Promoting Wellness or Waste? Evidence from Antidepressant Advertising

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December 29, 2018

Abstract

Direct-to-Consumer Advertising (DTCA) of prescription drugs is controversial. Even if drugs are efficacious, advertising may drive people to be prescribed for whom treatment will be ineffective. Leveraging plausibly exogenous variation in advertising driven by the borders of television markets, this paper provides the first quasi-experimental measurement of the effect of DTCA on ex-post well-being. In particular, antidepressant advertising decreases work absenteeism, a primary outcome associated with depression. The wage benefit of a 10% increase in advertising is about $770 million. This labor supply effect co-occurs at the individual level with an incremental $32 million in new initiations of antidepressant treatment. Keywords: Advertising, Outcomes, Welfare, Pharmaceuticals. JEL classification: M31, M37, M38, H23, I11, I12, I18, L51, M54.

*Bradley.Shapiro@chicagobooth.edu. University of Chicago Booth School of Business. I thank Stephen Lamb for excellent research assistance. I particularly thank Jennifer Brown, Amanda Starc, Daniel Sacks, Jason Abaluck, Jean-Pierre Dubé, Sanjog Misra, Ruben Jacobo-Rubio, Neale Mahoney and Stephen Ryan for detailed comments. Results calculated based on data from The Nielsen Company (US), LLC and marketing databases provided by the Kilts Center for Marketing Data Center at the University of Chicago Booth School of Business and from Truven Health Analytics, an IBM company, provided by the Center for Health and the Social Sciences (CHeSS) at the University of Chicago. The conclusions drawn from the Nielsen data and the Truven data are those of the researchers and do not reflect the views of either Nielsen or Truven. Nielsen and Truven are not responsible for, had no role in, and were not involved in analyzing and preparing the results reported herein. I thank Chris Lyttle for assistance with Truven data. I acknowledge generous financial support from the Beatrice Foods Co. Faculty Research Fund at the University of Chicago Booth School of Business.

1 Introduction

Understanding and measuring the consumer welfare effects of advertising is difficult. While recent empirical literature has documented that advertising can be effective to stimulate sales, there is no guarantee that advertising selects the "right" consumers to purchase in the category or to switch brands. Additionally, advertising may affect consumer behavior without increasing sales of the advertised product. In particular, it could alert consumers to evaluate their needs, which might be met by some action other than purchasing the advertised good.

Advertising could decrease consumer welfare. In particular, if advertising drives sales via distorting beliefs away from the true quality or match value of the product, it could lead to changes in behavior that are regrettable ex post. Even category expansive advertising could be harmful in such a scenario. Choice data are insufficient to draw conclusions about welfare if it is possible for consumers to make ex post incorrect choices. Whether advertising is good or bad is fundamentally a question of the relationship between advertising and selection.

This paper addresses this question by measuring the effect of advertising on ex post welfare-relevant outcomes of consumers in the context of direct-to-consumer advertising (DTCA) of antidepressant drugs. If advertising is good, it should improve consumer outcomes by at least as much as the cost of the product. If advertising distorts beliefs, ex post mistakes could be caused by advertising, which may reduce consumer well-being.

DTCA is a useful case study to study this question for two main reasons. First, it is very controversial in policy, making the results of the case study independently useful. Some believe it provides helpful information for consumers while others believe it misleads consumers into making inefficient decisions from an individual welfare perspective.[1] Which is true on average is an empirical question. Second, antidepressant drugs are a useful category for addressing the more general question of whether advertising draws from a useful margin because there is a clear welfare-relevant ex post outcome that can be measured in the data. In particular, the consumer's needs are clear: to ease the major impacts of clinical depression. The attainment of this goal is measurable. Since depression primarily causes individuals to lose motivation, I study the impact of antidepressant advertising on

[1] For example, PhRMA believes these ads to be useful to consumers while the AMA believes them to be harmful. In fact, Hillary Clinton even campaigned on making DTCA illegal as part of her 2016 presidential platform.

administratively measured labor supply.[2]

The answer to this question is not a priori obvious. Despite the fact that clinical trials document a link between antidepressants and (survey measured) labor supply, and previous literature (Shapiro (2018)) has documented a link between advertising and antidepressant utilization, the link between advertising and labor supply is not a priori obvious for two main reasons. First, advertising may select different patients than those selected for clinical trials. The central question in this paper is fundamentally one of the relationship between advertising and selection. Neither clinical trials nor the past literature on the link between advertising and utilization is helpful in understanding that selection. Second, advertising may affect consumer behavior over and above its effect on antidepressant utilization, which could lead to changes in welfare-relevant outcomes. In particular, a consumer could see an antidepressant ad, learn that he or she has clinical depression, and seek an alternative method for treating depression that does not involve antidepressants, such as cognitive behavioral therapy (CBT). Neither the clinical trials nor the literature on the effect of advertising on utilization is helpful in understanding how advertising may affect non-drug treatment of depression.[3]

Whether or not depressive symptoms subside and outcomes improve as a result of advertising is first-order important, has never before been studied and is a necessary input to measuring the consumer welfare effects of advertising. Additionally, the results speak to the mechanism of how advertising works. If advertising works through objective information, depression should only improve as a result of advertising, all else equal. If it works through distorting beliefs, depression could worsen. As there may be unmeasured elements of both cost and benefit, this study does not provide a full welfare analysis, but it provides necessary, important and never-before-studied inputs to such an analysis.

This empirical exercise comes with challenges. First, advertising is endogenously chosen by firms in a way that might lead advertising to be spuriously correlated with sales and labor supply outcomes. Second, labor supply is determined by many factors other than depression, and by extension, antidepressant advertising. This leads to the potential problem of low statistical power due to noise in the dependent variable in the estimation

[2] All of the main symptoms of depression listed by the Anxiety and Depression Association of America include elements of significant lost motivation. There is a rich literature, summarized below, documenting the link between depression and labor market effort.

[3] I note here that even if the relationship between antidepressant advertising and labor supply were obvious a priori to an economist, the relationship is quite clearly not obvious to policy makers or the AMA, who have directly suggested that advertising misleads consumers into making poor decisions. As a result, documenting the relationship is very important from a policy perspective.

of the effect of DTCA on labor supply. Finally, any effects of advertising on labor supply are not expected to materialize immediately, as it takes time for a patient to begin to show improvement from treatment. Antidepressants, in particular, take on average two or three weeks before showing any benefits, and six to twelve weeks before they show maximum beneficial effects (Frazer et al. (2002)). The need to evaluate both current and lagged advertising effects exacerbates statistical power difficulties.

In this paper, I leverage a unique panel data set that connects medical claims with administrative labor supply information from employers at the individual level. To overcome the endogeneity of advertising, I take advantage of the panel nature of the data to take into account both individual-specific differences in labor supply and systematic seasonal variation. To control for remaining endogeneity, I make use of quasi-random variation in advertising generated by the borders of television markets, as in Shapiro (2018). Despite decreasing the number of observations in estimation, focusing on borders in this case increases statistical power. Seasonal factors that impact labor supply, such as weather and local labor market conditions, are highly geographically correlated. By making close geographic comparisons, variation in labor supply driven by factors other than advertising is considerably reduced. The reduction of noise in this case outweighs the reduction in observations that would decrease power.

I find that DTCA causes benefits in the form of increased labor supply. In particular, past advertising causes a decrease in absenteeism in the current month. A 10% increase in DTCA in the past six months causes a decrease of 0.0138 days absent per person (in the population) in the current month, implying an aggregate of $769.5 million in wage benefits.[4][5] Additionally, advertising leads to a small increase in the likelihood of becoming a salaried rather than hourly employee and has no detectable effect on job separations in the medium term. This measurement of the effect of DTCA on ex post benefits to consumers is the primary contribution of this paper and has not been studied to this point. Together the results provide causal evidence that advertising helps consumers decrease the negative impacts of of depression.

The measured labor supply benefits of advertising could operate through one of two broad channels. First, advertising could induce changes in behavior with respect to pre-

[4]Depending on the individual's employment contract, these specific dollar benefits could accrue to employers rather than individuals. However, they are still an indication that the effects of depression are being successfully diminished because of the advertising.

[5] Using results from the medical literature as a benchmark, this point estimate is within a medically plausible range

scriptions: new initiations of treatment, discontinuations of treatment or switches between types of antidepressants could lead to improved outcomes. Second, advertising could provide informational spillovers about depression that lead to other consumer behavior that decreases the impact of depression. For example, advertising could induce increased visits to a therapist. Alternatively, it could make consumers and their families more aware of the nature of depression, allowing individuals to address depression through time with family, exercise or other non-medical means.

I find that one important mechanism by which advertising impacts labor supply is new initiation of antidepressant treatment. I replicate findings in the literature (Iizuka and Jin (2005, 2007); Shapiro (2018); Sinkinson and Starc (2017); Alpert et al. (2015); Hosken and Wendling (2013)) that DTCA induces more patients to initiate antidepressant treatment with an elasticity of about 0.031.[6] Further, I find that this increase in treatment initiation co-occurs with improvements in labor supply at the individual level.[7] I also find that advertising has a small negative effect on current period refills, conditional on treatment in the previous month. This is consistent with the list of side effects at the end of the ad persuading some consumers to stop taking their pills, but this effect is uncorrelated with the effect of advertising on labor supply.[8] Large effects of advertising on drug prices, the generic penetration rate and switching to different antidepressants conditional on treatment can be ruled out. In terms of non-prescription mechanisms, point estimates indicate no effect of advertising on therapy visits. Other behavioral changes, such as family intervention or exercise are not measurable in the data, and as a result, cannot be ruled out as possibilities.

Do the benefits of increased labor supply justify the costs? Costs to the consumer and payer include both the cost of advertising marginal prescriptions and the non-pecuniary

[6]This elasticity implies a 10% increase in DTCA costs patients or their insurers about $32 million, assuming patients choose a drug of average price. This elasticity is also entirely in line with single source, multi-category research on the effect of advertising on sales in Shapiro et al. (2018).

[7]The discussion of mechanisms is necessarily more directional/qualitative in nature than the main first order effect due to the relatively small and just significant effect of advertising on utilization. While more speculative attempts to assign the mechanism are possible with further ex post exploratory analysis, conclusions should be treated cautiously. An ex ante plausible and powerful design to precisely assign the relative importance of various mechanisms is not available. Therefore, all discussion of mechanisms is necessarily more qualitative and exploratory than the main effects. Requiring that a paper precisely pin down such a mechanism would either preclude studies on this topic from ever being published or drive studies to be overly speculative in search of mechanisms. This paper takes a strong stance that the main effect is well identified, independently important, and has not been studied to date. There is also good directional evidence on mechanisms.

[8]This is *not* consistent with advertising selecting patients who are more likely to quit. That effect would be estimated by the effect of past advertising on the likelihood of current refills. Such an effect is tested for and not found.

cost of any adverse effects. The benefits of increased labor supply outweigh the total cost of additional prescriptions by more than an order of magnitude. In addition to the dollar costs of advertising marginal prescriptions, I find that advertising does not predict increased adverse effects or increased failure to complete a full course of treatment, indicating that prescriptions which are advertising-marginal are no more likely to be of poor match value than average prescriptions.

