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Privacy and Marketing Externalities: Evidence from Do Not Call

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If not well targeted, advertising and direct marketing inflict nuisance and inconvenience on consumers. Theoretical analyses predict that consumer actions to avoid advertising impose externalities on other consumers. We investigate the extent of such externalities in the context of the U.S. Do Not Call (DNC) registry by exploiting the exogenous timing of the enforcement of the registry. Supported by multiple robustness tests, and validation and falsification exercises, we conclude that consumer DNC registrations imposed externalities on other consumers. An increase in the first wave of registrations by 1% was associated with a 3.1% increase in subsequent registrations. This effect was stronger in larger and more educationally or racially heterogeneous markets. The externality was possibly due to unregistered consumers being more receptive to telemarketing and telemarketers increasing calls to them. Our results suggest that managers should facilitate consumer opt-out, especially in larger and more educationally or racially heterogeneous markets.

Data, as supplemental material, are available at <http://dx.doi.org/10.1287/mnsc.2014.2051>.

Keywords: advertising; direct marketing; opt-out; externalities

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1. Introduction

Advertising and direct marketing communicate offers that may benefit consumers. Vendors strategically target different advertising messages to various consumer segments (Iyer et al. 2005). However, messages and solicitations, if not well targeted, impose annoyance and inconvenience. Vendors vie for consumers' limited attention and impose externalities on each other by displacing competing advertisements and solicitations (Van Zandt 2004, Anderson and de Palma 2009, Kuksov and Villas-Boas 2010, Bergemann and Bonatti 2011, Wilbur et al. 2013). Consumers avoid marketing in multiple ways: switching channels, using TiVo, concealing their addresses, using caller ID, and installing spam filters. The consumers' efforts in marketing avoidance may generate externalities on other consumers as vendors respond to such avoidance by adjusting their solicitations to the remaining consumers (Hann et al. 2008, Wilbur 2008, Anderson and Gans 2011, Johnson 2013).

The externalities between competing vendors and between consumers affect the effectiveness of advertising and direct marketing, and so affect vendor profits and consumer welfare. Yet despite their importance in

policy and management and the substantial theoretical analysis, there has been little empirical investigation into these externalities.¹ Policy makers and managers lack guidance as to the empirical significance of the externalities.

Here, we investigate the extent of externalities among consumers in the context of the U.S. Do Not Call (DNC) registry. We exploit a natural experiment arising from the exogenous timing of the government's implementation of the registry. On June 27, 2003, the DNC registry was opened for consumer registrations, with the first wave of registrations (up to August 31) being enforced on October 1 and later registrations enforced after a processing time of three months.

Our empirical strategy is to compare the pattern of DNC registrations after the start of enforcement on October 1 as a function of the first wave of registrations. After enforcement, telemarketers were not

¹ The notable exception is Wilbur's (2008) analysis of television advertising. Although he did not explicitly analyze externalities in marketing avoidance, he used the estimated parameters from a structural model to predict the effect of advertising avoidance technologies on the quantity of television advertising.

allowed to call the first wave of consumers, so, those consumers should have experienced a reduction in telemarketing. What about consumers who had *not* yet registered? Absent any externality, they should not have been affected in any way.

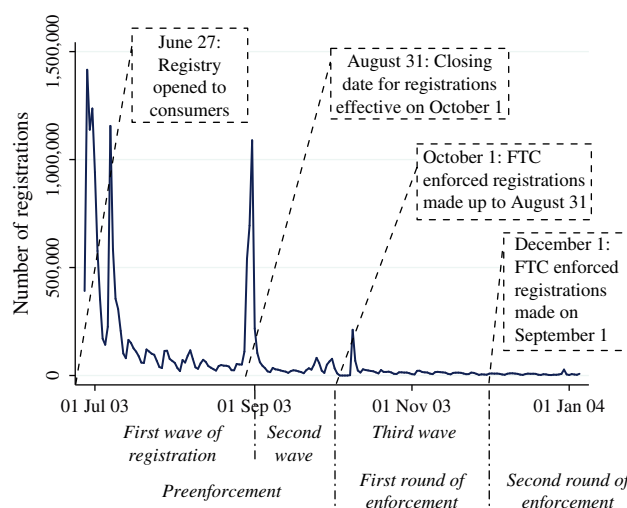
However, we find that, after enforcement, from October 1 onward, consumer DNC registrations *increased* with the magnitude of the first wave. Supported by validation and falsification exercises, we interpret this empirical relation as an externality from previously registered consumers to unregistered consumers, rather than the effects of individual preference or social influence. The estimated elasticity of postenforcement daily registrations with respect to the magnitude of the first wave was about 3.1. So, a 1% increase in the first wave (telephone lines registered up to August 31) was associated with a 3.1% increase in postenforcement DNC registrations (from October 1 onward). This finding was statistically and economically significant. Furthermore, the externality was stronger in markets that are larger or more educationally or racially heterogeneous.

We explore possible explanations of the externality and find that the empirical evidence points toward the mechanism as being consumer self-selection by their expected benefit from telemarketing offers.² The first wave of consumers who registered with the DNC were relatively less receptive to telemarketing offers, so the unregistered consumers were relatively more receptive to telemarketing. Hence, it was profitable for telemarketers to increase their calls to the unregistered consumers. However, the increase in calling prompted some of the previously unregistered consumers to join the DNC.

Our empirical findings provide insight and guidance to both managerial practice and public policy. Managers need to appreciate how the yield from marketing varies with consumer response to opt-out facilities and how to manage the responses. Policy makers need guidance on policies to address externalities among consumers and between vendors and consumers in the marketplace. We show that consumers' actions to avoid marketing do give rise to externalities. To the extent of consumer self-selection by expected benefit, managers should support government initiatives to provide opt-out facilities, and particularly in larger and more educationally or racially heterogeneous markets. For policy makers, our results suggest that opt-out facilities have some advantages over alternative policies such as Pigouvian taxes (Shiman 1996, Van Zandt 2004, Anderson

² People do buy from telemarketing offers. For example, before the advent of the U.S. DNC registry, time share operator, Fairfield Resorts, placed 16 million calls a year, of which 100,000 resulted in the consumer agreeing to take a tour of its resorts (*USA Today* 2003).

Figure 1 Timing of DNC Implementation



Notes. June 27–August 31: first wave of registration (enforced from October 1). September 1–30: second wave of registration (enforced within three months). October 1 onward: first round of enforcement; third wave of registration (enforced within three months). December 1 onward: second round of enforcement (registrations up to three months before).

and de Palma 2009) and attention fees (Ayres and Funk 2003, Loder et al. 2006).

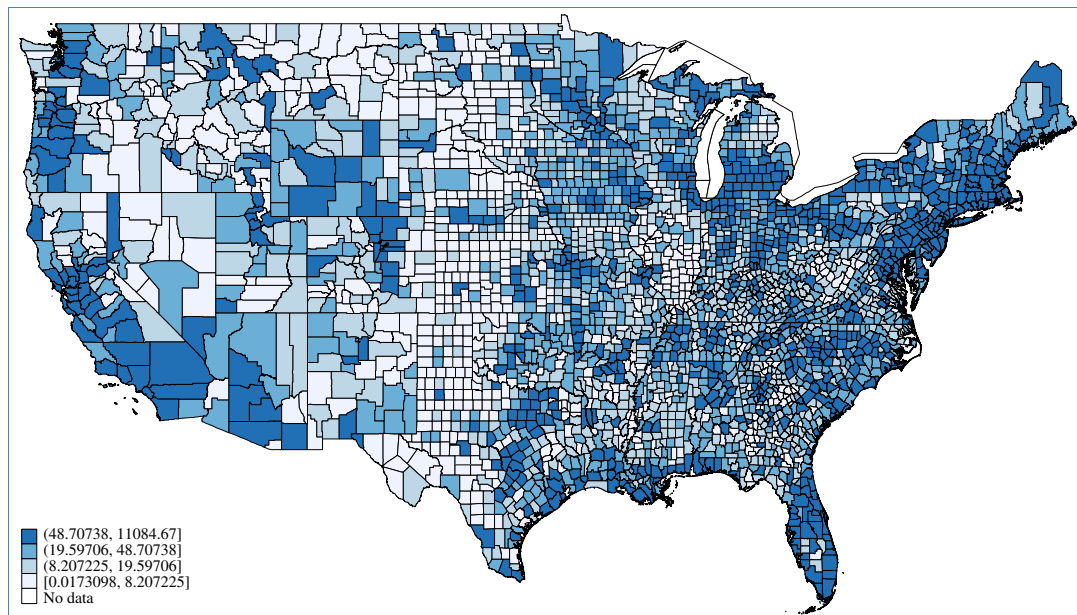
2. Context

The Federal Trade Commission (FTC) administers the U.S. DNC registry. With limited exceptions, federal law prohibits unsolicited telemarketing calls to telephone numbers on the DNC registry.³ The law prescribes a fine of up to \$11,000 per offense.

