Online Appendix: Promoting Wellness or Waste? Evidence from Antidepressant Advertising

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Online Appendices

Appendix A - Functional Form

A.1 Log Hours as Dependent Variable

In this sub-appendix, I consider the alternative dependent variable of the log of absentee hours per month. As such, these estimates can be read directly as elasticities of absent days with respect to advertising. The results presented in Table A.1 are consistent with the main results. In particular, in columns (1) and (2), the correlation between current DTCA and labor supply indicates that advertising increases absenteeism, which is a spurious result if firms are targeting advertising at places and during times when individuals are likely to miss a lot of work. In columns (1) and (2), past advertising is negatively correlated with labor supply, but the estimates are not especially precise. In column (3), the positive correlation between absenteeism and current advertising disappears and the magnitude of the point estimate on past advertising decreases and becomes statistically insignificant. In column (4), moving to the borders does not change the point estimates in a meaningful way, but considerable precision is gained, making this column the preferred specification for this table. It indicates that current advertising does not affect absenteeism while the elasticity of labor supply with respect to past advertising is about -0.045, quantitatively consistent with the main results using levels as the dependent variable. Column (5) shows that the labor supply result is driven entirely by those who miss the most days on average.

A.2 Alternative Functional Form on Independent Variable

In this sub-appendix, I consider alternative functional forms of advertising to see if specifying the response to advertising in log form is important to the conclusions in the paper. I show the results of regressions using the logarithmic function of past advertising, a linear function of past advertising, a cubic function of past advertising and a square root of past advertising. Table A.2 shows the results of these regressions. Column (1) replicates the main result from column (4) of Table 4. Columns (2) and (3) show the sensitivity of this result to changing log(1+A) to log(0.1+A) or log(2+A), simply to show that the arbitrary choice of the scalar to eliminate the zeros problem is not pivotal to the results. Column (4) shows the results using the square root of advertising as the functional form. Column (5) shows a linear specification and column (6) shows a cubic specification. These specifications are not easily comparable to one another. However, I note two observations from this exercise. First, the marginal effects, computed at mean past advertising levels (provided in the table), are nearly identical between the first four columns. In the fifth column, there is a smaller marginal effect at the mean, as would be predicted from not allowing concavity. Even so, this marginal effect is not smaller by a tremendous amount. In the sixth column, the cubic function seems to be

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leveraging extreme values a bit more to arrive at a considerably larger marginal effect at the mean. Second, in all specifications, past advertising remains negatively related to days missed over the range of the full range of the data, and the first order term is statistically significant in all specifications. In the cubic specification, the second and third order terms are marginally significant.

To further illustrate the robustness of these results, I show the implied predicted days absent as a function of past advertising graphically. Figure A.1 plots predicted days absent against past advertising GRPs for the three functional forms. Vertical dashed lines indicate the first and ninety-ninth percentile of past advertising GRP observed in the data. In that range, the four functional forms are very similar. The choice of log function is not pivotal to the qualitative or quantitative results in this study.

TABLE A.1—LABOR SUPPLY - LOG OF ABSENT HOURS

	(1)	(2)	(3)	(4)	(5)
$Log(1+GRP)$	0.09429	0.11331	-0.01139	-0.00636	-0.00222
	(0.0255)	(0.0291)	(0.0290)	(0.0124)	(0.0146)
x$HighAbsentee$					-0.01496
					(0.0171)
$Log(1+GRP_{past})$	-0.16632	-0.11521	-0.04367	-0.04483	0.01748
	(0.0927)	(0.0434)	(0.0422)	(0.0211)	(0.0308)
x$HighAbsentee$					-0.0783
					(0.0391)
Individual FEs		x	x	x	x
Month FEs			x		
Border-Month FEs				x	x
Mean DV	2.26174	2.2619	2.2619	2.47743	2.52064
R-squared	0.00331	0.24914	0.3124	0.38071	0.3627
Observations	16,310,368	16,303,199	16,303,199	3,363,046	2,850,985

Standard errors are two-way clustered. First, they are clustered by (border)x(DMA)x(Month) to account for correlation in the treatment variable. Second, they are clustered by individual to account for the fact that there are repeated observations within individual over the sample.