The first and most important contribution of the paper is to provide the first ever causal link between DTCA and welfare relevant consumer outcomes. Previous papers have linked DTCA to non-demand outcomes, but few with clear welfare implications. For example, Niederdeppe et al. (2017) associates statin advertising both to increased exercise and to increased fast food consumption; David et al. (2010) finds some evidence of advertising increasing adverse effect reporting; Kim and KC (2017) associates advertising for the erectile dysfunction drug, Viagra, to birth rates; Chesnes and Jin (2016) finds that advertising drives consumers to search for information about the drugs online; and Kamenica et al. (2013) find that exposure to ads affect the physiological response to Claritin, potentially increasing the placebo effect. In contrast to these studies, this paper ties advertising to real-world measures of consumer well-being, providing the first evidence of a link between DTCA and labor market outcomes.[9]

Second, this paper adds to the literature which traces out the benefits of access to medical care in terms of labor market outcomes. Labor supply decisions are often considered as part of a labor-leisure trade off by individuals, but in the case of labor supply decreases induced by health factors, I will consider absences a pure cost to the individual incurred by necessity rather than a choice to consume leisure. Previous papers have found effects of new technologies on more extreme margins, but this is the first paper to show that advertising-marginal access to treatment can have economically meaningful effects on labor supply. Garthwaite (2012) and Bütikofer and Skira (2018) find that when the

[9]I emphasize here that the relationship between DTCA and measurable benefits is not clear a priori despite what has been learned from clinical trials about drug efficacy. Clinical trials of antidepressants typically use survey measured labor supply as at least part of an outcome. However, this is on a controlled population of individuals known to have depression. A main concern with DTCA is that it might select on individuals who are not appropriate for treatment, and they might nonetheless get treated. That is, DTCA might select patients who do not meet the diagnostic criteria to be included in the clinical trial. Additionally, DTCA could operate through mechanisms other than the drugs themselves. If DTCA induced a clinically depressed individual to seek non-drug treatment or management for depression, it could lead to improved outcomes that are not at all informed by the clinical trials. Finally, clinical trials measure outcomes using survey instruments, while the labor supply data in this study are administrative. The estimates in this study can neither be easily replicated in a clinical study setting nor be easily constructed as a product of estimates in the extant literature.

Coxx-2 inhibitor, Vioxx, was pulled from the market for fear of adverse effects, there was a substantial decrease in labor supply. Papageorge (2016) links innovation of HIV drugs to patient choices about labor supply and medical treatment. Currie and Madrian (1999) provide an excellent review of the literature linking various types of health and access to treatment through insurance to labor supply, noting a particular link between mental health and labor supply.

Third, this paper adds to the marketing literature thinking about the relationship between advertising and selection. In terms of health, Aizawa and Kim (2018) show that if health insurance could select on health status using advertising, it could have a substantial equilibrium effect on prices, while Shapiro (2017) in that same market finds evidence that advertising provides no such advantageous selection. In the market for mortgages, Grundl and Kim (2017) find that advertising is both targeted at and more effective on people who stand to gain from re-financing. This study shows that those selected by advertising show increased well-being and are not more likely to incur adverse effects.

The paper proceeds as follows. Section 2 briefly discusses depression and its economic impacts. Section 3 outlines the possible mechanisms of DTCA being socially beneficial or socially harmful in a simple framework. In section 4, the data used in the study are discussed. Section 5 details the research design, focusing on the borders of television markets. Section 6 presents the results, and section 7 concludes.

2 Depression

Depression is a condition that affects roughly 10% of Americans at any moment in time and is a chemical imbalance in the brain leading to decreased self-worth and motivation. In economic terms, it is characterized by the systematic underestimation of one's marginal product of effort (De Quidt and Haushofer (2016)) and has been associated both with large direct costs of medical care as well as large indirect costs of reduced economic activity (Berndt et al. (2000); Currie and Madrian (1999); Greenberg et al. (1993a,b); Stoudemire et al. (1986); Woo et al. (2011); Tomonaga et al. (2013); Stewart et al. (2003); Boyer et al. (1998)). Survey evidence (Kessler et al. (2003)) suggests that only about half of those who have experienced psychiatric disorders have received any kind of professional treatment.

De Quidt and Haushofer (2016) provides a nice framework to think about depression from the perspective of economic theory. In particular, it models individuals as unsure of how much to attribute their productivity to luck or to their own efforts. If an individual

gets enough repeated unfavorable draws from the luck distribution, he or she will Bayesian update to believe that the low productivity is innate. The belief that the individual has low marginal product leads to lower effort and investment in human capital. This framework provides a theoretical and rational basis for the well-documented connection in the medical literature between depression and labor supply.

Providing evidence to posited economic effects, Berndt et al. (2000) finds that early-onset depression causes substantial human capital loss. Greenberg et al. (1993a),Stoudemire et al. (1986),Boyer et al. (1998) and Tomonaga et al. (2013) all find that the economic costs of depression in terms of labor supply and productivity are far in excess of the average cost of treatment. Stewart et al. (2003) estimates the productivity cost of depression to be about $31 billion to employers in the US. Greenberg et al. (1993b) similarly estimates the annual costs of depression to be about $44 billion per year in the US. Woo et al. (2011) finds that workers with major depressive disorder (MDD) lose about 30% of their annual salaries to costs associated with missing work or being unproductive at work. Additionally, Greenberg et al. (2015) update previous results and find that the economic burden increased substantially between 2005 and 2010.

In terms of other effects of depression and its treatment, Sobocki et al. (2007) find that depression is associated with significantly lower health related quality of life instrument scores, but people who initiated treatment saw improvement. Similarly, Rost et al. (2004) finds that treatment with the SSRI sertraline induces improvements in social adjustment and in subjective quality of life, relative to placebo. These results support the interpretation of depression-induced absences as pure cost rather than the consumption of leisure. Stewart et al. (2003) finds that self-reported use of antidepressants among people with depression is only around 30% even though reported effectiveness was moderate. Consistently, Bharadwaj et al. (2015) posits that because mental health often carries with it a stigma, it might be expected that society is still in the steep part of the marginal benefit curve with respect to depression treatment.

Additionally, survey evidence in Kessler et al. (2003) has found that around half of those afflicted with depression get any kind of treatment. Some of these people may fail to get treated because they are poorly suited to or cannot afford available treatments. It is difficult to know how many people fail to report depression accurately in surveys due to either stigmatization or lack of understanding of the exact nature of the condition. As a result, there may be some margin for improved information, de-stigmatization and treatment of depression, but it is unclear whether or not DTCA is an effective means of targeting those

with depression who stand to gain from treatment but who are as of yet untreated.

3 The Welfare Economics of DTCA

The social desirability of DTCA is the subject of considerable controversy, and it is legal in only the United States and New Zealand. A ban on DTCA was part of Hillary Clinton's 2016 platform as a presidential candidate, Senator Al Franken sponsored legislation to end the tax deductibility of DTCA, and recently, the American Medical Association (AMA) and the American Society of Health System Pharmacists (ASHP) came out in favor of a ban on DTCA. The main arguments opposing DTCA are twofold. First, advertising might mislead consumers into believing a drug would benefit them when it would not. This may lead them to make unreasonable requests of their physicians which are often honored. Second, advertising might steer patients to more expensive brands when less expensive generics are available.

The Pharmaceutical Research and Manufacturers of America (PhRMA) takes an alternative position– that advertising provides information about diseases and treatments that some consumers would otherwise not have. In the absence of that information, these patients might go untreated and miss out on important benefits of treatment. The FDA regulates the content of these ads to ensure that risks are presented and that claims are scientifically justifiable.

DTCA could affect consumers' decisions in many ways, some of which are good for society and some of which are bad. In this case, I focus on measuring whether advertising-marginal prescriptions are inefficient from the perspective of the patient or the payer.[10] To help fix ideas, I present a simple framework for how advertising affects prescription choice. This framework is not intended as a model to be estimated. Rather, it is to illustrate that category expansive effects of advertising are not sufficient for advertising to be welfare-positive as well as highlight ways in which advertising could be good or bad for consumers. Assume a simple expected utility model whereby each consumer expects utility from antidepressant drugs:

$$E[u_{ij}(A)] = I_{ij}(A) * [E[v_{ij}|A] - p_{ij}],$$

[10]This is an overly stringent test on total social welfare since the cost of individually inefficient prescriptions could be offset by increases in firm profits. Such offsets might not be viewed as acceptable trade-offs by policy makers.

where $I_{ij} \in \{0,1\}$ reflects whether or not consumer i is informed of the existence of product j, v_{ij} is consumer i's true individual-specific valuation of the product, $E[v_{ij}]$ is consumer i's expectation of the value received from product j, p_{ij} is the price that consumer i faces for product j and A is advertising. The utility of the outside good is normalized to zero. Consumer i buys product j if

$$E[u_{ij}(A)] > E[u_{ik}(A)] \, \forall \, k \neq j, \, and \, E[u_{ij}(A)] > 0.$$

Through this simple framework, it is straightforward to highlight arguments for and against DTCA. The negative view of DTCA can be translated into this framework as $E[v_{ij}|A > 0] > v_{ij}$. That is, advertising causes consumers to have a more optimistic view of how well a drug will work than is true.

Importantly, as long as $E[v_{ij}|A] > v_{ij}$, the informative effect is welfare ambiguous. It could be that $E[v_{ij}|A] - p_{ij} > v_{ij} - p_{ij} > max_{k \neq j}\{v_{ik} - p_{ik}\}$, so a prescription is still better than no prescription for that individual, despite the biased expectation. Alternatively, it could be that $E[v_{ij}|A] - p_{ij} > max_{k \neq j}\{v_{ik} - p_{ik}\} > v_{ij} - p_{ij}$, making the prescription inefficient—that individual would have been better off with a different choice.

The positive view on DTCA can be translated into this framework as $|E[v_{ij}|A > 0] - v_{ij}| \leq |E[v_{ij}|A = 0] - v_{ij}|$. That is, advertising serves to give consumers a better idea of their true match value with a product through information. If this is true, any behavioral effect of advertising would improve match value, making consumers at least as well off as if they saw no advertising. In this case, any informative effect of advertising on I_{ij} could only be welfare positive.

A final mechanism through which DTCA could be welfare negative has less to do with advertising in particular and more to do with the nature of health insurance. That is, because the price a consumer pays, p_{ij}, is typically far lower than the price insurance companies pay on a consumer's behalf, say P_{ij}, the consumer decision problem itself is biased in favor of getting prescribed from the perspective of the other members of the insurance plan. That is, since the end consumer is not bearing the full cost of the prescription, it must be passed through in premiums to other members of the health insurance plan or from the profits of the insurance company itself. With private insurance markets, this behavior would distort the insurance market, leading to associated costs of increased premiums on coverage, for example. In this case, there may be some inefficient prescriptions with or without DTCA, and DTCA will amplify the issue. Of course, co-pays are typically considerably larger than

the marginal costs of production, so these kinds of moral hazard effects will still push total quantity closer to the socially optimal quantity, but there may be externalities in financing this shift.

The main takeaway from this exercise is that ex post inefficient purchases from an individual consumer perspective are possible, even if advertising is category expansive. As a result, additional information beyond purchases is necessary to indicate whether the purchases were worthwhile. To that end, this paper will measure ex post welfare-relevant outcomes that occur as a result of advertising to speak to whether or not advertising is more likely to be helping or hurting individual decision making.

4 Data

4.1 Advertising Data

Advertising data from AC Nielsen's Media database from 2007-2010 are used in this study and are provided by the Kilts Center for Marketing at the University of Chicago Booth School of Business. The database tracks television advertising at the spot-time-DMA level for every product which advertises on television. A DMA, or designated market area, is a collection of counties, defined by the Nielsen company, that all see the same local television stations and affiliates. The top 130 out of 210 DMAs are indicated as "full discovery market" by AC Nielsen, meaning all local television advertising occurrences are measured using monitoring devices. In many of the smaller DMAs, only advertising occurrences that match ads in the larger markets are included. This study uses each of these full discovery markets which has a monitoring device on every major network affiliate (ABC, NBC, CBS and FOX), which is 120 DMAs.