Figure 1 illustrates the timing of implementation. The FTC opened the DNC registry to consumers on June 27, 2003. From June 27 to August 31, the first wave of consumers registered, and their registrations were enforced from October 1. From September 1 to 30, the second wave of consumers registered, and their registrations were enforced after a three month processing time. So, for instance, registrations on September 1 were enforced from December 1, registrations on September 2 were enforced from December 2, and so on.⁴ Beginning on October 1, the FTC enforced the DNC registry and prohibited calls to numbers registered on or before August 31. From October 1

³ The federal DNC registry applies to interstate and intrastate telemarketing calls and accepts registrations from fixed-line and mobile but not business telephone numbers. The DNC registry does not apply to inward telemarketing (calls from consumers to vendors). Also, the DNC registry exempts calls for political campaigning and survey research, by nonprofit and charitable organizations, and by businesses with a recent commercial relationship with the consumer.

⁴ Subsequently, the government reduced the processing time (during which telemarketers must update their calling lists) to 30 days.

Figure 2 Telemarketer Downloads: Geographical Variation

Notes. Map depicts the number of telemarketer downloads by county, weighted by number of households, of the DNC registry for area codes within a county between September 2 and October 31, 2003. Color represents the quartile (very dark blue = top quartile; dark blue = second quartile; light blue = third quartile; very light blue = fourth quartile).

onward, the third wave of consumers registered, and their registrations were enforced after three months.

From September 2, the FTC allowed telemarketing vendors to download telephone numbers in the DNC registry. From October 1, telemarketers were prohibited from calling the first wave of consumers (who registered their numbers by August 31). Thereafter, telemarketers were required to update their calling lists every three months.

By September 30, a total of 13,000 telemarketers had downloaded the DNC registry (FTC 2003). Figure 2 depicts the telemarketer downloads of the DNC registry by county, weighted by the number of households. The geographical variation in the intensity of telemarketing is consistent with telemarketers defining markets by county or even lower level. This inference is supported by Figures 3(a) and 3(b), which show that the overwhelming majority of telemarketers downloaded five or fewer area codes, whereas the modal number of downloads was either one or five area codes.⁵

Telemarketers could have refined their calling lists from September 2. However, many took the opposite tack: “As Oct. 1 approaches, many telemarketers are ratcheting up their calls while they can” (*USA Today* 2003, p. B01). Indeed, the FTC received multiple consumer complaints of increased telemarketing.

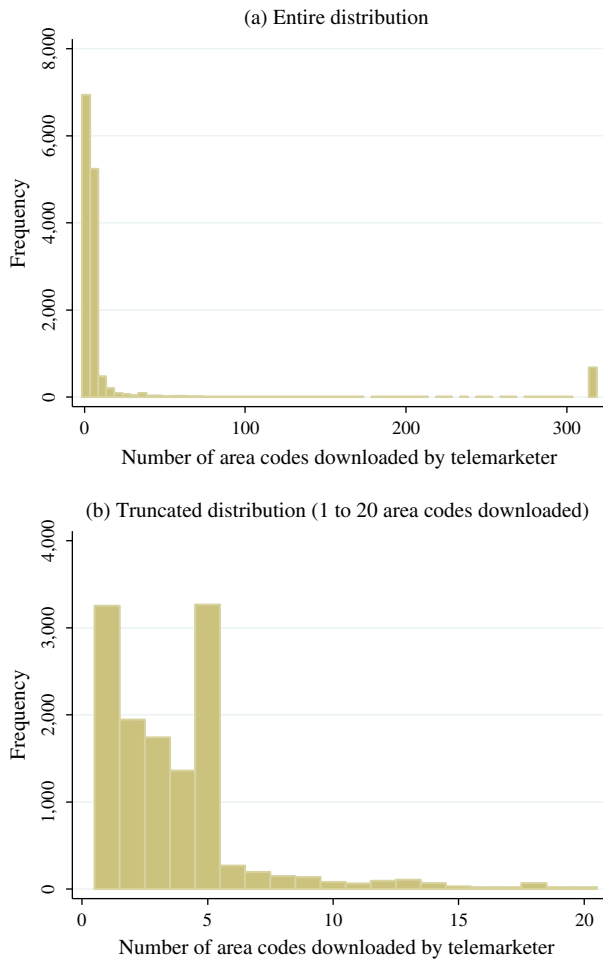
FTC spokeswoman, Cathy MacFarlane, explained, “It could be budget dumping. Telemarketers have a certain amount of money to spend for the year, and instead of spending it evenly for the last five months, they are concentrating their calls in July, August and September” (*Plain Dealer (Cleveland)* 2003, p. C1).

Prior to the federal DNC registry, 27 states had already established a state-level DNC registry (Varian et al. 2004). Of these, 16 states eventually merged their lists with the federal registry. To the extent of state enforcement of a state list, the consumer’s benefit from the federal DNC registry would be lower. Figure 4 depicts the states that established a state-level DNC registry and subsequently did or did not merge the state registry with the federal registry (dark and light blue, respectively) and that did not establish a state-level registry as of August 31, 2003.

Figure 5 depicts the first wave of federal DNC registration. For more intuitive comparison between states, the figure represents the first wave by the cumulative DNC registrations per household (as of August 31, 2003). Evidently, in states with an unmerged state-level registry (light blue in Figure 4), the first wave of federal DNC registrations was smaller. The mean cumulative registrations per household by August 31 in states with a state-level registry merged with the federal registry, state-level registry not merged with the federal registry, and without a state registry were 0.386, 0.185, and 0.328 per county, respectively. We

⁵ The FTC allowed downloads of up to five area codes without charge, while charging a fee for more area codes.

Figure 3 (Color online) Telemarketer Downloads: Size



Note. Each bar represents the number of telemarketers that had downloaded the specific number of telephone area codes on the horizontal axis (between September 2 and October 31, 2003).

exploit these differences in two validation exercises below.

3. Empirical Strategy

In general, several factors possibly influence a consumer's decision to register with the DNC. One is individual preference—her inherent like or dislike of telemarketing offers. The other three influences are due to earlier registrations by others. Suppose that some consumers have already registered with DNC so opted out of telemarketing. They may affect other consumers who have not yet registered with DNC in several ways.

First, they might influence the unregistered consumers through learning, peer pressure, and herding, or what we collectively call “social influence” (Manski 1993, Zhang 2010, Tucker and Zhang 2011). Social influence is a direct externality among consumers.

Second, as analyzed by Hann et al. (2008) and Johnson (2013), the registrations may affect vendors' expected profit, so the vendors' solicitations to the unregistered consumers whom vendors can solicit. The change in solicitations would then affect those consumers' DNC registrations. We call this “solicitation pressure”—it arises from consumers who already registered with DNC and affects those who have not yet opted out. Solicitation pressure is an indirect externality (through vendors) among consumers.

The third possible effect is exit of telemarketers, which would reduce solicitations. Prior to the DNC registry, vendors targeted the more profitable consumers. Suppose that the more profitable consumers are first to register with the DNC, and that telemarketing involves some fixed cost. Then, when the government enforces the DNC, some vendors cannot cover their fixed costs and drop out, which reduces solicitations to the unregistered consumers.

Both social influence and solicitation pressure give rise to positive externalities from the first wave of registrations on unregistered consumers. By contrast, telemarketer exit implies a negative externality from the first wave of registrations on unregistered consumers. Empirically, we find a positive relation between the first wave of registrations and subsequent post-October 1 registrations. So, in the following discussion, we focus on individual preference and the externalities due to social influence and solicitation pressure.