FIGURE A.1. PREDICTED DAYS WITH DIFFERENT FUNCTIONAL FORMS

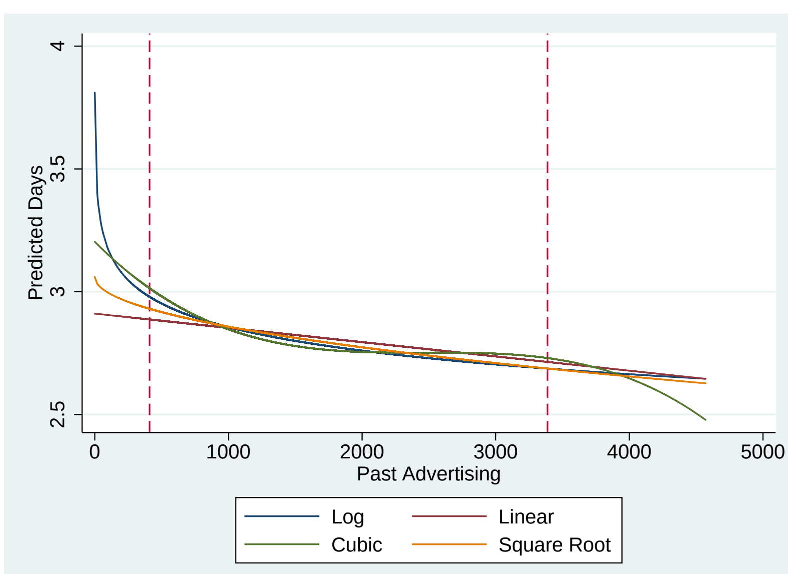

TABLE A.2—LABOR SUPPLY - ALTERNATIVE FUNCTIONAL FORMS ON ADVERTISING

	(1)	(2)	(3)	(4)	(5)	(6)
$f(A)=$	log(1+A)	log(0.1+A)	log(2+A)	$\sqrt{A}$	A	A
$f(A)$	-0.0339	-0.0284	-0.0353	-0.0014	2.505E-05	4.727E-05
	(0.0288)	(0.0263)	(0.0298)	(0.0050)	(0.0001)	(0.0001)
$f(A_{past})$	-0.1382	-0.1401	-0.1379	-0.0064	-0.0001	-0.0005
	(0.0602)	(0.0603)	(0.0602)	(0.0028)	(2.92E-05)	(0.0002)
A^2_{past}						2.222E-07
						(1.15E-07)
A^3_{past}						-2.993E-11
						(-1.71E-11)
$1000 * MFX(Mean(A))$	-0.1038	-0.1053	-0.1035	-0.0878	-0.0581	-0.3075
Individual FEs	x	x	x	x	x	x
Border-Month FEs	x	x	x	x	x	x
Mean DV	2.876	2.876	2.876	2.876	2.876	2.876
R-squared	0.3555	0.3555	0.3555	0.3555	0.3555	0.3555
Observations	3,363,046	3,363,046	3,363,046	3,363,046	3,363,046	3,363,046

In all columns, days of work missed is the dependent variable, which is exactly the same as in the main results, Table 4. Column (1) replicates the results from column (4) of Table 4. Columns (2)-(3) change the scalar under the log function to show that the arbitrary choice of the number 1 to eliminate the zeros problem is not pivotal. Columns (4)-(6) provide the regression results to go with the picture provided in Figure A.1.