In the top 25 DMAs, household impressions are measured from set top viewing information that is recorded in households. Each household impression represents that a single household viewed an ad in a time period. In DMAs ranked 26-210, advertising impressions are estimated from quarterly diaries filled out by households.[11] The data also include the

[11] While impressions are the main advertising measure of interest, there is some concern that the infrequent and self-reported viewing data may be measured with error. However, the set of Nielsen panelists is relatively stable, so as long as that error is persistent within individual, fixed effects at the DMA-level or finer (in this case the individual level fixed effects) will control for the error. All analysis either has been or easily can repeated using ad occurrences as an alternative measure to see if the results are consistent. Please contact the author if you are interested in such analysis.

total estimated expenditure of the firm on the advertisement; the duration of the advertisement; and very coarse age, race, and gender demographic breakdowns of the impressions data. The data include the parent company of the product advertised, a description of the product being advertised, and a very brief description of the content of the advertising copy.

In addition to local advertising, there is also national advertising. National advertising occurrences are aired in all DMAs. For example, if a firm were to buy a national ad for a product on the CBS evening news, that ad would play in the New York DMA, the Chicago DMA and all other DMAs during that episode of the CBS evening news. As the identification strategy in this paper will exploit variation in local advertising, it is important that there be a significant amount of variation in local advertising.

Total DTCA of prescription drugs, while significant, has decreased from about $3 billion in 2004 to a little over $2 billion in 2012. Meanwhile, antidepressant DTCA makes up an important fraction of total DTCA and has increased from about $200 million in 2004 to a peak of about $400 million in 2011, declining to about $300 million in 2012. Natural fluctuations in DTCA over time come from the dynamics of the set of drugs that are under patent at any given time and whether or not those are suitable to DTCA.

For an average DMA-month, 7% of the advertising is local advertising, but there is considerable variation in that fraction, with some DMA-months having zero local antidepressant advertising occurrences and some DMA-months having as much as 74%. The standard deviation of the percent of advertising that is local is 13.4%, meaning there is considerable variation both in local advertising and in the share of total advertising that is made up by local advertising in any given DMA-month. It is important to note that in the antidepressant category, the advertising copy for local advertising is identical to that for national advertising, and a viewer would be unable to tell whether an advertisement for an antidepressant were purchased locally or nationally by that producer from watching the ad.

Pairing these data with market size estimates, the total number of Gross Rating Points (GRPs) that each advertisement constituted is computed. A GRP is the typical unit of sale between a firm and a television network for advertising space: it is calculated as the total number of advertising impressions per household in the time period divided by the population in the DMA, multiplied by 100. As such, a monthly increase of 100 monthly GRPs can be interpreted as the average person viewing the ad one additional time over the course of that month.

This study focuses on advertisements for the antidepressant drug category. There are many antidepressants, most of which are now generic. The few products that advertise in

the data are branded. The primary brands advertising between 2007 and 2010 are Abilify, Cymbalta, Effexor XR, Pristiq and Seroquel XR. While Effexor XR and Pristiq are all solely indicated for the treatment of depression during the sample period, Abilify and Seroquel XR are used both in the treatment of depression and in the treatment of psychosis. Cymbalta is also indicated for fibromyalgia (during the sample period) and pain (after the sample period). However, the ad copy description indicates that all Cymbalta, Abilify and Seroquel XR advertisements over the course of the data are for the depression indication. The average DMA-month has 227.27 GRPs for antidepressants, but with wide dispersion. The standard deviation of DMA-monthly GRPs is 148.67. A histogram of DMA-monthly GRP levels is provided in Figure 1.

4.2 Claims Data

Insurance claims data come from Truven Health MarketScan® Commercial Database, which come from Truven Health Analytics, Inc., an IBM company. The claims are for individuals with employer-sponsored insurance in the United States who work for companies that are willing to provide the data. From these claims, I harvest prescription and demographic information on a monthly basis. In terms of geography, the claims include county codes.[12] To ensure accurate measurement of whether a prescription is a new prescription, I focus on those individuals present in the data from the start in January of 2007 and consider their prescription decisions beginning in February 2007. Employees are included in the data whether or not they have a particular depression diagnosis. This is because a potential counterfactual of not seeing an ad is for pre-existing underlying depression to continue in an individual without a diagnosis. I define a new prescription as a prescription following a month with no prescriptions and a refill prescription as a prescription following a month with a prescription. Slight variations on the date of refill might cause a mistake in coding a month as non-prescription that should have been coded with a prescription. To deal with this possibility, if a three month sequence indicating prescription - no prescription - prescription is observed, I infer that the middle month in fact was a month prescribed. As a result, in this situation, the third month will not be coded as a "new" initiation of treatment. The final data set that is cleaned and matched to advertising data in the full discovery

[12] Some versions of data from this source provide zero geographic information. Other versions provide 3 digit zip codes. This one provides county FIPS codes with the condition that geography-specific cell means cannot be disclosed. Having county FIPS codes is important for matching the advertising data with the claims data, as DMAs are defined as collections of counties. That is, each county only belongs to one and only one DMA.

markets contains 1,835,265 individuals with employer-sponsored insurance who are on average 45 years old. About 36% of the employees are paid hourly and 64% are salaried. In an average month, 8.1% of the individuals in the data are prescribed an antidepressant and on average, 59.1% of those antidepressant prescriptions are for generics. While 88.5% of those prescribed continue to be prescribed an antidepressant in the following month, 35% of those switch to a different molecule. Only around 2% of the population has a visit to a therapist in a given month.[13] More summary statistics are available in Table 1.

The claims data also contain information on the transacted prices and co-payments of prescriptions. Co-payments are based on individual formularies that come with insurance contracts. These contracts typically last a full year, so there is not within-year month-over-month variation in co-payments of each option for a given individual. Similarly, prices reflect the co-payment plus the payment by the insurance company, as reported by the insurance company. If there are rebates that the insurance company receives from the drug manufacturer that are not included in the insurance company's reported prices, the reported prices would over-state the true cost of a prescription. The contracts between insurance companies and manufacturers also typically last at least a year. As such, all variation within an insurance-contract year in transacted co-payments or prices would reflect variation in choices of different drugs, which carry with them different prices rather than changes in prices for a given drug.

The average price of an antidepressant prescription filled in the data is $62.48, and the average co-payment faced by the individual enrollee for an antidepressant prescription filled is $11.22.

4.3 Labor Supply Data

Information about worker labor supply is provided from the Truven Health MarketScan® Health & Productivity Management (HPM) database. A subset of the employers who provide the claims data also provide human resources records on individual enrollee absences from work. It is important to point out that this is administratively recorded data and not survey data. Of the individuals in the claims data, there are 518,284 individuals in the labor supply data between 2007 and 2010. This information is administratively recorded for both hourly and salaried employees. All of these individuals are present in the claims data, but not all of the individuals in the claims data are present in the labor supply data. As a

[13] This number is exactly in line with what is found in literature studying therapy directly (Cronin et al. (2017)).

result, the analysis using sales as a dependent variable will have more observations than the analysis of labor supply as a dependent variable. These are included to improve statistical power in the analysis of sales.

The average number of missed work days in the data is 2.375 with a standard deviation of 3.155 and a median of 1.25. As missed days can be for any reason, there is a large amount of month-to-month variation in missed work days, even within individuals. The median number of missed days of 1.25 reflects an average of 3 weeks out of the office per year, which is a reasonable number for a generally healthy person with two weeks of paid vacation as well as some paid sick leave.[14] Further discussion of the representativeness of the Truven data is provided in Appendix H.

5 Research Design

There are two main empirical challenges with identifying the effects of advertising on prescriptions, prices and labor supply in this setting: endogeneity and statistical power. First, as advertising is a firm choice, it is likely targeted at consumers in a non-random way, in particular towards the potential consumers most likely to be responsive to it. Those most likely to be responsive to advertising might also be more likely to get prescribed anyway, eventually receiving any costs or benefits associated with those prescriptions. They could also be more depressed than a randomly selected television viewer, leading the researcher to find worse outcomes associated with advertising even though the causality runs the reverse direction.

A second challenge is statistical power, as advertising is thought to have generally small effects and outcomes are noisy. In particular, workers miss work for many reasons that have nothing to do with depression, advertising or antidepressants. For example, many workers may miss work in a particular area due to a local outbreak of influenza. It would be difficult in the data to know where and when every flu outbreak happens. Even if it happens in a way independent of advertising, it will add considerable noise to any estimates of the advertising effect on labor supply. Similar arguments can be made for vacation days, local

[14] The reader may be concerned that vacation days are included in missed days of work. To the extent that vacation days are random with respect to advertising, their inclusion in the dependent variable will serve to increase noise. If, on the other hand, advertising led to both increased vacation time and decreased sick time as a result of the patient feeling better, the effect of advertising on labor supply would be the result of two potentially offsetting effects, even though both effects would be welfare-positive. This would understate the extent to which advertising reduced undesirable absences, providing a conservative overall estimate for welfare purposes.

labor market conditions or weather conditions, for example. An additional complication that exacerbates power issues in this setting is that treatment of depression takes time to produce individual outcomes. Antidepressants, in particular, take on average two or three weeks before showing any benefits, and six to twelve weeks before they show maximum beneficial effects (Frazer et al. (2002)).

To address these challenges, this study exploits quasi-random variation in local advertising generated by the borders of DMAs. This design was first used in Shapiro (2018) to study the effects of television advertising on antidepressant demand, but is also used in Tuchman (2018) to study e-cigarette advertising, as well as in Spenkuch and Toniatti (2018) to study political advertising. Consumers who live on different sides of DMA borders face different levels of advertising, due to market factors elsewhere in their DMAs–DMAs are relatively large and the border areas are relatively small. However, the individuals immediately across the border from one another are otherwise more similar to each other than they are to other individuals further away but in their same DMA, making the cross-border comparison a clean way to identify the effect of the differential advertising. In this way, at the borders, observed advertising is 'out of equilibrium' from what firms would set advertising if they could micro-target very local areas and simulates an experiment.

Capturing this intuition, I estimate the causal effect of advertising on labor supply, controlling for unobservable geographic characteristics with border-specific time fixed effects. This allows unobservables to be spatially correlated in ways that are consistent with the dynamics of local labor supply. To control for individual-specific unobservables that impact labor supply, individual fixed effects are included. As a number of individual-specific factors and geographic time-specific factors having little to do with depression affect labor supply, these fixed effects will also help to decrease noise in the dependent variables of interest.

The top 120 DMAs contain 209 such borders, and 163 border areas make up no more than 35% of the total counties in the DMA over the course of this time period. Attention will be restricted to these borders that make up a smaller fraction of the whole DMA, as in Shapiro (2017). Each of these border pairs will be considered a separate experiment, with the magnitude of the treatment determined by the advertising in each DMA at a given time, measured in GRPs. Only the individuals residing in counties bordering each other will serve as controls for each other to partial out any time-specific local effects that are correlated with outcomes, including any national advertising. The level of an observation is an individual-month.

For an illustrative example, Figure 2 shows the Cleveland and Columbus DMAs in the state of Ohio. The border experiment considered is outlined in bold. People on the Cleveland side of the border might see more antidepressant ads due to factors affecting the city of Cleveland. Such factors could include things like changes in the rate of depression, home values, employment or crime rates in the city of Cleveland, all of which could be relevant to antidepressant demand and fluctuate over time within a market. To the extent that those factors also affect the people away from the city of Cleveland at the border of the DMA, they are assumed also to affect the people immediately on the opposite side of the border in the Columbus DMA equally. I compare how outcomes on the Cleveland side of the border change when when the Cleveland DMA receives a change in advertising GRPs relative to the Columbus DMA.