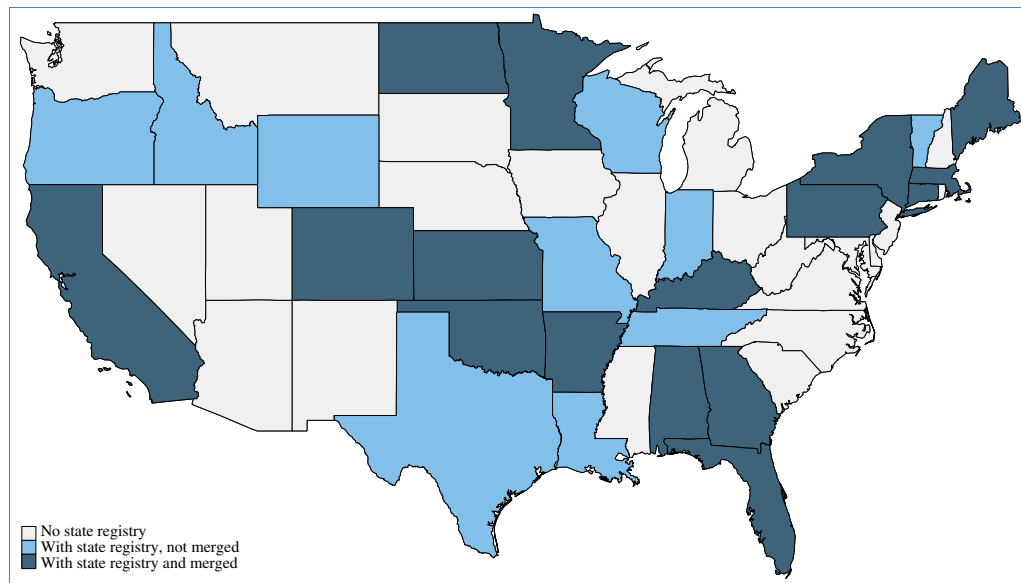
Lacking detailed information on telemarketing solicitations, we cannot generally distinguish between social influence and solicitation pressure, which are both positive externalities. Fortunately, for administrative reasons the FTC enforced the DNC registrations only after a processing time, which differed by the date of registration. Referring to Figure 1, individual preference and social influence affected all three waves of consumer DNC registration. However, solicitation pressure could only have affected the third wave.

Hence, as Figure 6 illustrates, our empirical strategy analyzes the observed third wave of DNC registrations as the sum of (i) solicitation pressure and (ii) individual preference and social influence. We identify solicitation pressure by the enforcement date (October 1 and after) and the size of the first wave of registrations (up to August 31) in the county. We use the second wave of registrations, in part, to model the effect of individual preference and social influence.

Accordingly, our empirical model of daily DNC registrations is

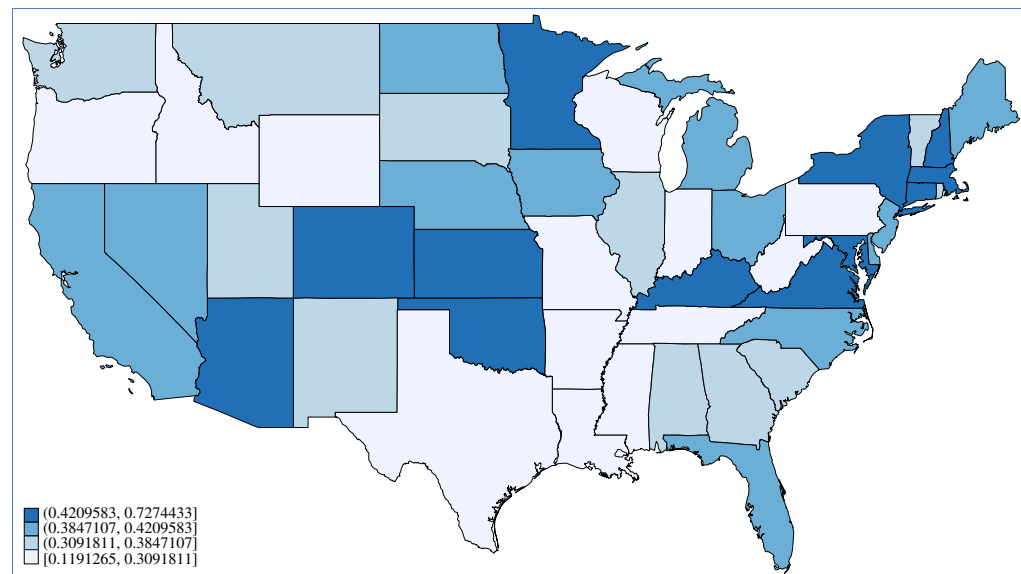
$$\begin{aligned} & \ln(1 + r_{kt}) \\ &= \beta_1 \ln R_{k,t-1} + \beta_2 ENF_t \times \ln R_{k,t-1} + \beta_3 ENF_t \\ & \times \ln R_{k, \text{Aug31}} + \beta_4 \ln(1 + N_{kt}) + \beta_5 \psi_k + \beta_6 \tau_t + e_{kt}, \quad (1) \end{aligned}$$

Figure 4 State-Level DNC Registries



Notes. Status as of August 31, 2003. White = no state DNC registry; light blue = state DNC registry not merged with the federal registry; dark blue = state DNC registry merged with federal registry.

Figure 5 First Wave of DNC Registration



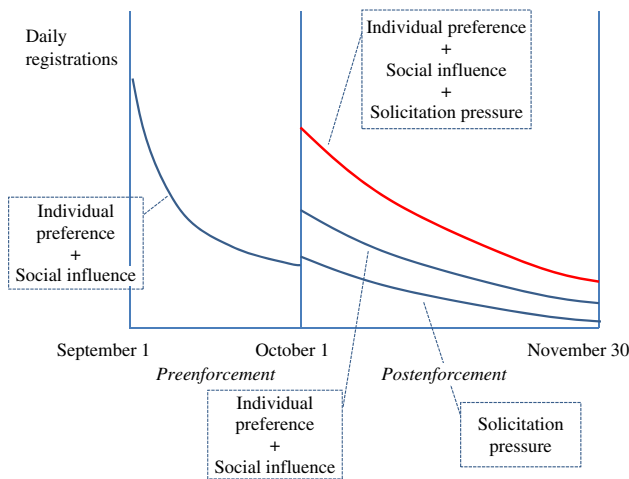
Notes. Map depicts the first wave of registrations by state (for more intuitive comparison between states, first wave is represented as cumulative DNC registrations per household as of August 31, 2003). Color represents the quartile (very dark blue = top quartile; dark blue = second quartile; light blue = third quartile; very light blue = fourth quartile).

where r_{kt} is the DNC registrations in county k on day t ,⁶ the indicator of DNC enforcement; $ENF_t = 1$ for any day on or after October 1, and $ENF_t = 0$ otherwise; $R_{k,t-1}$ is the cumulative DNC registrations in the

⁶ We add one before taking the logarithms of DNC registrations and news reports to ensure that the variable is well defined in the case of zero registration or zero news report.

county from June 27 up to the previous day; $R_{k, \text{Aug31}}$ is the first wave of registrations (cumulative DNC registrations from June 27 to August 31) in the county; N_{kt} is the daily number of news reports about the DNC registry in the county, weighted by circulation, which has been shown to influence DNC registration (Goh et al. 2011); ψ_k are county-level fixed effects;

Figure 6 (Color online) Identification



τ_t are day fixed effects; and e_{kt} is an idiosyncratic error.⁷

Equation (1) includes the cumulative registrations up to the previous day, $\ln R_{k,t-1}$, for two purposes. First, the cumulative registrations account for the diffusion of DNC registrations over time (Mahajan et al. 1990, Van den Bulte and Stremersch 2004). As more consumers join the DNC registry, the number available to register would fall, which tends to attenuate the social influence. Second, the cumulative registrations capture the effect of social influence.⁸ Equation (1) includes the interaction of enforcement with cumulative registrations, $ENF_t \times \ln R_{k,t-1}$, because the effect of social influence might differ between the pre- and postenforcement periods. With enforcement, the remaining addressable consumers (who have not yet opted out) might experience changes in vendor solicitations so might respond differently to the registrations of others.

Given that the registry was enforced only from October 1, we can unambiguously identify the solicitation pressure through the interaction of enforcement and the magnitude of the first wave of registrations, $ENF_t \times \ln R_{k, \text{Aug 31}}$. Referring to (1), a finding that $\beta_3 > 0$ suggests that consumers in counties with a larger first wave were themselves more likely to register when the DNC was enforced. From the relevant literature (FTC 2003; Johnson 2003; Rotfeld 2004; Varian

et al. 2004, 2005), enforcement of the DNC registry was the only relevant event around October 1. Note that our identification of the externality relies on the exogenous timing of the shock on telemarketers (represented by ENF_t), not the magnitude of the first wave of registrations, $R_{k, \text{Aug 31}}$, as such. The direct effect of the first wave would be absorbed by the county fixed effects so cannot be separately identified.

As Figure 1 shows, DNC registrations peaked several times, notably in the first few days, on the 11th day and a few days after, and in the days leading up to and on August 31. It is difficult to capture such multimodal patterns with a simple model (Bonfrer and Dreze 2009). Accordingly, to identify the effect of enforcement from October 1 onward, in estimating (1) we limit the sample to a three-month panel between September 1 and November 30.⁹

Consumers in states with a preexisting state DNC registry might have already experienced the effects of DNC enforcement so would be less sensitive to the effects of the federal registry. Accordingly, in most estimates, we limit the sample to counties in states without state-level DNC registries to focus on consumers who had not experienced any DNC enforcement.