Appendix B - Outcome Timing

Antidepressants take on average two or three weeks before showing any benefits, and six to twelve weeks before they show maximum beneficial effects Frazer, Benmansour and Manji (2002). As a result, effects should be strongest for advertising that is lagged two to three months with possible effects continuing in later months. In this appendix, I construct the measure of "past advertising" using different numbers of past months to see if the effect of past advertising comes into focus after some number of months. In Table B.1, each column is associated with how many lags are included in "past advertising." For example, in the first column, past advertising is just advertising from the previous month, while in the second column, past advertising is advertising from the sum of the past two months. The coefficient on log of past advertising is about -0.093 and not statistically significant in column (1). In column (2), the coefficient is -0.112 and is statistically significant. It continues to grow in magnitude in columns (3) through (5). Column (6) is back to the main preferred specification from the text and is almost identical to the estimate in column (5). There is evidence of an effect going back one month, but the effect size comes into focus when including two to four months of lags. This is consistent with the timing of how antidepressants should work. These results are consistent with at least some of the effect of advertising manifesting through the mechanism of increased prescriptions.

TABLE B.1—OUTCOME TIMING - VARYING MONTHS IN PAST ADVERTISING MEASURE

	(1)	(2)	(3)	(4)	(5)	(6)
$Log(1+GRP)$	-0.01638	-0.01991	-0.02178	-0.02694	-0.02533	-0.03388
	(0.0327)	(0.0289)	(0.0283)	(0.0285)	(0.0285)	(0.0288)
$Log(1+GRP_{past})$	-0.09306	-0.11232	-0.12183	-0.13345	-0.14047	-0.13824
	(0.0484)	(0.0481)	(0.0486)	(0.0554)	(0.0571)	(0.0602)
Individual FEs	x	x	x	x	x	x
Border-Month FEs	x	x	x	x	x	x
Mean DV	2.838	2.849	2.859	2.867	2.871	2.876
R-squared	0.3578	0.3594	0.3604	0.3594	0.3573	0.3555
Observations	3,532,123	3,496,077	3,461,857	3,428,355	3,395,111	3,363,046

Dependent variable is days absent from work in all columns. Each column number is associated with the number of lagged months included in the past advertising variable. Standard errors are two-way clustered. First, they are clustered by (border)x(DMA)x(Month) to account for correlation in the treatment variable. Second, they are clustered by individual to account for the fact that there are repeated observations within individual over the sample.

Appendix C - Selection into the Border Sample

In this section, I investigate how the border sample used for the preferred specifications is different than the non-border sample in terms of observables. I do this at a single point in time, July 2007, and provide information about the distributions of county-level averages in the border sample and in the excluded non-border sample. The point of this exercise is to show the extent of the overlap in the support in county-level observables. The border sample used for estimation is borders that make up less than 35% of the counties in a DMA on both sides of the DMA border. The distributions of county level averages look different from the individual level distributions, as highly populous counties often have very different observables at the individual level than less populous counties. Statistics presented are median and the 90th-10th percentile range to demonstrate the extent of overlapping support.

The variables examined are average income, population, percent Medicare Eligible, percent white, percent black, death rate, physicians per 1000 capita, hospital beds per 1000 capita, age, absentee days, percent employed hourly, percent prescribed an antidepressant, average price conditional on prescription and average co-pay conditional on prescription. The results are in Table C.1. While there are some small differences in medians and extremes, the border counties and non-border counties have considerable overlapping support.

Appendix D - County-Level Placebo Analysis

In this section, I extend the placebo analysis conducted at the individual level in Table 2 to county-level covariates collected from the census. As county-level demographics change slowly, I conduct the analysis both in changes and in levels. While a failed placebo check using levels would not invalidate the research design (only parallel trends are needed given the individual fixed effects), seeing that advertising does not predict demographic levels lends some credence to the intuition that the border counties are not specifically targeted by the advertisers. The demographics considered are population, average income, Hispanic share, Asian share, black share, elderly (over 65 years old) share, death rates, physicians per capita and hospital beds per capita. Each variable, with the exception of population, is normalized to have mean 0 and standard deviation 1 in the full sample for ease of comparison.