5.1 Econometric Model

To model the main effects of advertising on demand, let i index individuals, b index borders, and t index time in months. Let Y_{ibdt} be the outcome of interest for individual i, in border area b, in DMA d, in month t. Let GRP_{dt} indicate advertising, measured in gross rating points, in DMA d in month t. The effect of an increase in advertising GRP on outcome Y is estimated with regressions of the form

$$Y_{ibdt} = \beta_1 f_1(GRP_{dt}) + \beta_2 f_2(\sum_{\tau=t-t_0}^{t-1} GRP_\tau) + \alpha_i + \alpha_{bt} + \varepsilon_{ibdt}, \quad (1)$$

where β_1 and β_2 capture the causal effects of current and past advertising, respectively; α_i is an individual fixed effect; α_{bt} is a border area-month fixed effect and ε_{ibdt} is an econometric error term. I consider the outcomes related to both labor supply (days of work missed, likelihood of being compensated hourly, job separations) and potential mechanisms (new initiations of treatment, refills, prices, therapy visits). All prescription measures are in terms of category-wide rather than brand-specific prescriptions. I use two-way clustering to account for two forms of correlation between error terms when computing standard errors. First, conditional on the fixed effects, residual variation in advertising is perfectly correlated at the border-DMA-month. Second, from an sampling design standpoint, there are repeated measurements over time in Y at the individual level. As such, I two-way cluster by border-DMA-month and by individual (Abadie et al. (2017)).[15]

[15] If the identification strategy is correct, then conditional on the variables in the model, there is no se-

For the main results, I set $f_1(x) = f_2(x) = log(1+x)$, but alternative functional forms are considered in Appendix A. Additionally, I will consider past advertising to be the sum of GRP for the previous six months. Alternative numbers of months contributing to the effect of past advertising are considered in Appendix B. In the case of labor market outcomes, this is to account for the fact that it takes time for outcomes to materialize from depression treatment and that there is variance around exactly how much time. In the case of new prescriptions, allowing lagged advertising to have an effect accounts for advertising carry-over. That is, a consumer might watch an ad, but not see the physician for more than a month, and at that time remembers last month's ad.

For this approach to be useful in identifying advertising effects, two conditions must hold. First, there must be sufficient variation in advertising across the borders in the data. If all advertising variation were at the national level over time, the border-specific time fixed effects would sweep away all variation in advertising and standard errors would tend to infinity. Second, an individual's location with respect to border side must be quasi-random with respect to changes in potential outcomes.

5.2 Features and Limitations

A more detailed analysis accounting of the features and limitations of the approach is available in both Shapiro (2018) and Shapiro (2017). As the identification strategy in particular is not the main contribution of this study, I will focus here only on the most important aspects to validity and interpretation.

The largest feature of this approach is that the observed advertising levels at the border are quite different than they would be if firms micro-targeted advertising at individuals or counties rather than DMAs. That is, the variation at the border is driven by the equilibrium supply and demand in *other* markets. At the border of the Cleveland, OH DMA, viewers see antidepressant ads that were driven by a desire to reach viewers in metro Cleveland rather than the customers away from the city at the border of the DMA. If ads were micro-

rial correlation in the residual treatment variable. Otherwise, we could not be sure that the omitted lagged treatment variables were not confounding the estimated effects. As a result, the only error term correlation induced by the treatment under the identification assumptions is cross-sectional correlation between individuals within a treatment region and time. If the identification strategy is correct, serial correlation in the error term can only be driven by repeated observations within individual. As such, the correct clustering is not at the DMA level or at border-DMA level, but two-way by the border-DMA-month (for the cross-sectional correlation) and by individual (for the serial correlation). If the identification strategy is incorrect, then the first order problem is that the estimates will be inconsistent. The "correct" level of clustering in such a situation is somewhat second order given that the estimates would be wrong.

targeted to the county level, these consumers would likely see different ads. Similarly, on the Columbus, OH side of the DMA border, the advertising is largely driven by metro Columbus viewers, which is away from the border, again giving rise to rather different advertising at the Columbus border than if ads could be micro-targeted. If metro Columbus and metro Cleveland are sufficiently different from each other, these very similar consumers right on the border will get very different ads, even though their equilibrium micro-targeted ads would have been very similar. This gives a reasonable amount of variation away from what would be the equilibrium in the micro-targeted world while using the fact that these consumers across the border from one another are very similar to control for unobservable factors driving demand, prices and labor supply.

As is the case in experiments and instrumental variables strategies, one might be concerned that the treatment effects estimated by the border strategy will not be fully generalizable to the full population. In this case, the estimated effect will be local to those consumers who live in border areas. That is, the 'compliers' will be the set of people who live within the border sample, which is a group that can be characterized and compared with the population at large in a straightforward way. In Appendix C, I compare border areas with the full sample in terms of county-level observables. There is a considerable amount of overlapping support in observables. The typical border county is very similar to the typical non-border county. Additionally, Shapiro et al. (2018) show that specifications estimating advertising effects using the border population tend to be similar to specifications using the full population.

An additional potential limitation to this approach is that it relies crucially on variation in local advertising, which is often a remnant of the upfront market and might be systematically different from national network or cable advertising. In this market, there is a considerable amount of national advertising, meaning that much of the variation in advertising identifying the effects of interest is away from the zero-advertising counterfactual. For an average DMA-month, 7% of the advertising is local advertising, but there is considerable variation in that, with some DMA-months having no local advertising and some DMA-months having as much as 74%. The standard deviation of the percent of advertising that is local is 13.4%, meaning there is considerable variation both in local advertising and in the share of total advertising that is made up by local advertising in any given DMA-month. It is also important to note that local advertising copy is identical to national advertising copy in this category. To get an idea of the amount of variation in GRPs that is helpful for identification using this approach, Figure 3 shows a histogram of GRPs, net of the fixed

effects in the border approach, centered at the average GRP level of 227.27 and winsorized at the 0.1st and 99.9th percentiles. The standard deviation of residual GRPs is 34. This is exactly the variation that is used for identification of the main effects of interest.[16]

There are some threats to validity. First, it could be the case that consumers at the border are significant drivers of firm targeting relative to the rest of the DMA, violating the theory that advertisers are targeting the average of the DMA as a whole. In this case, firms might direct their advertising specifically to the DMAs that have border-sides with more depressed individuals. To address this concern, concurrent advertising is included in all labor supply regressions as a placebo test. Because addressing depression takes time, concurrent advertising should have no effect on labor supply. If concurrent advertising appears to be correlated with people missing more work, it would indicate this type of targeting. The main results will show that under the border approach, there is indeed no effect of concurrent advertising on labor supply. More naive approaches show a strong correlation in this direction, highlighting the importance of the identification strategy.

Second, this approach would be invalid if people on opposite sides of DMA borders were systematically different from one another in ways that were correlated with changes in prescriptions and labor supply. On one hand, people move infrequently and this approach includes individual fixed effects, which suggests that systematically different demographics should not pose a problem. On the other hand, there are some individual attributes that could change over time. As a result, I test whether the change in these attributes over time is predicted by changes in advertising by running the specification in Equation (1) with the attribute of interest as the dependent variable. Table 2 provides the result of this placebo test.[17] For variables, I use age, whether the individual is prescribed in the preceding six months, whether the individual terminates antidepressant treatment conditional on being prescribed in the past six months and whether the individual missed more than the population median number of work days in the preceding six months. These variables are included to test whether the firm can micro-target advertising specifically at the people most relevant to the depression market– people who were recently prescribed antidepressants or recently

[16]Past GRP is also partialed out in this specification. This shows that conditional on past advertising and all of the fixed effects in the model, current advertising has plenty of variation. By extension, this means that conditional on concurrent advertising and all of the fixed effects in the model, there must also be plenty of variation in past GRP. However, a histogram depicting residual past GRP is available from the author upon request.

[17]Each of these analyses is conducted using the sample from the main outcomes analysis conditional on the dependent variable being non-missing. Each dependent variable changes over time at the individual level, so the inclusion of the individual fixed effect does not render the result moot.

missed a lot of work, potentially due to depression. Current advertising does not predict any of these variables at the $p<0.05$ level. In particular, the fact that levels of prescriptions and missed days in the past six months exhibit similar trends across the borders suggests that the evolution of depression rates is similar across borders, lending even more credibility to the quasi-randomness of the variation in advertising directly across the border.

An additional similar placebo analysis is conducted on county-level demographic variables in Appendix D. Advertising changes at the border do not predict county-level demographic changes. Additionally, advertising levels at the border do not predict county-level demographic levels, lending credibility to the quasi-randomness of advertising at the borders of DMAs. The populations of individuals directly across borders from one another exhibit similar trends and levels of demographics. Together, these analyses support the notion that there are not significant differences in the populations, in demographics or in propensity to be depressed, across DMA borders that might drive a spurious correlation between advertising and labor supply.

Another final potential threat to identification is that there is some specific kind of systematic measurement error in the advertising data that would induce a spurious correlation between any kind of advertising and labor supply. This might be the case if advertising changes were systematically exaggerated in some DMAs relative to other DMAs due to flaws in the measurement technology. To address this, a final placebo analysis is presented in Appendix E with a placebo treatment, statin advertising, which is measured the same way as antidepressant advertising but is not expected to produce the outcome of interest. That changes in statin advertising do not predict changes in labor supply across DMA borders is reassuring that something about crossing the borders in the data by itself is not causing the changes in labor supply.[18]

A final concern is omitted firm variables that might be correlated with DTCA mechanically due to firms taking advantage of complementarity or substitutability between marketing levers. The most salient such variable is detailing to physicians. Detailing data is not available in this study. Shapiro (2018) finds that detailing and DTCA for antidepressants are not correlated at the DMA-month level, nor does crossing the DMA border discontinuously change detailing. Additionally, it finds that when DTCA became feasible in the late

[18] A related concern is that advertising for other drugs might be correlated with advertising for antidepressants and lead to other improvements in health that lead to improved labor supply that are not attributable to mental health or antidepressants. That is also considered in Appendix E. While there is not sufficient power to rule out labor market effects of other drug advertising, including such advertising in the regression does not eliminate the effect of antidepressant advertising on labor supply.

1990s, firms did not increase detailing. If that result holds for this time period as well, the omitted variables bias from detailing would be zero. As that study was for earlier years, this study cannot definitively rule out coordination between DTCA and detailing.

However, it is instructive to consider how such coordination would affect the results here. Since detailing is an interaction between physician and manufacturer, it cannot generate new patient visits to physicians on its own. Detailing could affect the physician's likelihood of prescribing any antidepressant given a DTCA-induced physician visit or the physician's choice of which drug to prescribe given such a visit.[19] If that were the case and DTCA and detailing were causally linked to each other, the estimates here would reflect the combined direct effect of DTCA on the outcome of interest with the indirect effect of DTCA through its causal effect on detailing. This combined effect is exactly the policy relevant effect when considering restrictions or subsidies for DTCA, as a restriction on DTCA would necessarily also change detailing strategy. Overall, previous literature finds little coordination between DTCA and detailing for antidepressants, and if there were such coordination, the combined effect would be the policy relevant one when considering restrictions (or subsidies) to DTCA. The combined effect, however, would be less informative to firms trying to set profit maximizing DTCA and detailing separately, which is not the main focus of this study.