Besides state-level DNC registries, the various U.S. states might have established other policies that possibly affected consumer sign up with the federal DNC registry. The counties varied by income, employment, ethnicity, and other demographics. We include county fixed effects, ψ_k , to represent these unobserved heterogeneities that did not vary with time. We also include a full set of day fixed effects, τ_t , to represent factors that affected all counties, such as the suspension of the DNC registry between October 4 and 7 by order of the Federal District Court in Colorado due to legal challenges by the telemarketing industry (FCC 2004).

Finally, in estimating standard errors, we cluster by state (Bertrand et al. 2004). In addition, we specify all continuous variables in logarithms. Daily DNC registrations (mean, 20.89, s.d. 131.65) and other continuous variables are over-dispersed, some extremely. Also, the double-log specification allows us to directly interpret the coefficients as elasticities. For brevity, we omit mention of the logarithm in discussing the results.

4. Data

The FTC provided us with redacted telephone numbers on the DNC registry for each area code and exchange, for example, (617) 363-xxxx, by date of registration. Some exchanges spanned county borders, so we allocated the DNC registrations by the number of

⁷ We conducted the analysis at the county level for several reasons. Telemarketing is governed by state-level laws and regulations, so it is important that the unit of analysis lies within state boundaries. Although counties fit within state boundaries, telephone area codes may not. Furthermore, the county provides an intuitive geographical definition of a market. We could then match the geographical unit to demographic information at the county level from the U.S. Census. The effect of ENF_t itself is captured by the day fixed effects, and so cannot be separately identified.

⁸ In the diffusion literature, social influence is called “internal” or “imitation” influence (Bass 1969, Mahajan et al. 1990).

⁹ In an alternative test, focusing on enforcement of the second wave of DNC registrations, we extended the sample to December 31.

households in the counties using the North American Local Exchange NPA-NXX Database and the 2000 U.S. Census.¹⁰ The FTC also provided us with telemarketer downloads of the DNC registry by date. We used the downloads to strengthen the identification of the externality in DNC registrations (see §6.1 for details).

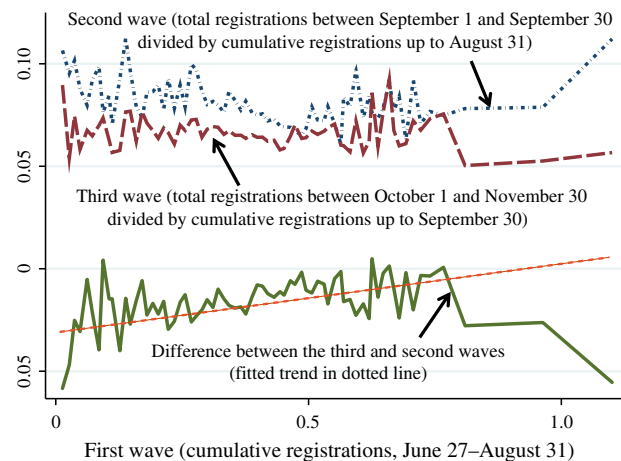
Using the proprietary news database Factiva, we searched for reports in U.S. newspapers including the words “do not call.” For each report, we recorded the title of the newspaper, the date, and whether the report mentioned number of people registering, e.g., “The FTC estimates some 60 million phone numbers will be registered out of 166 million residential phone numbers in America” (*New York Post* 2003, p. 29). We weighted each report by the circulation of the newspaper in the county as published by the Audit Bureau of Circulation (Goh et al. 2011).

We limit most of the analysis to counties in the states without a state-level DNC registry and the period from September 1 to November 30. Tables 1 and 2 present summary statistics of registrations and the covariates and their correlations.¹¹

For a first look at the data, Figure 7 compares the registrations in the second wave of DNC registrations (from September 1 to 30) with the third wave (from October 1 to November 30). To provide a basis of comparison, we normalize the registrations by the cumulative registrations up to the day before (so, divide the second wave by the cumulative registrations up to August 31, and divide the third wave by the cumulative registrations up to September 30).

Referring to Figure 7, the solid line represents the difference between the third and second waves. As shown by the fitted trend (the dotted line), this difference increased in the magnitude of the first wave (cumulative registrations per household up to August 31). So in counties where the first wave was larger, the third wave (postenforcement) was relatively larger than the second wave (preenforcement). This observation from the graphs is consistent with the first wave of consumer registrations imposing

Figure 7 (Color online) DNC Registrations: Second Vis-à-Vis Third Waves



Note. The graphs depict the median-band plots of the second and third waves of DNC registrations as functions of the first wave, which is represented on the horizontal axis in 100 bands of equal width.

externalities on unregistered consumers in the post-enforcement period.

5. Results

Although intuitive, the graphs in Figure 7 do not account for confounds that might possibly affect daily DNC registrations such as changes in social influence and news coverage. Accordingly, we turn to multiple regression analysis of daily DNC registrations, as reported in Table 3. To provide a baseline, column (1) reports an estimate including only the control variables. As intuitively expected, the coefficient of circulation-weighted number of daily newspaper reports of DNC in the county for the same day is positive and significant (see also Goh et al. 2011).

The coefficient of cumulative DNC registrations in the county up to the previous day is negative but not significant. The cumulative registrations account for the diffusion of DNC registrations due to individual preference and social influence across counties and time. The coefficient of the interaction of cumulative DNC registrations with enforcement (October 1 and after) is negative and significant. As more and more consumers registered, the pool available for registration shrank, so it seems reasonable that the diffusion would attenuate.

Table 3, column (2) reports our estimate, including the identification of the externalities among consumers. The coefficient of the first wave (cumulative registrations up to August 31) interacted with enforcement, 2.932 (s.e. 0.738), is positive and precisely estimated. This estimate suggests that for a county in which the first wave was 1% (124 telephone lines on

¹⁰ On reviewing the data, the DNC registrations in Kenedy, TX (FIPS 48261) and Loving, TX (FIPS 48301) were less than 1, and 4.73 per household in Williamsburg, VA (FIPS 51830). We excluded these three outlier counties from the analyses.

¹¹ Cumulative DNC registrations could possibly exceed the number of households because some households owned multiple telephone lines. We would have liked to account for the number of telephone lines in the county but could not find the relevant data. Note that the estimation model, (1), was specified as double-log. The logarithm of DNC registrations per household, i.e., the logarithm of DNC registrations divided by the number of households, would resolve to the logarithm of DNC registrations minus the logarithm of the number of households and similarly for the logarithm of number of news reports per household. The number of households and population of the county were invariant over time and would have been absorbed by the county fixed effects.

Table 1 Summary Statistics

Variable	Obs.	Mean	Std. dev.	Min	Max
DNC registrations	109,200	20.89	131.65	0	18,778.31
Cumulative registrations	109,200	13,643.14	44,994.33	11.47	856,875.80
Cumulative registrations up to Aug 31	109,200	12,384.81	40,762.96	11.40	740,905.30
News reports	109,200	1.56	7.92	0	264.80
Cumulative news reports up to Aug 31	109,200	48.65	96.03	0	1,163.60
Social interaction	48,412	0.02	0.81	−0.89	4.98
News reports of others' registration	109,200	1.32	7.22	0	264.80
Cumulative telemarketer downloads up to Oct 31	109,200	78.99	344.99	0.46	8,624.77
Cumulative telemarketer downloads	108,000	59.22	284.56	0.03	10,115.73
Total registrations in Jul–Aug	109,200	8,902.74	28,123.96	8.37	541,879.40
Total households	109,200	31,328.70	89,669.18	185	1,974,181
Inequality of income	109,200	0.43	0.04	0.34	0.57
Education heterogeneity	109,200	0.82	0.03	0.70	0.90
Racial heterogeneity	109,200	0.24	0.19	0.00	0.72

Note. Sample comprises counties in states without state-level DNC registry, From September 1 to November 30, 2003.

Table 2 Correlations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
(1) DNC registrations	1													
(2) Cumulative registrations	0.58	1												
(3) Cumulative registrations up to Aug 31	0.59	1.00	1											
(4) News reports	0.15	0.10	0.10	1										
(5) Cumulative news reports up to Aug 31	0.16	0.25	0.25	0.36	1									
(6) Social interaction	0.02	0.03	0.03	0.01	0.00	1								
(7) News reports of others' registration	0.15	0.09	0.09	0.95	0.34	0.01	1							
(8) Cumulative telemarketer downloads up to Oct 31	0.55	0.93	0.93	0.07	0.18	0.02	0.06	1						
(9) Cumulative telemarketer downloads	0.38	0.86	0.84	0.05	0.16	0.01	0.05	0.90	1					
(10) Total registrations in Jul–Aug	0.58	0.99	1.00	0.10	0.26	0.03	0.09	0.94	0.85	1				
(11) Total households	0.57	0.97	0.97	0.09	0.24	0.02	0.09	0.96	0.87	0.97	1			
(12) Inequality of income	0.04	0.06	0.06	−0.05	−0.02	−0.08	−0.04	0.09	0.08	0.07	0.09	1		
(13) Education heterogeneity	0.14	0.23	0.23	0.08	0.16	−0.02	0.07	0.18	0.16	0.23	0.25	0.42	1	
(14) Racial heterogeneity	0.19	0.30	0.31	0.10	0.20	0.07	0.10	0.26	0.23	0.31	0.33	0.36	0.66	1

average) larger, the postenforcement daily registration was about 3.1% (0.65 telephone lines) higher.¹² This effect is both statistically and economically significant. The estimate provides strong evidence that is consistent with DNC externalities among consumers. We prefer this estimate for its simplicity as compared with other estimates, reported below.