Results are presented in Table D.1 and Table D.2. Table D.1 runs the analysis from equation (1), but at the county level with county fixed effects and county-month fixed effects. Changes in antidepressant advertising at the border do not predict changes in any of the demographic variables in an economically

TABLE C.1—SELECTION INTO THE BORDER SAMPLE

	Border Median [10th pctile, 90th pctile]	Non-Border Median [10th pctile, 90th pctile]
Income	$27,260	$29,918
	[$22,226, $40,426]	[$23,380, $41,380]
Population	27,270	32,990
	[8,253, 125,679]	[6,249, 315,108]
% Medicare Eligible	15.3%	14.8%
	[11.8%, 20.7%]	[10.1%, 20.8%]
% White	93.7%	92.9%
	[63.6%, 98.2%]	[64.6%, 98.1%]
% Black	2.56%	2.56%
	[0.34%, 32.6%]	[0.26%, 29.7%]
Death Rate	1.05%	0.96%
	[0.73%, 1.30%]	[0.61%, 1.28%]
Docs per 1000	0.830	0.941
	[0.242, 2.120]	[0.264, 2.969]
Beds per 1000	2.159	2.244
	[0, 5.896]	[0, 7.708]
Age	44.59	44.19
	[41.02, 48.79]	[40.26, 49.00]
Absentee Days	1.67	1.68
	[0.583, 3.43]	[0.750, 3.25]
% Hourly	66.7%	57.0%
	[32.6%, 88.8%]	[21.6%, 84.4%]
% Antidep Rx	8.41%	7.97%
	[0%, 16.7%]	[0%, 14.3%]
Price	$57.24	$60.19
	[$28.93, $82.13]	[$32.06, $87.16]

Income, Population, % Medicare Eligible, % White, % Black and Death Rate are all county level measurements from census data in 2007. Age, Absentee Days, % Hourly, % Antidep Rx, and Price are individual level variables from the Truven sample that have been aggregated to county-level averages. In this way, we are comparing the distribution of counties rather than individuals for all variables, as the selection criteria into the estimation sample is by county.

significant way. The only variable of the nine which is statistically significantly predicted is the number of hospital beds per capita, though the estimate is very small. Table D.2 drops the county fixed effects to assess whether or not advertising levels predict demographic levels. Levels of antidepressant advertising at the border do not predict levels of any of the demographic variables across borders in a statistically or economically meaningful way.

TABLE D.1—COUNTY PLACEBO REGRESSIONS - CHANGES

	Pop	AvgIncome	Hispanic	Asian	Black	Elderly	DeathRate	Docs/Cap	Beds/Cap
$Log(1+GRP)$	-5.5326	0.0020	0.0008	-0.0004	0.0005	-0.0042	0.0049	0.0002	-0.0017
	(50.8684)	(0.0033)	(0.0010)	(0.0011)	(0.0010)	(0.0049)	(0.0117)	(0.0002)	(0.0008)
Mean DV	65,900								
R-squared	0.999	0.989	0.997	0.995	0.999	0.975	0.950	0.999	0.997
Observations	27,445	27,402	27,445	27,445	27,445	27,445	20,229	20,229	27,445

Each column represents a regression with the given variable as the dependent variable. Each includes county fixed effects and border-month fixed effects and log of past advertising. It shows that changes in these variables are not systematically predicted by changes in antidepressant advertising.

TABLE D.2—COUNTY PLACEBO REGRESSIONS - LEVELS

	Pop	AvgIncome	Hispanic	Asian	Black	Elderly	DeathRate	Docs/Cap	Beds/Cap
$Log(1+GRP)$	-2040	-0.0208	-0.0087	-0.0174	-0.0072	0.0028	0.0276	-0.0130	0.0038
	(3890)	(0.0190)	(0.0129)	(0.0138)	(0.0261)	(0.0266)	(0.0361)	(0.0150)	(0.0094)
Mean DV	65,900								
R-squared	0.603	0.683	0.678	0.511	0.788	0.502	0.503	0.436	0.550
Observations	27,445	27,402	27,445	27,445	27,445	27,445	20,229	20,232	27,446

Each column represents a regression with the given variable as the dependent variable. Each includes border-month fixed effects and log of past advertising, but no county fixed effects. It shows that levels in these variables are not systematically predicted by levels in antidepressant advertising.