6 Results

6.1 Labor Supply

6.1.1 Days of Work Missed

In this section I provide the main result of interest in the paper– the effect of DTCA on ex-post well-being using labor supply. I estimate equation (1) using missed days of work as the dependent variable.[20] In this case, any potential effects of DTCA on labor supply

[19] We might especially expect to see such an effect in the results of the effects of DTCA on switches, prices or the generic penetration rate in this paper. That we find a precise null effect on these dependent variables lends some credence to the hypothesis that DTCA and detailing are not particularly coordinated, as was found in Shapiro (2018).

[20] Here I note that the interpretation of the regression coefficient from a regression with levels on the left hand side and log on the right side is that a 100% increase in X leads to a $\hat{\beta}$ increase in Y in levels. I will express the results in the text with regards to a more plausible 10% increase in X and correspondingly divide $\hat{\beta}$ by ten. Using log(absent hours) instead of level of missed days and correspondingly interpreting the coefficient as an elasticity does not change the marginal effects significantly quantitatively or qualitatively.

should manifest with a lag, as antidepressants do not work instantaneously. As such, the coefficient of interest is the one attached to lagged DTCA. The coefficient on concurrent advertising is a placebo. Any significant effect of concurrent advertising on labor supply indicates targeting on an omitted variable by the firm, such as the level of depression in a given market.

Firms might well direct their advertising most at places where individuals have a high incidence of depression. In that case, a naive correlational analysis would find a spurious positive effect of current DTCA on current missed days of work. Mean reversion could then lead to a spurious negative effect on past advertising.

Table 3 presents the results. Column (1) provides the naive regression with no controls and no fixed effects. It indicates that work days missed are significantly increased by current DTCA and significantly decreased by past DTCA. These results are directionally consistent with the expected spurious result. In column (2), individual fixed effects are included. Both effects persist, but the effect of past DTCA is muted to some degree. In column (3), month fixed effects are added, which control for seasonal factors that are correlated with labor supply. For example, many families go on vacation during December or July. With the inclusion of month fixed effects, the estimate on current DTCA changes sign and becomes insignificant. The estimate on past DTCA persists, but is no longer statistically significant. The large standard errors suggest there is a lot of variation in month over month labor supply that has little to do with antidepressant DTCA and that generates significant noise. Additionally, due to the noise, large effects of concurrent advertising cannot be ruled out, which would indicate a placebo violation. Column (4) addresses both of these concerns by implementing the border strategy from equation (1). Despite losing 80% of the observations by focusing only on the borders, standard errors decrease by a factor of three.[21] The border-month fixed effects soak up significant noise. Current DTCA has no significant effect on days missed, and precision allows large positive effects can be ruled out, indicating that this specification performs well on the targeting placebo test. The main coefficient of interest, past DTCA has a significant and negative effect on days missed, suggesting that a 10% increase in advertising in the past six months leads to the average

Other functional forms also do not effect the results significantly. Those results are provided in Appendix A.

[21] In columns (1)-(3) the clustering of standard errors is also slightly different. To account for correlation in treatment and repeated observations, errors are two-way clustered at the DMA-month level an at the individual level. As it might be hypothesized that the increased power in column (4) is due to the difference in clustering, I re-ran column (4) with the same clustering as columns (1)-(3) and the standard error on past advertising was slightly larger at 0.0609, but still roughly a factor of three smaller than the corresponding standard error in column (3).

individual in the sample to miss 0.0138 fewer days of work per month. Column (5) shows that this effect appears to be coming from those who have missed a lot of work in the previous six months. While statistical power is not ideal in column (5), it provides suggestive evidence that advertising is operating on a margin of people who are actually experiencing the negative outcomes associated with depression in the last several months.[22][23]

The point estimate of column (4) of -0.1382 indicates that the average individual in the sample gains about 0.11 hours of monthly time at work from a 10% increase in the past 6 months of DTCA. Assuming this number applies to all working adults (about 145 million) and assuming the national average wage of $24/hour, a sustained 10% increase in DTCA for a year would lead to $769.5 million in increased wage benefits, or about $5.31 per working adult.[24] Figure 4 visually illustrates how days missed varies as advertising varies across borders. The 95% confidence interval on the total yearly benefits of a 10% increase in DTCA is [$99.3 million, $1.44 billion].

Is this effect size medically plausible? Woo et al. (2011) find that eight weeks of antidepressant treatment leads to decreased monthly absenteeism of about 1.94 work days per month.[25] If those numbers are credible and advertising selects roughly "average" patients afflicted with MDD into prescriptions, then the yearly advertising marginal prescriptions from a 10% increase in DTCA would account for about $387 million per year.[26] This is just over half of the $769.5 million in total benefits of increased labor supply estimated above and is firmly within the confidence interval. This also illustrates that the effect size is within a (medically) plausible range, especially if non-prescription mechanisms also cause

[22] Ideally a richer characterization of heterogeneity would be possible. However, a median split was used to maximize the chance of reasonable statistical power, given the limited power on the main effect.

[23] I note here that I cannot statistically distinguish the estimates in column (3) from the estimates in column (4), even though the point estimate in column (4) is roughly half as large. This is partially due to the large amount of noise in the estimates in column (3), but it is also consistent with the panel structure of the data and individual and month fixed effects being sufficient to remove contamination, while the border strategy adds additional statistical power. One might also be concerned with the estimates in column (3) due to the larger point estimate on concurrent advertising which also has a larger standard error, making it difficult to rule out significant firm targeting at that level. I will proceed using the estimates in column (4) as the preferred estimates, as they are the both the most conservative with respect to controlling for confounds, perform better on the placebo test on concurrent advertising and exhibit the best statistical power.

[24] I note here that using $24 as the basis for the social hourly benefit of work assumes that workers are paid their marginal product of labor. If employers have market power in the labor market and pay workers less than their marginal products, this will leave out benefits to employers.

[25] Page 479, Table 4. This number is the sum of the effect on health related absences and "all cause" absences. To get the effect size, subtract the average number of missed days in week 8 from the average number of missed days in week 0.

[26] This number comes from the 86,500 advertising marginal individuals, multiplied by 1.94 days per month, multiplied by 8 hours of work per day, multiplied by 12 months per year, multiplied by $24 per hour wage.

some change in labor supply.

6.1.2 Other Labor Supply Margins

While the data is most suitable for studying the number of days missed from work, it is also possible that advertising affects other margins of labor supply, such as whether or not individuals are employed or whether employees are more or less likely to become salaried. Since the unemployed are unobservable in the data, it is not possible to measure movements into the labor force. However, job separations are measurable. Individuals churn from the sample when they leave the firm. Additionally, the data contain indicators for whether the employee is an hourly or salaried employee. As these outcomes are more extreme behaviors, it might be expected that any effect of advertising would take longer to manifest and thus be more difficult to detect. Table 4 presents the results using these variables as the dependent variables.

Column (1) presents the results for whether or not an employee is hourly. There is no effect of concurrent advertising on the propensity of an employee to be compensated hourly, as expected. Additionally, there is a small and statistically significant negative effect of past advertising on the propensity to be an hourly worker. The point estimate suggests that a 10% increase in advertising leads to a 0.02% decrease in being hourly, which might be interpreted as an increase in the likelihood of being promoted. Column (2) presents the effect on the likelihood of churning from the sample. The effect on both concurrent and lagged advertising is near zero and precisely estimated. Any meaningful effects of advertising on job separations in the medium term can be ruled out. Of course, this does not rule out potential longer-run effects.

6.2 Mechanisms

6.2.1 Potential Mechanisms

The main results show that past advertising for antidepressants causes individuals to miss fewer days of work. While that result is novel and important, the mechanism through which that advertising generates labor supply is also interesting and could provide information as to the optimal policy in this setting. I propose two categories of mechanisms: behavior change with respect to prescription treatment and non-prescription strategies for dealing with depression. As the data are medical claims, prescription behaviors are more

straightforward to measure than non-prescription behaviors. In terms of prescription related mechanisms, I consider whether advertising causes new initiations of treatment, the propensity to refill the prescription, switches between medications and changes in propensity to get expensive or inexpensive drugs measured in co-pays, prices and the probability of choosing a generic. A prescription is defined as a new initiation if the individual was not prescribed in the previous month but is prescribed in the current month.[27] A prescription is defined as a refill if there was a prescription in the preceding month. A switch is defined as a prescription for one antidepressant in the previous month and a prescription for a different antidepressant in the current month. For non-prescription related mechanisms, whether advertising causes visits to therapists is considered.

Table 5 presents the results. Each column uses the border approach on the population relevant to the outcome in question. In column (1), current month DTCA causes new initiations of antidepressant treatment. A 10% increase in current antidepressant GRPs leads to about a 0.00031 (0.31%) increase in the probability of a new prescription, and past advertising has no effect. This amounts to about a 0.031 elasticity and is quantitatively similar with the category expansive effects of DTCA found in most of the recent literature Shapiro (2018); Iizuka and Jin (2005); Sinkinson and Starc (2017) and smaller than the effect found in Alpert et al. (2015).[28][29] This effect size is also very much in line with the distribution of advertising elasticities estimated in Shapiro et al. (2018) across categories outside of pharma.

In column (2), current month DTCA causes a slightly lower propensity to refill a prescription. Importantly, this empirical exercise is conditioned on having received a prescription the previous month for any reason and controls for past advertising. This approach

[27] One might be concerned about slight variations on the date of refill causing me to code a month as non-prescription that should have been coded with a prescription. To deal with this possibility, if I observe a three month sequence indicating prescription - no prescription - prescription, I infer that the middle month in fact was a month prescribed. As a result, in this situation, the third month will not be coded as a "new" initiation of treatment.

[28] This effect size implies that a sustained yearly 10% increase in advertising GRPs would lead to about 86,500 individuals to initiate treatment. Multiplying this by the average price of antidepressants and the average length of treatment yields about $32 million in prescription cost as a result of the 10% increase in advertising.

[29] The effect of past advertising on new prescriptions, at first blush, suggests that there is no carry-over effect of advertising. However, that should be interpreted with caution. In order for it to be possible to get a new prescription, one must not have been prescribed in the previous month. Receiving a large treatment of advertising in the past month but not getting prescribed suggests that this treatment, past advertising plus no past prescription, could be negatively selected. Additionally, even if the result were not subject to this limitation, it does not imply zero dynamic effect of advertising. It merely implies that all dynamic effects of advertising must operate through state-dependence rather than through carry-over.

will control for any selection induced by previous advertising, should it exist. As such, the coefficient on concurrent advertising should be interpreted as holding fixed past advertising, the extent to which concurrent advertising causes increased or decreased likelihood of refilling an existing prescription from the previous month. The point estimate on current advertising is negative and small, indicating that a 10% increase in current advertising decreases adherence to treatment by 0.05%. The estimate is statistically significant and small in magnitude, consistent with Cardon and Showalter (2015), Donohue et al. (2004) and Wosinska (2005).[30] Note that this effect is consistent with the side effects list in the ad persuading a consumer to stop taking his or his pills. It is *not consistent* with advertising selecting patients more likely to quit - such an effect would be detected if the effect of *past* advertising on current refills were negative. Here, past advertising has no significant impact on refill behavior, suggesting those who had more advertising in the past are not more or less likely to discontinue treatment.