Next, we report a very flexible representation of the externality. In Equation (1), the externality is represented by the interaction of enforcement with the first wave of registrations, i.e., the variable, $ENF_t \times \ln R_{k, \text{Aug 31}}$. Instead of this simple representation, we use a full set of day interactions, namely,

¹² The dependent variable in the estimate is $\ln(1 + r_{kt})$, so the elasticity of $1 + r_{kt}$ with respect to the first wave is roughly $100 \times (e^{2.932 \times \ln(1.01)} - 1) = 2.96$. Hence, an increase in the first wave by 1% would be associated with a 2.96% increase in $1 + r_{kt}$, or $\Delta(1 + r_{kt}) / (1 + \bar{r}) = \Delta r_{kt} / (1 + \bar{r}) = 2.96$, since $\Delta(1 + r_{kt}) = \Delta r_{kt}$. The above simplifies to $\Delta r_{kt} / \bar{r} = 2.96 / \bar{r} + 2.96$. By Table 1, the average daily registrations, $\bar{r} = 20.89$, and hence, the proportionate increase, i.e., the elasticity, $\Delta r_{kt} / \bar{r} = 2.96 / 20.89 + 2.96 = 3.10$.

$Sep\ 1 \times \ln R_{k, \text{Aug 31}}, Sep\ 2 \times \ln R_{k, \text{Aug 31}}, \dots, Nov\ 30 \times \ln R_{k, \text{Aug 31}}$. This specification allows the effect of the first wave to vary with each day. Table 3, column (3) reports part of the estimate, whereas Figure 8(a) presents the estimated coefficients of the day interactions. Evidently, the effect of the first wave on DNC registrations jumped on October 1 and remained elevated thereafter.

Referring to Figure 6, our empirical strategy depends on correctly modelling the diffusion of registrations due to individual preference and social influence in the postenforcement period. Generally, we model the diffusion by cumulative registrations and their interaction with enforcement, so allowing the effect of cumulative registrations to differ between pre- and postenforcement periods. To check the robustness of this model, we consider alternative models of the diffusion process.

A flexible specification would allow the effect of cumulative registrations to vary by day. Table 3, column (4) reports an estimate in which the effects

Table 3 Externality in DNC Registration

Variables	(1) Baseline	(2) DNC enforcement	(3) Day-specific first wave effects	(4) Day-specific first wave and diffusion	(5) With day trend and square of day trend	(6) County-specific day trend and square of day trend	(7) Nov–Dec: Second enforcement round	(8) Sep–Dec: Both enforcement rounds
<i>News reports</i> + 1 (ln)	0.027*** (0.008)	0.026*** (0.008)	0.008 (0.005)	0.009* (0.005)	0.156*** (0.024)	0.155*** (0.026)	0.032*** (0.010)	0.028*** (0.007)
<i>Cumulative registrations</i> [<i>t</i> − 1] (ln)	−1.688 (1.060)	1.562 (1.337)	1.062 (1.456)	6.160 (3.628)	8.407*** (2.570)	7.995* (4.219)	−1.321 (1.852)	2.534* (1.257)
<i>Enforcement</i> × <i>Cumulative</i> <i>registrations</i> [<i>t</i> − 1] (ln)	−0.150*** (0.010)	−3.092*** (0.735)	−2.859*** (0.772)		−8.327*** (1.813)	−10.322*** (2.314)		−3.355*** (0.848)
<i>Enforcement</i> × <i>First wave</i> (ln)		2.932*** (0.738)			8.149*** (1.817)	10.064*** (2.309)		3.195*** (0.851)
<i>Enforcement</i>					1.663*** (0.116)	2.467*** (0.234)		
<i>Day trend</i> (ln)					−1.058*** (0.070)			
<i>Day trend squared</i> (ln)					0.114*** (0.013)			
<i>Sep 1, ..., Nov 30</i> × <i>First wave</i> (ln)			(Figure 8(a))	(Figure 8(b))				
<i>Sep 1, ..., Nov 30</i> × <i>Cumulative</i> <i>registrations</i> [<i>t</i> − 1] (ln)				Omitted for brevity				
<i>County fixed effects</i> × <i>Day trend</i> (ln)						Omitted for brevity		
<i>County fixed effects</i> × <i>Day trend squared</i> (ln)						Omitted for brevity		
<i>Post-Dec 1</i> × <i>Cumulative</i> <i>registrations</i> [<i>t</i> − 1] (ln)							−1.440*** (0.361)	−0.772 (0.592)
<i>Post-Dec 1</i> × <i>Cumulative</i> <i>registrations</i> [<i>t</i> − 91] (ln)							1.344*** (0.359)	0.680 (0.590)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes	No	No	Yes	Yes
Observations	109,200	109,200	109,200	109,200	109,200	109,200	72,000	145,200
<i>R</i> -squared	0.632	0.632	0.725	0.727	0.246	0.266	0.320	0.630
Counties	1,200	1,200	1,200	1,200	1,200	1,200	1,200	1,200

Notes. Estimated by ordinary least squares. Dependent variable is logarithm of $(1 + \text{registration})$. The key coefficients are displayed in bold for easy reference. Robust standard errors clustered by state in parentheses. Column (1): Baseline estimate including all control variables. Column (2): Including representation of externality—*enforcement* × *first wave* (cumulative registrations up to August 31). Column (3): Effect of first wave varying by day—depicted in Figure 8(a). Column (4): Effects of first wave and diffusion varying by day—day-specific first wave effects depicted in Figure 8(b). Column (5): Day trend and the square of the day trend in place of day fixed effects with a separate DNC enforcement effect. Column (6): Day trend and the square of the day trend varying by county with a separate DNC enforcement effect. Column (7): Effect of second round of enforcement on registrations between November 1 and December 31. Column (8): Separate effects of the first and second rounds of enforcement on registrations between September 1 and December 31.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