Appendix E - Other Drug Advertising

E.1 Systematic Measurement Error - Statin Placebo

This sub-appendix is meant to address the potential threat to identification that systematic measurement error in the advertising data that would induce a spurious correlation between any kind of advertising and labor supply. I evaluate this concern by testing whether statin advertising has an estimated effect on labor supply. Similar to antidepressant advertising, statin advertising is decided at the DMA level and has discontinuous changes at DMA borders. It is also measured using identical technology by Nielsen. However, unlike antidepressants, statins are designed to lower cholesterol, which is a proxy for heart disease risk. It is typically prescribed well before heart disease is acute. High cholesterol by itself is not causally linked to reduced labor supply or work functionality, so response to statin advertising by individuals is not predicted to alter labor supply. If it does, it would indicate some kind of measurement error leading to a spurious effect.

Table E.1 is analogous to Table 4, but with statin advertising in place of antidepressant advertising. Column (4) is the preferred specification using the border approach. No relationship between past statin advertising and labor supply is found, though there is some amount of noise.

E.2 Omitted Other Drug Advertising

This sub-appendix is meant to address the potential threat to identification that other forms of drug advertising may lead to improved health, leading to improvements in labor supply. If such advertising were positively correlated with antidepressant advertising, it would induce a spurious correlation between antidepressant advertising and labor supply.

To address this concern, I test whether including non-antidepressant advertising in the main specification estimating the effect of antidepressant advertising on labor supply changes the estimated effect. Table E.2 shows that the effect of antidepressant advertising on labor supply is not significantly changed by including advertising for other prescription drugs into the regression. It remains negative and significant, and if anything, the magnitude increases slightly from 0.1382 to 0.1527

TABLE E.1—PLACEBO LABOR SUPPLY FROM STATIN ADS - MISSED DAYS OF WORK

	(1)	(2)	(3)	(4)
$Log(1+GRP)$	-0.4630	-0.4134	0.1071	0.0111
	(0.0578)	(0.1399)	(0.1955)	(0.0413)
$Log(1+GRP_{past})$	-0.30005	-0.2645	-0.36549	-0.01554
	(0.4030)	(0.3268)	(0.3149)	(0.0922)
Individual FEs		x	x	x
Month FEs			x	
Border-Month FEs				x
Mean DV	2.463	2.463	2.463	2.958
R-squared	0.0086	0.2804	0.3214	0.35549
Observations	14,134,343	14,126,575	14,126,575	2,876,150

Standard errors are two-way clustered. First, they are clustered by (border)x(DMA)x(Month) to account for correlation in the treatment variable. Second, they are clustered by individual to account for the fact that there are repeated observations within individual over the sample.

TABLE E.2— LABOR SUPPLY FROM OTHER DRUG ADS - MISSED DAYS OF WORK

	(1)
$Log(1+AntidepGRP)$	-0.0402
	(0.0357)
$Log(1+AntidepGRP_{past})$	-0.1527
	(0.0620)
$Log(1+NonAntidepGRP)$	0.0634
	(0.0493)
$Log(1+NonAntidepGRP_{past})$	0.3209
	(0.2288)
Individual FEs	x
Border-Month FEs	x
Mean DV	2.9373
R-squared	0.3355
Observations	2,989,572

Standard errors are two-way clustered by (DMA)x(Border)x(Month) and by individual.