In column (3), neither current nor past advertising cause a significant amount of switching between drugs for those already prescribed. The 95% edge of the confidence interval suggests a 10% increase in advertising would lead to a 0.23% (0.08 percentage point) increase in the likelihood of switching. In columns (4) and (5), I find that advertising does not cause the choice of a cheaper or more expensive treatment, conditional on getting treated. In column (6), I find that advertising does not make individuals more likely to get branded over generic drugs. All of these effects are near zero and precisely estimated. That is, at the 95% end (most "pessimistic") of the confidence interval, a 10% increase in advertising implies a $0.048 increase in the average co-payment and a $0.015 increase in the average price paid by the insurer.[31] At the 5% end (most "pessimistic") end of the confidence in-

[30]The welfare effect of non-adherence is ambiguous and depends on the particular mechanism. In particular, if non-adherence is rational and in response to lack of benefit and high adverse effects, decreased adherence would be welfare positive. If non-adherence were due to incorrect expectations about the relative risks and benefits, it would be welfare negative. If non-adherence were due to inattention, it would be welfare negative. However, it would be difficult to argue that advertising-marginal non-adherence is due to inattention, since a causal effect of advertising requires attention in this setting. All of these specific mechanisms surrounding adherence are very important but beyond the scope of this study.To the extent that the net effect of non-adherence found here is either welfare positive or negative, it is assumed to be reflected in the labor supply estimates above. These results do speak to the incremental cost from advertising marginal changes in adherence.

[31]I point out here that I am assessing effects on transacted prices rather than on equilibrium list prices. Month-to-month co-payments and prices tend not to vary due to the structure of insurance company-manufacturer bargaining. The effects here reflect whether advertising causes individuals to choose drugs that are already more or less expensive rather than if changes in DTCA cause the profit-maximizing menu of prices from the manufacturer to change. Empirically, the menu of prices for drugs is nationally consistent within an insurer-plan type despite considerable differences in DTCA, suggesting the absence of such an

terval, a 10% increase in advertising implies a 0.138% (0.08 percentage) decrease in the likelihood of choosing a generic over a brand.[32]

In column (7), I consider whether advertising causes an increase in non-drug treatment of depression in the form of visits to a cognitive behavioral therapist. I find no statistically significant effect of either past or current advertising on visits to a therapist. The effect on concurrent advertising is near zero, with a 10% increase of advertising implying between a 0.6% decrease and 0.5% increase in the likelihood of a therapy visit. However, it may take time to schedule an appointment with a therapist. The effect on lagged advertising is also near zero and more precise, with a 10% increase in past advertising implying between a 0.2% decrease and a 0.18% increase in therapy visits. While the point estimates imply no effect, increased therapy visits contributing to the estimated effect of advertising on labor supply cannot be ruled out entirely.

That the primary effect of advertising on prescriptions is on new initiations of treatment, and I find little effect on switches or on the price conditional on treatment is consistent with the idea that advertising causes significant positive spillovers on rival demand, as in Shapiro (2018). This may lead to an under-investment in advertising from a social perspective as firms may free-ride on rival efforts. These results are consistent with new initiations of treatment contributing to the effect of advertising on labor supply, but do not entirely rule out the possibility of non-prescription related mechanisms also contributing.

6.2.2 Do the Potential Mechanisms Co-Occur with the Labor Supply Effect?

While the above section largely rules out mechanisms related to switching drugs, picking more expensive drugs or initiation of cognitive behavioral therapy, two potential mechanisms remain: increased antidepressant initiations and decreased refills. It could be the case, however, that the effect of initiation of treatment happens on one fraction of the population treated with advertising while the labor supply effect happens on a completely separate fraction of the population treated with advertising. This could similarly be true with the effect on refills. As a result, it is important to know whether these effects co-occur with the labor supply effect at the individual level.[33]

effect.

[32] Together, these results together are somewhat different from those found in Dave and Saffer (2012), which does not employ quasi-random variation in advertising.

[33] It is important to note that even if the two effects co-occur, it need not be the case that the prescriptions caused the labor supply. For example, the advertising could have led to both a new prescription and increased family attention in the same individual. It could be that the prescription had no effect, but the family attention

To address this possibility, I test the effect of advertising on two new outcomes: (1) the product of days missed at work this month and the new initiation of treatment in the past six months and (2) the product of days missed at work this month and the discontinuation of treatment in the past six months. If the new initiations of treatment caused by advertising and the labor supply improvements caused by advertising happen in different segments of the treated population, then past advertising would have no effect on the product of missed days and initiations. If the effects were correlated at the individual level, past advertising would have a negative effect on the product, as advertising has a positive effect on prescriptions but a negative effect on days missed of work. This is similarly the case for discontinuations. As advertising increases discontinuations and decreases missed days of work, if the effect co-occurs in the same segment, we should expect a negative effect of past advertising on the product. If the effects are in separate segments, we would expect no effect.

Table 6 presents the results. Column (1) shows that the effect of past advertising on the product of missed days and initiations in the past six months is negative and significant, indicating that the effect of advertising on prescriptions and the effect of advertising on missed days of work co-occur at the individual level. Column (2) shows that the effect of past advertising on the product of missed days and discontinuation of treatment in the past six months is also negative, but is small and not statistically significant. The effect could simply be too small to detect, but the hypothesis that the discontinuation effect and the labor supply effect happen in different populations cannot be rejected.

The evidence presented here is consistent with the theory that advertising drives new initiations of treatment which lead to future improvements in labor supply. Further corroborating evidence is presented in Appendix B which shows that the specific lagged timing of the labor supply effect is consistent with the science of antidepressants. However, neither of these rule out the possibility that non-prescription mechanisms could also be caused by advertising and lead to labor supply improvements. While point estimates indicate no effect of advertising on therapy visits, I cannot entirely rule out some effect. Additionally, the data do not contain information about non-medical behavioral change that could impact depression symptoms, such as exercise or family intervention.

had a large effect on labor supply. Such a story would not be detectable in this data and would be difficult to detect in any data.

6.2.3 Discussion of Mechanisms Overall

Each individual mechanism need not be especially large in order to explain the size of the aggregate estimated labor supply effect. Taking as a benchmark the survey evidence in Kessler et al. (2003) that roughly half of people afflicted with depression are untreated,[34] then roughly 8% of individuals should be considered as potential beneficiaries of the labor supply effect. If the total labor supply benefit were evenly spread out among these people, it implies a $66.34 per year wage benefit per person, or about 2.75 hours per year, which is reasonably modest.[35] Alternatively, we could consider that the 86,500 individuals who are marginal to prescriptions make up roughly half of the effect, as described above, and roughly the same number of individuals experience a quantitatively similar effect from other mechanisms. This formulation requires only 1.5% of depressed individuals to exhibit any behavioral response at all.[36] Broadly speaking, this range of potential per person benefits reflects economically plausible effect sizes. Even though the aggregate effect of DTCA on absenteeism may seem quite large, the various possibilities imply reasonable individual level effect sizes.

6.3 Are the Benefits Worth The Cost?

Some possible costs and benefits of DTCA are either not measured in this study or do not come with easily calculable dollar values. On the benefits side, since employees are often paid less than their marginal products of labor, increased hours of work will translate into more benefits for firms than implied by average wages. Also, some individuals may have no change in labor supply, but become more productive while they are at work due to being treated.[37] The evidence of an effect of past advertising on propensity to be compensated hourly is suggestive of this possibility, but it is difficult to assign a dollar value. Additionally, this study only includes those who are employed. It could be that those not employed or retired see benefits in improved leisure or subjective well-being. Those who

[34] Recent evidence suggests that over half of suicides are committed by people with no diagnosed mental health conditions. (last accessed 6/9/2018).

[35] To obtain the estimate assuming advertising impacts all those who are depressed and untreated, I take the $769.5 million and divide it by the (0.08 untreated depressed share)*(145 million).

[36] (86,500 individuals through prescriptions + 86,500 individuals through other mechanisms)/(8% of Workers times 145 million total workers)

[37] For example, Rost et al. (2004) finds that individuals randomized to receive improved depression treatment show significant improvement above typical treatment in both work productivity and absenteeism.

are employed could also see such benefits, which we cannot measure.[38]

In terms of unmeasured costs, consumers pay the cost of any adverse effects incurred from the marginal prescriptions. In Appendix G, I show using a variety of metrics from the claims data, there is no detectable effect of advertising on either adverse effects or propensity to discontinue in the near future (which would indicate advertising selecting more poorly suited individuals). Additionally, some people may directly dislike watching television ads for antidepressants (see, for example, Wilbur et al. (2013)).[39] As a result of these limitations, I make no claim of ability to do a complete cost-benefit analysis or full welfare analysis.

Despite the limitations in what can be measured, this study can say whether the labor supply benefits are sufficient to justify the total cost of the marginal prescriptions, which is a highly policy relevant comparison. It is also conceptually relevant as it gives a benchmark of how big unmeasured costs must be to fill the gap between cost and benefit to flip the conclusions.

While consumers only pay a portion of the drug price, society (through insurers) pays the full price. I assume the average number of prescriptions resulting from a new prescription is 6, which is a full course of treatment and roughly the average number of prescriptions resulting from a new prescription in the data. I evaluate the cost of the incremental quantity at the average price and co-payments of antidepressants in the data, $62.48 and $11.22.[40] Assuming these results apply to all adults in the United States, 230 million, a 10% increase in advertising leads to 520,000 new antidepressant prescriptions to about 86,500 individuals, which yields approximately $32.4 million in total costs of new prescriptions per year, about $5.8 million of which is paid in co-pays by the consumer.[41] The 95% confidence interval associated with this cost estimate is [$49,871, $64.4 million]. In terms of the negative effect of advertising on refills, the estimate implies a total yearly savings from a 10%

[38] For example, Kocsis et al. (2002) finds that treatment with the SSRI sertraline provides significant improvement in measures of social adjustment and survey-measured subjective well-being relative to placebo.

[39] If the alternative to antidepressant DTCA were additional television programming, the effect might be significant. However, if we were to remove antidepressant DTCA, it is much more likely the viewer would get a different advertisement, which would presumably imply a smaller welfare effect.

[40] As the estimated effect of advertising on prices and co-payments was null and reasonably precise, assuming the average prices in this scenario is reasonable.

[41] To reach these numbers, note that a 10% increase in DTCA corresponds with one-tenth the estimated coefficient on log(GRP). The assumed six months of treatment following an initial prescription is the average observed in the data. So we take one-tenth the coefficient on log(GRP), multiply by six months of prescriptions, multiply by the price of a prescription and multiply by twelve months per year.

increase in DTCA of about $6 million, or about 97,152 prescriptions.[42] Recall that the wage benefit of a 10% increase in DTCA was about $769.5 million, with a confidence interval of [$99.3 million, $1.44 billion], implying that we can comfortably reject that the wage benefits are less than the cost of incremental prescriptions.

6.4 Cautions & Limitations

The reader should take some caution in interpreting these results for policy. First and foremost, they only apply to DTCA as it relates to antidepressant treatment. DTCA for another drug category might have a different interaction with patient selection on potential to gain from treatment. Depression is thought by many physicians to be under-treated due to the stigmatization, which leaves plenty of opportunity for welfare-increasing market expansion. For cholesterol-lowering drugs or erectile dysfunction drugs, that might be quite different. However, these estimates do provide evidence that a blanket ban on all DTCA would be costly in the depression space. Put differently, if a policy maker wanted to institute a blanket ban on DTCA, he or she should consider how to recover the social losses that would cause in the relevant population. Additionally, that advertising draws from a desirable margin in this space might make us more optimistic that a similar mechanism of advertising effectiveness is at play for other products.

Second, the border identification strategy identifies an effect local to the borders of television markets. It is possible that the true effect of advertising away from the borders of TV markets is different from the effect at the border. One reason to worry less about this is that the specification using all of the data, including away from the border with month and individual fixed effects, is consistent with the result at the border, simply with more noise. Additionally, there are many borders in these data with many types of individuals that span the support of individual characteristics in the full data. For the non-border counties to flip the cost-benefit in this case would require drastically different advertising effects, both on labor supply and on prescriptions. The effect on prescriptions would need to be much larger and the effect on labor supply much smaller.