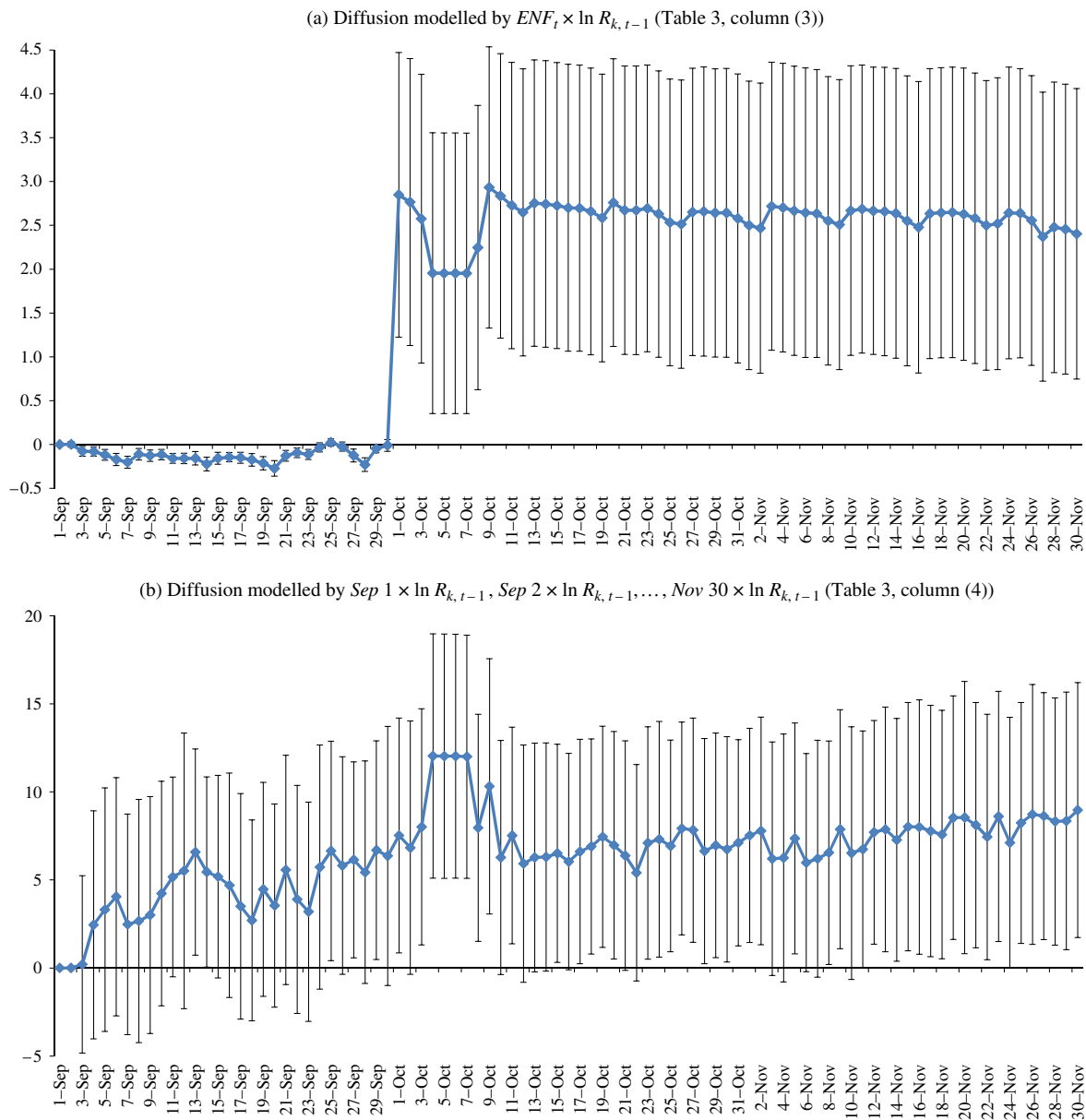
of both the first wave and diffusion vary by day, whereas Figure 8(b) plots the estimated coefficients of the interactions between the first wave and day. The effects of the first wave are mostly positive but not significant before October 1, and larger and significant on October 1 and after. The sum of the first wave effects before October 1 is not significantly different from 0 ($p = 0.13$). By contrast, the sum of the first wave effects on October 1 and after is significantly larger than 0 ($p < 0.05$) and significantly larger than the sum of the first wave effects before October 1 ($p < 0.01$).

The specifications in Table 3, columns (3) and (4) provide very flexible parameterizations of the

diffusion of registrations and externalities from the first wave on subsequent registrations. However, in view of the large number of estimated parameters relative to the incremental change in model fit, we prefer the more parsimonious specification in Table 3, column (2).

Table 3, column (5) reports an estimate including a day trend and the square of the day trend in place of day fixed effects. Table 3, column (6) further allows the day trend and the square of the day trend to vary by county. These specifications allow us to separately estimate the effect of the enforcement on October 1, which, consistent with the presence of solicitation pressure, is positive and statistically significant. More

Figure 8 (Color online) Day-Varying Effects of First Wave on Daily Registration



Note. Figures plot the coefficients of day \times logarithm of first wave (cumulative registrations up to August 31) in the respective models of log daily registration (Table 3, columns (3) and (4)).

important, consistent with the preferred estimate in Table 3, column (2), the first wave has a positive effect on postenforcement registrations, i.e., the coefficient of $ENF_t \times \ln R_{k,Aug31}$ is always positive and significant.

Furthermore, the online appendix (available as supplemental material at <http://dx.doi.org/10.1287/mnsc.2014.2051>) reports three other estimates, the first one applying a quadratic model of diffusion, the second one allowing the cumulative registrations up to the previous day to vary by county, and the third one allowing both the cumulative registrations up to the previous day and their interaction with enforcement to vary by county. Consistent with our preferred

estimate in Table 3, column (2), the first wave has a significant and positive effect on postenforcement registrations in the first two specifications.

However, in the third specification, its effect is negative and significant. This inconsistent finding could be due to collinearity—the cumulative registrations up to the previous day after October 1 include all the first wave registrations, and as shown in Figure 1, the number of registrations after September 1 was small. So allowing the cumulative registrations up to the previous day to vary by county and before and after October 1 would replicate most of the variation in the interaction between the first wave and enforcement.

Indeed, as shown in the online appendix, if we modify the specification slightly and allow the cumulative registrations up to the previous day and their interaction with enforcement to vary by state (rather than by county), then the first wave again has a significant and positive effect on postenforcement registrations. Hence, our finding is robust to allowing for cross-sectional variation in diffusion after the enforcement.

Finally, we identify the externality in another way. This focuses on the second wave of registrations (from September 1 onward) which the FTC enforced with a three-month lag. Consider the registrations between November 1 and December 31. The first wave of registrations would have been enforced throughout this period. However, the second wave was enforced only from December 1. So, did the second wave induce an externality on postenforcement (December 1 and after) registrations?

By contrast with the first wave of registrations, which had a sharp effect beginning on October 1, we expect the externality from the second wave to be more diffuse. By law, telemarketers had three months to scrub their calling lists. Given fixed costs of updating, smaller telemarketers might check the DNC registry and scrub their lists periodically, perhaps weekly or monthly. Hence, for the second wave of consumers, the reduction in telemarketing would be spread out, so the externality on unregistered consumers would be spread out. For instance, consumers who registered on September 1 might experience less telemarketing and the unregistered consumers might get more telemarketing calls *before* December 1. They might not experience a big bang, unlike consumers who registered before September 1.

As reported in Table 3, column (7), the sample comprises registrations between November 1 and December 31. The coefficient of post-December 1 interacted with cumulative registrations up to three months before, 1.344 (s.e. 0.359), is positive and significant. The corresponding elasticity is 1.41. As expected, it is smaller than (less than half of) the externality due to the first wave on October 1 and after registrations. The result from the second wave provides further evidence of the externality.

Another, more general specification is to estimate the separate effects of the first-round enforcement (first wave registrations interacted with October 1 and after) and second-round enforcement (registrations lagged by three months interacted with December 1 and after). Table 3, column (8) reports an estimate similar to our preferred estimate in Table 3, column (2), but which expands the sample to December 31 and includes the interactions of post-December 1 with registrations lagged by three months and registrations up to the previous day as additional

covariates. The coefficient of the first wave of enforcement, 3.195 (s.e. 0.851), is positive and significant. The coefficient of the second wave of enforcement, 0.680 (s.e. 0.592), is also positive but not precisely estimated ($p = 0.26$). This is understandable since, as discussed above, the effect of the second wave of enforcement likely diffused, so the second wave of enforcement is identified rather weakly. Moreover, as Figure 1 shows, relatively few households registered on September 1 and after, and hence they might not provide sufficient variation to precisely identify the effect of the second wave of enforcement.

5.1. Validation

We have interpreted the correlation between the first wave (cumulative registrations up to August 31) and postenforcement DNC registrations (October 1 onward) as an externality between consumers. One way to check this interpretation is validation exercises in which, by a priori reasoning, the strength of the externality and likewise, the correlation should differ in a particular direction. Table 4 presents results of validation and falsification exercises, with, for convenience, column (1) reproducing the preferred estimate from Table 3, column (2).

Twenty-seven states established state-level DNC registries before the federal registry (Varian et al. 2004, Appendix A). Among them, we identify the states that *did enforce* their state registries as those that *did not merge* the state-level registry with the federal registry. In such states, the effect of federal DNC enforcement should be weaker—to the extent that state enforcement had already generated some externality on unregistered consumers. Table 4, column (2) reports the estimate for counties in these states. Indeed, the coefficient of enforcement interacted with the first wave, 1.219 (s.e. 0.441), is less than half of the preferred estimate but still statistically significant ($p < 0.05$). This result is consistent with the federal DNC registry giving rise to an externality, albeit smaller than in states without a state-level registry.¹³

California, and possibly other states, set up a state-level DNC registry solely as a way for residents to “preregister” for the federal registry without intending to enforce the state registry (Varian et al. 2004, Appendix A). We identify these as states that established state registries in 2003 and merged the state-level DNC registry with the federal registry. Table 4,

¹³ The estimate in Table 4, column (2) excludes Georgia and Pennsylvania, which merged their state registries with the federal registry in October 2003 and May 2004, respectively. Our results are robust to the inclusion of Georgia and Pennsylvania—the coefficient of enforcement interacted with the first wave, 0.728 (s.e. 0.265), is smaller but still statistically significant, possibly because Georgia and Pennsylvania had enforced their state registries before the federal registry was opened.