Appendix F - Incremental Statistical Power

This appendix section highlights strategies to increase incremental statistical power from what the main border strategy offers and shows that there will be little additional power to be gained from this sample. All specifications here use the border sample with border-month and individual fixed effects. The two strategies used are first to include a lagged dependent variable, and second, to select among many individual level control variables and their interactions using a lasso as in Belloni, Chernozhukov and Hansen (2014).

Results of this analysis are in Table F.1. Column (1) replicates column (4) of Table 4 and serves as a baseline from which I will try to increase power. Column (2) adds in a lagged dependent variable to increase power. From an identification standpoint, adding in lagged days is not necessarily a good idea, as past advertising could have an effect on lagged days absent, which in turn has an effect on current month days absent. While lagged days absent likely provides better noise reduction than any other single control variable, it would be 'controlling' for one of the channels of the true effect. Column (2) shows that inclusion of the lagged dependent variable reduces the standard error on past advertising from 0.06021 to 0.05268, or about 12.5%. The point estimate is slightly reduced, so the t-statistic is approximately unchanged. Column (3) floods the model with controls and uses a post lasso, as in Belloni, Chernozhukov and Hansen (2014), to select the optimal control variables. The controls included in the first-stage lasso are polynomials of age, up to degree 10, lagged days missed from work, whether or not there was a prescription in the previous month, and all possible interactions of those variables. Using the lasso to select the controls yields a standard error of 0.05166, or about a 2% reduction from simply including the lagged dependent variable by itself. The point estimate decreases a small amount, but is not statistically from column (1) or column (2). These results show that individual month-over-month variation in days missed from work contains considerable noise. Reducing that noise is difficult, even with a large number of control variables and lagged outcomes. Inclusion of those controls does not significantly change the main result.

TABLE F.1—STATISTICAL POWER

	(1)	(2)	(3)
$Log(1+GRP)$	-0.0339	-0.0260	
	(0.0288)	(0.0297)	
$Log(1+GRP_{past})$	-0.1382	-0.1197	-0.1138
	(0.0602)	(0.0527)	(0.0517)
Lagged DV		x	
Full Controls Lasso			x
Mean DV	2.8763	2.8546	2.8311
R-squared	0.3555	0.3921	0.3776
Observations	3,363,046	3,249,118	3,238,529

All specifications include border-month fixed effects and enrollee individual fixed effects. Standard errors are two-way clustered. First, they are clustered by (border)x(DMA)x(Month) to account for correlation in the treatment variable. Second, they are clustered by individual to account for the fact that there are repeated observations within individual over the sample.

Appendix G - Representativeness of the Truven Data

A potential concern with these data could be that they are not a representative sample of the United States population. It comes from a sample of employers that were willing to participate in Truven's data collection. As a result, the individuals for whom we have labor supply information are all employed and insured by their employers. The individuals for whom we have medical claims information are either employed or are family members of those who are employed and have insurance coverage through the employer. As a result, this study will not be able to provide inferences about how advertising necessarily affects those who are uninsured or unemployed. That would be particularly problematic if those who are uninsured or are unemployed are more likely to be misled by advertisements.

However, the employed and insured population is both a quantitatively relevant population as well as a policy relevant population when considering labor supply. Labor supply results are only scaled up to the working population rather than to the entire population. In this way, any benefits of advertising that are measured might be viewed as somewhat conservative, as they do not include any benefits accruing to retirees or unemployed persons. This population is also practically useful for the exercise in this study in a way that other populations are not– it has an available measure of ex post well-being to measure under different levels of advertising.

There is no other data comparable in size and scope that I am aware of that provides the opportunity to measure the effects of advertising on administratively measured labor supply. As a result, any non-representativeness does not eliminate the policy relevance of the population studied nor the conceptual value of the measurement on relatively clean data.

REFERENCES

Belloni, Alexandre, Victor Chernozhukov, and Christian Hansen. 2014. "Inference on treatment effects after selection among high-dimensional controls." *The Review of Economic Studies*, 81(2): 608–650.