiVo	0.0003	0.0003	(-.0001, .0009)	0.0003	0.0003	(-.0004, .0009)
Model Fit		R <sup>2</sup> = 0.006			R <sup>2</sup> = 0.002	
Sample Size	62,332					

Table 9

DIFFERENCES REGRESSION FOR NEW PRODUCTS

Model	Coeff.	Std. Err.	95% Conf. Int.
New Product Sales			
TiVo Effect	0.0008	0.006	(-0.012, 0.043)
Model Fit			R <sup>2</sup> =0.043
Sample Size			6,135

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Table 10

ESTIMATED LOGIT PROPENSITY SCORES FOR TIVO TREATMENT

Variable	Coefficient	Std. Err.
Technology Ownership	4.936	0.356
Age	-0.229	0.037
Education	0.116	0.029
Income	-0.006	0.018
Constant	-2.221	0.275
pseudo R <sup>2</sup>		0.132
N		3747

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Table 11

A TEST FOR WEAK INSTRUMENTS

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Cragg-Donald Wald F statistic: 142.56

Threshold	Critical Value
10%	24.58
15%	13.96
20%	13.96
25%	8.31

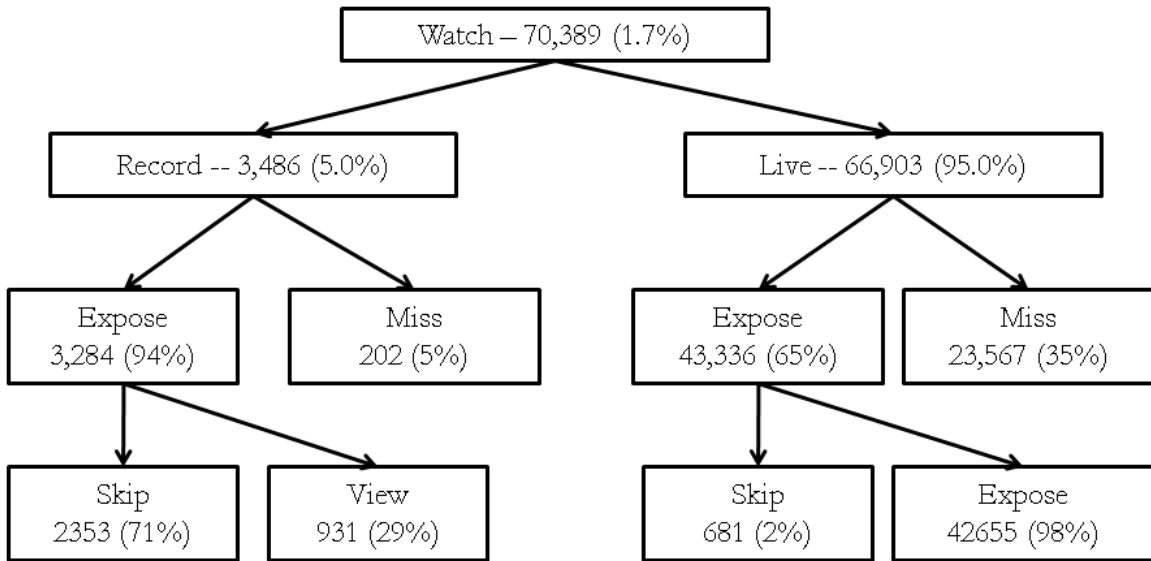
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FIGURES

Figure 1

FAST FORWARD BEHAVIOR



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## ENDNOTES

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<sup>1</sup>In some international markets, these devices are labeled Personal Video Recorders (PVRs).

<sup>2</sup>UPN and WB have since merged to form the CW network.

<sup>3</sup><http://www.pbs.org/mediashift/2008/03/the-new-rules-of-media091.html>

<sup>4</sup><http://www.reuters.com/article/pressRelease/idUS194788+31-Mar-2008+PRN20080331>

<sup>5</sup>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.

<sup>6</sup>We describe the advertising data used to determine the largest selling brands in advertising data subsection.

<sup>7</sup>We additionally considered two other measures; total category sales and sales of discounted products. The former measure captures any potential effects on primary demand. The latter measure may reflect effects on changes in consumer price response and/or increased brand switching (Bucklin, Russell and Srinivasan 1998). We find no effect of DVR on either of these measures well. To conserve space, we do not report these outcomes in our subsequent analyses.

<sup>8</sup>Regressing  $Y_{imc,2006} - Y_{imc,2004}$  on the DVR variable and controlling for market and category fixed effects provides a direct statistical test for  $\tau_{2005} + \Delta\tau_{2006}$ , or the 2006 treatment effect.

Consistent with the findings in Table 6, we find no effect. To conserve space and because this

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test offers limited additional insight, we refrain from reporting this contrast in our subsequent analyses.

<sup>9</sup>Ackerberg (2001) found that the marginal effect of advertising on demand fell considerably after the first trial of a newly-launched product.

<sup>10</sup>We refer the interested reader to Leuven and Sianesi (2003) for details on how we implement this estimator in STATA.

<sup>11</sup>In our data, recorded viewing is defined as views that are stored on the DVR. This does not include paused and delayed viewing. Hence, the total number of delayed views might be somewhat higher than the total number of recorded views as indicated in the figure below. It is further possible that recorded views are longer in duration than live views. In spite of this, overall delayed or recorded viewing rates remain low.

<sup>12</sup>Specifically, we combined our advertising campaign data with the DVR log files to measure the ad exposures for seven brands in 2005 and 2006. We then compute the correlation between changes in brands' advertising exposures and changes in brands' expenditures. These results are available from the authors upon request.