Table 4 Validation and Falsification

Variables	(1) Preferred	(2) Enforced state lists (unmerged)	(3) Unenforced state lists (merged)	(4) Placebo: News reports	(5) Placebo: Enforce Nov 1	(6) Placebo: Enforce Sep 30	(7) Social interaction	(8) News reports of others	(9) Tele- marketer dumping
<i>News reports + 1</i> (ln)	0.026*** (0.008)	0.052*** (0.007)	0.026* (0.012)	0.027*** (0.008)	0.020*** (0.007)	0.021*** (0.007)	0.019* (0.009)	−0.067*** (0.017)	0.026*** (0.008)
<i>Cumulative registrations [t − 1]</i> (ln)	1.562 (1.337)	−0.340 (0.500)	2.679* (1.437)	−1.727 (1.086)	−0.057 (1.099)	−0.808 (1.121)	1.987 (2.272)	1.519 (1.331)	1.620 (1.411)
<i>Enforcement × Cumulative registrations [t − 1]</i> (ln)	−3.092*** (0.735)	−1.419*** (0.447)	−4.229** (1.450)	−0.153*** (0.013)			−3.454** (1.612)	−3.061*** (0.734)	−2.983*** (0.616)
<i>Enforcement × First wave</i> (ln)	2.932*** (0.738)	1.219** (0.441)	4.060** (1.459)				3.315* (1.619)	2.900*** (0.738)	
<i>Enforcement × Cumulative news Aug 31 + 1</i> (ln)				0.007 (0.040)					
<i>Cumulative news [n − 1] + 1</i> (ln)				0.013 (0.031)					
<i>Enforcement × Cumulative news [t − 1] + 1</i> (ln)				−0.003 (0.030)					
<i>Post-Nov 1 × Cumulative registrations [t − 1]</i> (ln)					−1.328** (0.584)				
<i>Post-Nov 1 × First wave</i> (ln)					1.252** (0.589)				−0.233 (0.595)
<i>Post-Sep 30 × Cumulative registrations [t − 1]</i> (ln)						0.785 (1.153)			
<i>Post-Sep 30 × First wave</i> (ln)						−0.576 (1.154)			
<i>Enforcement × Social interaction</i>							−0.043 (0.079)		
<i>Enforcement × First wave</i> (ln) × <i>Social interaction</i>							0.006 (0.009)		
<i>News reports of others + 1</i> (ln)								0.097*** (0.020)	
<i>Enforcement × News reports of others + 1</i> (ln)								0.023 (0.016)	
<i>October × First wave</i> (ln)									2.821*** (0.615)
<i>November × First wave</i> (ln)									3.058*** (0.962)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	109,200	74,880	98,370	109,200	109,200	109,200	48,412	109,200	109,200
R-squared	0.632	0.575	0.498	0.632	0.620	0.617	0.735	0.633	0.632
Counties	1,200	832	1,093	1,200	1,200	1,200	532	1,200	1,200

Notes. Estimated by ordinary least squares. Dependent variable is logarithm of $(1 + \text{registration})$. The key coefficients are displayed in bold for easy reference. Robust standard errors clustered by state in parentheses. Column (1): Preferred estimate including *enforcement × first wave* (cumulative registrations up to August 31). Column (2): Validation exercise on counties in states that provided state registry but did not merge with federal. Column (3): Validation exercise on counties in states that established state registry in 2003 and eventually merged with federal. Column (4): Falsification with placebo being *enforcement × logarithm of cumulative news* reports of DNC up to August 31. Column (5): Falsification with placebo being enforcement from November 1. Column (6): Falsification with placebo being enforcement from September 30. Column (7) Including measure of social interaction. Column (8): Including newspaper reports of number of people registering with DNC. Column (9): Comparing the effect of the first wave on registrations in October vis-à-vis November.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

column (3) reports an estimate limited to counties in these states. The coefficient of enforcement interacted with the first wave is positive, significant, and considerably larger than the preferred estimate. One reason could be that residents in these states disliked telemarketing relatively more than residents of states without any state-level registry, which is why the state government took action to help their residents preregister. Indeed, by August 31, the cumulative

DNC registrations per household in these states and California were 0.386 and 0.361, respectively, which exceeded the cumulative registrations per household in states without state registries, 0.328.

5.2. Falsification

We further check the robustness of our finding of an externality through three falsification exercises with placebos designed so that absent the externality, the

observed relation with later DNC registrations should be weaker. One placebo changes the source of the externality, whereas the other two arbitrarily vary the effective date of the externality.

The first placebo is cumulative news reports of the DNC registry in the county up to August 31 in place of the first wave (cumulative DNC registrations up to August 31). We aim to rule out the effect of the FTC enforcement on consumer DNC registration being through some other privacy-related difference among consumers. Newspapers tend to publish reports that interest their readers, so news reports of the DNC registry in the county would be correlated with residents' concern about privacy.

Table 4, column (4) reports the estimate including the placebo, which was enforcement interacted with the cumulative news reports of the DNC registry in the county up to August 31. The coefficient of the placebo is not significant, which is inconsistent with FTC enforcement affecting DNC registrations through some other privacy-related difference among consumers.

The second placebo stipulates that the FTC enforced the DNC registry from November 1 rather than October 1. This falsification exercise compares the effect of the first wave from November onward to its effect in September and October. If our analysis is correct, the estimated externality with the placebo should be weaker. Indeed, as reported in Table 4, column (5), the coefficient of the placebo, the post-November 1 indicator interacted with the first wave, is less than half the preferred estimate, which is consistent with the actual implementation being one month earlier.

Similarly, in the third placebo, we change the enforcement date from October 1 to September 30, which is just one day earlier. As reported in Table 4, column (6), the coefficient of post-September 30 interacted with the first wave is negative but insignificant. As explained in §3 and Figure 6, our identification strategy relies on a combination of the enforcement date (October 1 and after) and the first wave (cumulative registrations up to August 31). It is striking that shifting the "enforcement date" by just one day wipes out the "externality." This result shows clearly that our preferred estimate of the externality in Table 3, column (2) is not entirely driven by the first wave of registrations.

5.3. Alternative Explanations

The correlation between the first wave of registrations and postenforcement registrations can also be explained by social influence. The first wave of consumers may have noticed a drop in telemarketing calls, and their good experience prompted them to recommend the DNC registry to others, who had not yet registered.

We addressed this alternative explanation in two ways. One directly accounted for social influence using measures of social interaction from the Social Capital Community Benchmark Survey (Saguaro Seminar 2000). Recommendations by the first wave ought to have a more pronounced effect in communities with more social interaction. This implies that in a regression of DNC registration, the interaction of social interaction with enforcement and the first wave should be positive.

The Social Capital Community Benchmark Survey covers only some U.S. counties, so the inclusion of social interaction reduced the sample by more than half. As reported in Table 4, column (7), neither the measure of social interaction (i.e., connections through formal memberships and associations) in the postenforcement period nor its interaction with the first wave is significant. By contrast, the coefficient of enforcement interacted with the first wave is similar to the preferred estimate and close to being statistically significant ($p = 0.05$). The weaker statistical significance of the estimate might be due to the smaller sample size.¹⁴

We also address the alternative explanation by including the number of news reports of how many people had registered with DNC. The good experience of the first wave could have been spread through newspaper reports (instead of being directly communicated through social interaction). By the alternative explanation, reports of the number of consumers who had already registered with the DNC should have a larger effect from October onward. As Table 4, column (8) reports, the coefficient of the number of news reports of how many people had registered with DNC is positive and significant, but the coefficient of its interaction with enforcement is not significant. News reports did *not* have a larger effect when enforcement commenced on October 1. Collectively, the estimates in Table 4, columns (7) and (8) tend to rule out social influence as an alternative explanation for the externality in DNC registrations.

Another explanation of the correlation between the first wave of registrations and postenforcement registrations is telemarketer dumping. Suppose that, after October 1, telemarketers were surprised by the number of DNC registrations. Then, they dumped their excess calling resources on the (unregistered) consumers still available for telemarketing.

However, anecdotal evidence does not support the alternative explanation. Apparently, businesses were

¹⁴ In the online appendix, we report similar results using two other measures of social interaction, namely, organized social involvement and informal social interaction. The social interaction variables do not vary over time, so their (main) effects are captured by the county fixed effects and cannot be separately estimated.

aware of the DNC registry well ahead of October 1 and had already adjusted or were adjusting: “big corporations say they are taking the list in stride and will simply shift marketing dollars to avenues such as direct mail, TV, radio, print and online advertising.... Some big telemarketing services say they have shifted gears the past few years in anticipation of the do-not-call list. ... Calling Solutions expects to pare its staff of 4,000 telemarketers by 25% through attrition” (*USA Today* 2003, p. B01). Furthermore, the news media reported that telemarketer dumped resources *before* the DNC registry was enforced in October (*USA Today* 2003; *Plain Dealer* (Cleveland) 2003) but *not after*.