Third, all calculations on costs and benefits are computed at the point estimates, but each of these is a function of estimated values with standard errors. Taking a pessimistic view of both the benefits and using the 5% end of the confidence interval for labor supply results and a 95% end of the confidence interval for new prescriptions, the benefits still

[42] The dollar value of one advertising-marginal prescription averted is about $62.48. Since 8% of adults are taking antidepressants at any given time, the estimate would apply to about 18.4 million individuals.

outweigh the costs, but the gap between them is muted. For a 10% increase in DTCA using these maximally pessimistic estimates, cost of new prescriptions is $64.4 million and the benefit of those prescriptions is $99.3 million. Of course this is a highly pessimistic view, and the joint probability of the most pessimistic estimates on both labor supply and costs of new prescriptions is tiny, and even in that case, the benefits outweigh the costs. A further analysis to improve statistical power with additional control variables is provided in Appendix F.

Fourth, it is possible that a substantial amount of the measured benefit of DTCA is operating through channels other than prescriptions. If that is the case, the optimal policy for addressing the remaining untreated depressed population might be through public service announcements rather than through vastly expanded or subsidized branded DTCA.

7 Conclusion

In this paper, I provide the first quasi-experimental evidence that advertising tends to help consumers meet their needs. Using the variation in advertising generated by the quasi-random placement of DMA borders, I find that past antidepressant DTCA decreases days absent from work. I find that these labor supply improvements co-occur with new initiations of antidepressant treatment. Meanwhile, I rule out large effects of advertising on switches between drugs, the prices of the drugs chosen or on visits to a therapist. The total wage benefits of labor supply from a 10% increase in DTCA, at $769.5 million are substantially larger than the $32.4 million in direct costs of the marginal prescriptions generated. Further, I show that the estimates are within a plausible range given the clinical evidence on antidepressant effectiveness at improving workplace absenteeism.

These results highlight the importance of understanding which types of consumers are affected by advertising and measuring how much these consumers benefit from marginal treatment when assessing the desirability of DTCA. In the case of antidepressants, the marginal consumers stand to gain and do gain from treatment in a way that far exceeds the measurable components of social cost. While this result might not be the same across different drug categories, it highlights that a more nuanced approach than a blanket ban on DTCA might be desirable.

The relative magnitude of the different mechanisms behind the advertising-marginal labor supply is very interesting and worthy of further study. I explore possible mechanisms and show that the range of potential contribution by each mechanism implied by my esti-

mates is medically reasonable. However, statistical power makes precisely pinning down these relative contributions of different mechanisms difficult in the current data.

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Tables

Table 1: Summary Stats

	Mean	SD	N
Age	45.39	10.63	68,527,405
GRP	227.3	148.7	5,428
Days Absent	2.375	3.155	17,593,438
Compensation (1=Hourly, 0=Salary)	0.361	0.480	68,527,405
Job Separation	0.057	0.232	73,490,466
Antidepressant Rx	0.081	0.273	68,527,405
New Antidepressant Rx	0.009	0.097	68,527,405
Renewal Rx	0.885	0.319	5,522,979
Drug Switch	0.352	0.478	4,889,121
Co-payment	11.22	12.49	5,855,154
Price	62.48	69.58	5,855,154
Generic Rate	0.591	0.492	5,551,058
Therapy Visits	0.022	0.147	66,793,937
Price	62.48	69.58	5,855,154

Table 2: Placebo Regressions

	Age	Prescribed Past 6	Churn Past 6	High Absentee
$Log(1+GRP)$	-0.0002	0.0005	0.00477	0.00264
	(0.00085)	(0.00043)	(0.00345)	(0.00429)
Individual FEs	x	x	x	x
Border-Month FEs	x	x	x	x
Mean DV	45.97566	0.11161	0.38258	0.63196
R-squared	0.99923	0.74793	0.37623	0.51123
Observations	12,731,985	12,746,153	1,419,929	2,857,781

Each column represents a regression with the given variable as the dependent variable. In addition to concurrent advertising, the independent variables are individual fixed effects and border-month fixed effects as in the main specifications. These regressions show that changes in these variables are not systematically predicted by changes in antidepressant advertising across the border, which is the identifying variation in advertising used to generate the main effects of interest. Log of past advertising is also included in the regression though not reported, as some of these variables, such as Prescribed Past 6 and Churn Past 6, are potential outcomes of past advertising, though not concurrent advertising. Note that with individual fixed effects, if an individual has the same dependent variable value for all observations, that individual will be dropped, explaining the different numbers of observations between columns where the beginning sample is the same.

Table 3: Labor Supply - Missed Days of Work

	(1)	(2)	(3)	(4)	(5)
$Log(1+GRP)$	0.2100	0.2518	-0.0650	-0.0339	-0.0066
	(0.0990)	(0.0768)	(0.0904)	(0.0288)	(0.0255)
x$HighAbsentee$					-0.0694
					(0.0516)
$Log(1+GRP_{past})$	-0.5356	-0.3710	-0.2757	-0.1382	-0.05923
	(0.2886)	(0.1477)	(0.1830)	(0.0602)	(0.0519)
x$HighAbsentee$					-0.2086
					(0.12134)
Individual FEs		x	x	x	x
Month FEs			x		
Border-Month FEs				x	x
Mean DV	2.432	2.432	2.432	2.876	2.876
R-squared	0.00639	0.27876	0.32319	0.35549	0.36187
Observations	16,310,368	16,303,199	16,303,199	3,363,046	3,362,328

Standard errors in columns (1)-(3) are two-way clustered. First, they are clustered by (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. Standard errors in columns (4)-(5) 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. If I adopt the exact clustering from columns (1)-(3) into the specification of column (4) to facilitate an apples-to-apples comparison of precision, the standard error on the log of past advertising coefficient is slightly higher, at 0.0609.

Table 4: Labor Supply - Compensation Type and Job Separations

	(1) Hourly	(2) Separation
$Log(1+GRP)$	-0.0004	-0.00007
	(0.0004)	(0.0001)
$Log(1+GRP_{past})$	-0.0023	8.5e-7
	(0.0010)	(0.0008)
Individual FEs	x	x
Border-Month FEs	x	x
Mean DV	0.3653	0.0527
R-squared	0.9785	0.9953
Observations	11,599,609	12,734,273

The DV in Column (1) is an indicator for whether or not the employee is compesnated hourly. The DV in Column (2) is an indicator for whether the employee leaves the sample in the month in question, indicating a separation from the employer. The 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. Note that with individual fixed effects, if an individual has the same dependent variable value for all observations, that individual will be dropped, explaining the different numbers of observations between columns where the beginning sample is the same.

Table 5: Potential Mechanisms

	(1) NewRX	(2) RenewalRx	(3) Switch	(4) Copay	(5) Price	(6) GenericRate	(7) Therapy
$Log(1+GRP)$	0.00031	-0.00441	0.00286	0.1499	-0.61605	-0.00335	-0.00011
	(0.00015)	(0.00213)	(0.0028)	(0.16599)	(0.38631)	(0.00246)	(0.0006)
$Log(1+GRP_{past})$	-0.00019	-0.00081	0.00666				-0.00002
	(0.00027)	(0.00414)	(0.0047)				(0.0002)
Individual FEs	x	x	x				x
Border-Side FEs				x	x	x	
Border-Month FEs	x	x	x	x	x	x	x
Mean DV	0.0099	0.8919	0.3615	$10.84	$61.96	0.59867	0.0212
R-squared	0.0745	0.1335	0.19234	0.0686	0.02373	0.03573	0.44033
Observations	12,064,669	1,018,908	906,012	1,053,769	1,053,769	1,054,561	12,438,333

Column (2) is limited to observations where there was an antidepressant prescription in the previous month. Columns (3) is limited to observations with a prescription in both the previous month and the current month. Columns (4)-(6) are limited observations where there is an antidepressant prescription in the current month. 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. Note that with individual fixed effects, if an individual has the same dependent variable value for all observations, that individual will be dropped, explaining the different numbers of observations between columns where the beginning sample is the same.

Table 6: Do Prescription Behaviors Co-Occur with Labor Supply?

	(1) Days x Initiations	(2) Days x Discontinuations
$Log(1+GRP_{past})$	-0.04335	-0.01049
	(0.0176)	(0.0117)
Mean DV	0.1737	0.1313
R-squared	0.2641	0.2695
Observations	2,305,356	2,876,150

Border strategy for both columns. In column (1) the dependent variable is days missed in the current period times an indicator for whether the individual initiated treatment in the past six months. In column (2) the dependent variable is days missed in the current period times an indicator for whether the individual discontinued treatment in the past six months. In Clustered standard errors in parentheses

Figures

Figure 1: Antidepressant GRPs

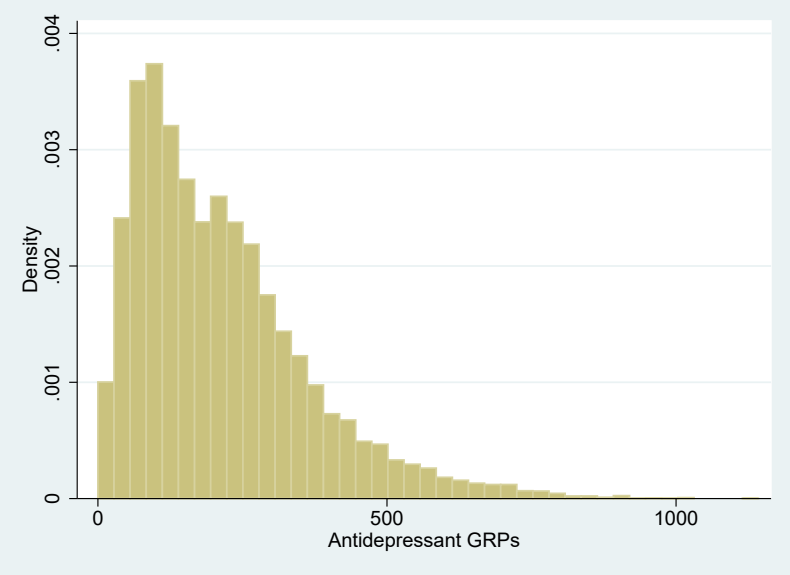


Figure 2: Ohio and DMA Border Example

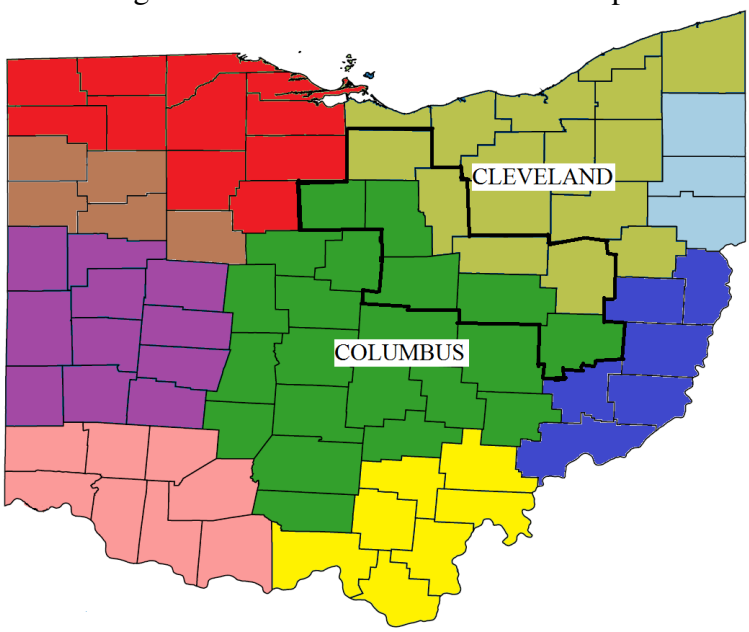


Figure 3: Antidepressant DTCA GRP variation across Borders - SD 34

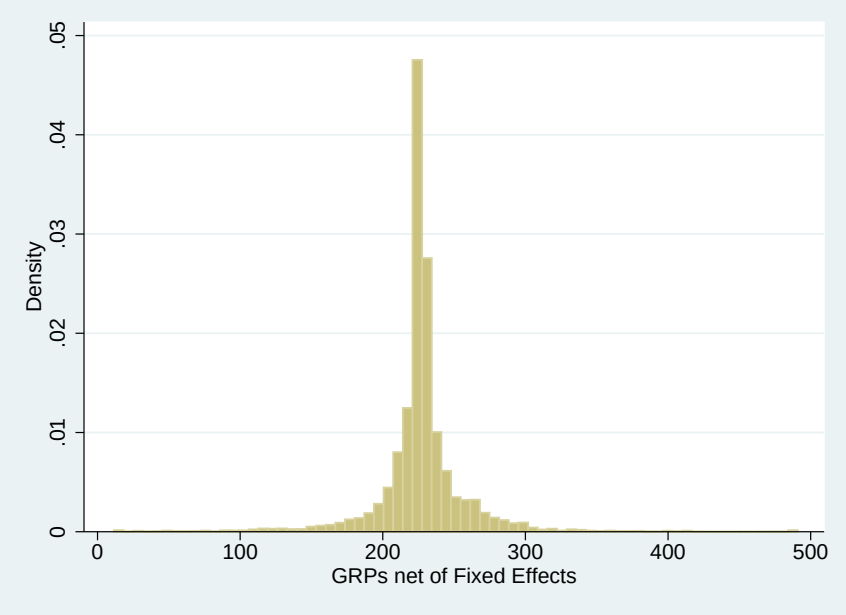
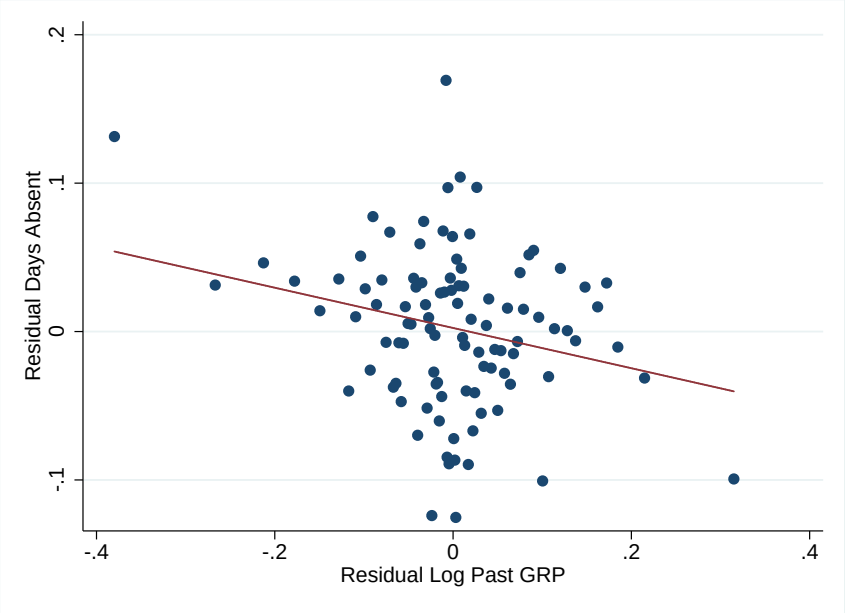


Figure 4: Days Absent Versus Log Past DTCA



Binned scatterplot with 100 bins. Variables are net of fixed effects included in the model and centered at sample means. For readability, borders with minimal residual advertising variation are removed. This is done by including only borders with greater than median standard deviation of residual advertising variation across the sample.

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

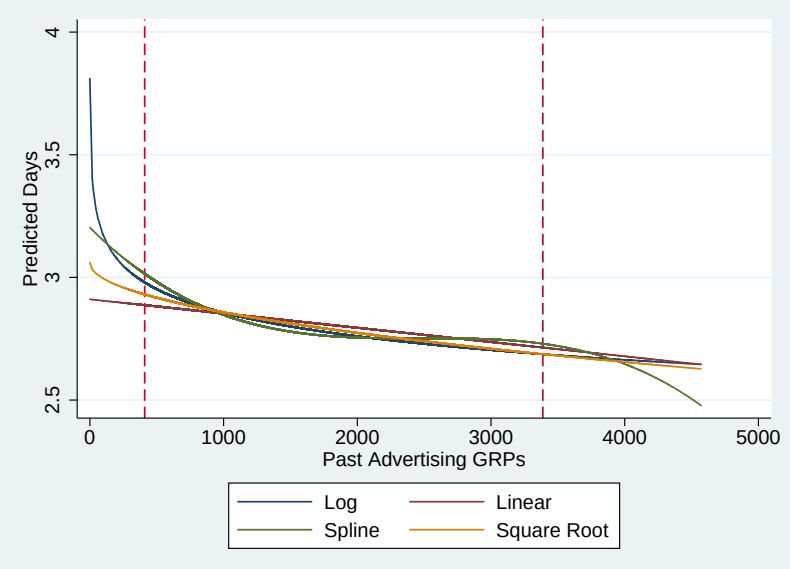
In this sub-appendix, I consider alternative functional forms of advertising to see if specifying the response to advertising in log form is important to the conclusions in the paper. I show the results of regressions using the logarithmic function of past advertising, a linear function of past advertising, a cubic function of past advertising and a square root of past advertising. As the coefficients are not directly comparable, 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 three functional forms are very similar. The choice of log function is not pivotal to the qualitative or quantitative results in this study. Detailed regression results are available from the author upon request.

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



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 et al. (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, 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, and death rates. 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.

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%]
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	52.9%	46.5%
	[19.0%, 80.5%]	[14.8%, 77.8%]
% 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.

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 a statistically significant way, nor are the point estimates significant in magnitude. 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
$Log(1+GRP)$	-5.5326	0.0020	0.0008	-0.0004	0.0005	-0.0042	0.0049
	(50.8684)	(0.0033)	(0.0010)	(0.0011)	(0.0010)	(0.0049)	(0.0117)
Mean DV	65,900						
R-squared	0.999	0.989	0.997	0.995	0.999	0.975	0.950
Observations	27,445	27,402	27,445	27,445	27,445	27,445	20,229

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
$Log(1+GRP)$	-2040	-0.0208	-0.0087	-0.0174	-0.0072	0.0028	0.0276
	(3890)	(0.0190)	(0.0129)	(0.0138)	(0.0261)	(0.0266)	(0.0361)
Mean DV	65,900						
R-squared	0.603	0.683	0.678	0.511	0.788	0.502	0.503
Observations	27,445	27,402	27,445	27,445	27,445	27,445	20,229

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 3, 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.1 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.1 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 et al. (2014).

Results of this analysis are in Table F.1. Column (1) replicates column (4) of Table 3 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 et al. (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 - Adverse Effects

One potential cost associated with advertising-marginal prescriptions that is difficult to put a dollar value on is adverse effects. In particular, David et al. (2010) and Cardon and Showalter (2015) point out that advertising-marginal prescriptions could be worse matches than average prescriptions and result in a greater incidence of adverse effects.[43]

Here, I evaluate the effect of advertising on appropriateness of treatment using six different measures. First, I measure the effect of advertising on the propensity to complete a full course of treatment (six months) conditional on a new prescription. If advertising drives worse matches, then markets with higher advertising should see higher rates of churn in the first six months, prior to completing a full course. Second, the claims data include information on adverse effects reported to physicians. I evaluate these in both a forward-looking and backward-looking way, for antidepressant-related adverse effects and for adverse effects related to any drug.

[43] I note here that any particularly severe adverse effects would likely mute the effect of advertising on labor supply and thus already be accounted for in the analysis above. As such, the effects I measure here are almost by definition second order.

The first adverse effect-related measure is forward-looking adverse effects for any drug (FLAE - Any). In this case, the dependent variable is an indicator for whether or not an adverse effect is reported in the six months following a new prescription. It is important to consider other drug adverse effects rather than just antidepressant-related adverse effects, as some adverse effects could come from drug interactions. That is, a bad interaction between an antidepressant and a statin could be caused by antidepressant advertising. The second measure I evaluate is forward-looking adverse effects specifically for antidepressants (FLAE - Antidep). These two measures are only comparing individuals who began treatment in the month in question and looking forward to see if those in higher advertising markets were more likely to experience adverse effects.

I also consider backward-looking adverse effects (BLAE - Any) and (BLAE - Antidep). In this case, the dependent variable is whether or not an adverse effect was observed in the current period. All observations are included, whether or not a prescription was filled, and both concurrent and lagged advertising are of interest. In this way we can test whether the absolute number of adverse effects increased, rather than just the likelihood of adverse effects, given a prescription. Concurrent advertising could affect concurrent reporting of adverse effects only through those who were already prescribed in previous periods. Past advertising is related to the total number of advertising-marginal adverse effects. However, it is a composition of two potential effects. First, past advertising could drive lower current adverse effects by decreasing adherence among those who were experiencing adverse effects previously. Second, past advertising could drive higher adverse effects through its effect on new prescriptions, and some fraction of new prescriptions will inevitably come with adverse effects. As such, the net effect can be seen as the net effect of advertising on adverse effects through those two channels.

Table G.1 presents the results. Column (1) shows that DTCA predicts a reduced chance of an incomplete course of treatment, but is statistically insignificant. This suggests that the advertising-marginal are about equally likely to complete a course of treatment as the average new patient, and if anything they are less likely to churn. Columns (2) and (3) show no effect of advertising on the likelihood of observing reported adverse effects (in general or antidepressant-related) in the six months following a new prescription. Columns (4) and (5) show no significant effect of concurrent advertising on adverse effect reporting. Column (4) shows no significant effect of past advertising on concurrent general adverse effect reporting. Column (5) shows a negative effect of past advertising on antidepressant adverse effect reporting. This suggests that any effect of advertising on new adverse effects

is more than offset by any effect of advertising on adherence for those likely to have adverse effects.

Taken together, these results indicate that the advertising-marginal are not substantially different from the average in terms of propensity to experience adverse effects, and as a result, advertising-marginal adverse effects will not close the gap between the costs and benefits of advertising-marginal prescriptions.

Table G.1: Adverse Effects - Various Measures and Proxies

	Churn	FLAE Any	FLAE Antidep	BLAE Any	BLAE Antidep
$Log(1+GRP)$	-0.00954	-0.0034	0.00013	-0.00004	0.0000005
	(0.01195)	(0.00629)	(0.00064)	(0.00012)	(0.00001)
$Log(1+GRP_{past})$	-0.00597	-0.00372	-0.00115	0.00014	-0.00004
	(0.02170)	(0.01008)	(0.00128)	(0.00023)	(0.00002)
Individual FEs	x	x	x	x	x
Border-Month FEs	x	x	x	x	x
Mean DV	0.66461	0.03626	0.00042	0.00417	0.00002
R-squared	0.5195	0.54831	0.54514	0.08998	0.0434
Observations	93,995	77,916	77,916	12,438,333	12,438,333

Columns (1)-(3) are only estimated on individuals prescribed in the current month to see if advertising predicts future behaviors. Columns (4)-(5) include all indviduals, to see if past advertising predicts current behaviors. 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 H - 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.