To complement the qualitative evidence, we estimate a specification that allows the effect of the first wave on daily DNC registrations to differ between October and November. As Table 4, column (9) reports, the effects of the first wave in October and November are very similar. Indeed, the hypothesis that the two coefficients are the same cannot be rejected ($F = 0.157$, $p = 0.695$). If vendors were surprised by the consumer DNC registrations and had dumped excess resources on unregistered consumers, the impact should have been larger in October. By November, they would have had up to two months (from September 2, when they were allowed to download the DNC registrations) to adjust their capacity and the effect of past registrations ought to attenuate. However, the empirical evidence suggests otherwise.¹⁵

The qualitative and quantitative evidence tends to rule out telemarketer dumping as an explanation for the externality in DNC registrations.¹⁶ Incidentally, the estimate in Table 4, column (9) also suggests that few telemarketers exited in the first two months of enforcement. With a smaller population of consumers to call, some telemarketers might not have covered their fixed costs, so exited the industry, causing the externality on unregistered consumers to be attenuated. However, the estimate in Table 4, column (9) does not suggest any such attenuation.¹⁷

¹⁵ A subtle hybrid of our hypothesis and the alternative explanation is that in October, vendors mechanically shifted their excess resources to calling unregistered consumers, and then discovered (to their surprise) an increase in yield. With this additional knowledge, they continued to maintain their calling strategy in the following month of November.

¹⁶ As a further check of the alternative explanation, reported in the online appendix, we extend the sample period to December 31 and consider the effect of the first wave on daily DNC registrations in the months of October, November, and December separately. The effect of cumulative registrations up to August 31 appeared to persist over time. There is no significant difference among the October, November, and December coefficients.

¹⁷ Consumer opt-out may generate a different indirect externality for other consumers—by reducing competition among vendors

6. Nature of the Externality

Our empirical results are consistent with an externality between the first wave of DNC registration and the decision of (not yet registered) consumers to register in the postenforcement period. For policy and managerial decision making, it is important to understand the nature of this externality. Why did the earlier DNC registrations influence other unregistered consumers to register?

The analyses of Hann et al. (2008) and Johnson (2013) suggest one mechanism. Telemarketers call consumers to communicate marketing offers that benefit consumers to different degrees depending on their preferences. Each call, whether the consumer answers it or not, directly imposes harm on the consumer. To avoid this harm, consumers can register with the DNC. They decide whether to register by comparing (i) the expected net benefit from calls that they choose to answer, with (ii) the harm caused by all calls that they receive.

In this setting, the DNC registry will induce self-selection among consumers according to their expected net benefit. Those with lower net benefit will be more likely to register. Hence, the remaining consumers (who have not registered) would be those with higher expected net benefit. They would be more receptive to telemarketing in the sense of being more likely to answer a call and accept an offer.

This interpretation is consistent with some anecdotal evidence. Alan Elias, spokesman for major credit card issuer Provident Financial, emphasized “If you’re marketing to individuals who have elected not to opt out, you may be marketing to a more receptive audience” (*USA Today* 2003, p. B01).

Given that DNC registrations lead to an increase in expected yield and profit from the remaining (unregistered) consumers, telemarketers would increase calls to the unregistered consumers. The increase in calls would directly increase the harm to all unregistered consumers and induce some marginal consumers to also register for DNC. Effectively, one consumer’s DNC registration (indirectly) generates negative externalities for consumers who have not yet registered.

6.1. Telemarketing Intensity

To the extent that the externality among consumers arises from telemarketers increasing calls to unregistered consumers, the magnitude of the externality should increase with the intensity of telemarketing. In counties with more intense telemarketing, the telemarketer response to the consumer self-sorting by

(Campbell et al. 2015). For another context in which consumer actions to protect their privacy may reduce vendor competition, see Armstrong et al. (2009).

expected net benefit would be larger. So, the externality on the unregistered consumers would be greater.

To check this explanation, we estimate a specification including a measure of telemarketing intensity. The FTC charged a fee for downloads of DNC registrations for more than five area codes, so the number of downloads is a reasonable measure of the actual intensity of telemarketing. For each county, we measure the intensity of telemarketing by the number of telemarketers that downloaded the DNC registrations up to October 31.¹⁸

For convenience, Table 5, column (1) reproduces the preferred estimate from Table 3, column (2), showing the presence of an externality. Table 5, column (2) reports the estimate including enforcement interacted with the first wave and with telemarketing intensity. The estimated coefficient is positive and significant, suggesting that, in counties with more intense telemarketing, the externalities were indeed larger.¹⁹ This result is consistent with the externality on unregistered consumers being caused by an increase in telemarketing.

We investigate the relation between the consumer externalities and telemarketing intensity in another way. This exploits the exogenous timing of the enforcement of the DNC registry and the pattern of downloads of the DNC registry by telemarketers. We compute, for each county, the total number of telemarketers that had downloaded the DNC registrations up to the previous day. If more telemarketers downloaded the DNC registry for a particular county, then more telemarketing resources could be re-allocated to unregistered telephone numbers, and hence, the solicitation pressure would be greater. Table 5, column (3) reports an estimate including enforcement interacted with the first wave and with the number of telemarketers who had downloaded the DNC registrations up to the previous day. The estimated coefficient is positive and statistically significant ($p < 0.05$), suggesting that with more telemarketers downloading the DNC registry, the solicitation pressure was indeed larger.²⁰

¹⁸ The FTC enforced the DNC registry from October 1, so in principle, all telemarketers should have downloaded the registry before then. However, there were a fair number of downloads during the month of October, so we felt it safer to include those as well.

¹⁹ For completeness, we also include the interaction of enforcement with the number of telemarketers who had downloaded the DNC registrations up to October 31. However, it is not possible to separately identify the interaction of the first wave with the number of downloads. This interaction varies by county but not with time, so is absorbed by the county fixed effects.

²⁰ For completeness, we include the number of telemarketers who had downloaded the DNC registrations up to the previous day, and its interactions with the first wave and enforcement.

6.2. Differences in Consumer Benefit

In the analyses of Hann et al. (2008) and Johnson (2013), consumers differ in their expected net benefit from telemarketing. Referring to Figure 1, registrations peaked in the first few days, between June 27–30, dropped, and then peaked again in early July. Strictly, all consumers who registered by August 31 would get the same benefit from the DNC registration. So, why did so many consumers rush to register early? A reasonable explanation is that those who disliked telemarketing the most were the first to register with the DNC, whereas those who disliked telemarketing relatively less registered later. From the telemarketer viewpoint, the earlier DNC registrations were relatively more helpful, because they removed the consumers who were least interested in telemarketing offers. This implies that the externalities from the earlier registrations would be larger than those from later registrations.

To test this implication, we estimate a specification focusing on the externality from consumers who registered for DNC in July and August rather than the entire first wave, who registered from June to August. As Table 5, column (4) reports, the coefficient of enforcement interacted with July–August DNC registrations, 0.479 (s.e. 0.184), is much smaller than the estimated externality from the entire first wave, as reported in Table 5, column (1). This result is consistent with earlier registrants being relatively less interested in telemarketing offers and their registration giving rise to a relatively larger externality.

Another way to check for differences in consumer benefit uses the second wave of DNC registrations. Suppose that consumers opted out according to their expected benefit, then the externality from the second wave would be weaker than from the first wave. Indeed, the estimates reported in Table 3, columns (7) and (8) are consistent with the externality from the second wave being weaker.

6.3. Market Size

For policy and managerial decision making, it is useful to know the characteristics of the market that influence the magnitude of the externality in DNC registrations. Prior research suggests that market size may affect seller targeting because of economies of scale, and that sellers segment markets by consumer demographics (Waldfoegel 2003, George and Waldfoegel 2003). In our setting, market size and segmentation may affect vendors' targeting and hence the solicitation pressure. We investigate four characteristics—size, inequality of income, and differences in education and race—that affect vendors' choice of markets to serve (Waldfoegel 2003).

Table 5, column (5) reports an estimate including enforcement interacted with the first wave and with

Table 5 Nature of the Externality

Variables	(1) Preferred	(2) Tele- marketing intensity	(3) Tele- marketer downloads	(4) July– August registration	(5) Market size	(6) Income inequality	(7) Education hetero- geneity	(8) Racial hetero- geneity
<i>News reports</i> + 1 (ln)	0.026*** (0.008)	0.026*** (0.008)	0.027*** (0.008)	0.026*** (0.008)	0.026*** (0.008)	0.026*** (0.008)	0.026*** (0.008)	0.026*** (0.008)
<i>Cumulative registrations</i> [<i>t</i> − 1] (ln)	1.562 (1.337)	1.878 (1.368)	1.874 (1.468)	−0.929 (0.859)	1.972 (1.382)	1.512 (1.326)	1.780 (1.353)	1.772 (1.345)
<i>Enforcement</i> × <i>Cumulative registrations</i> [<i>t</i> − 1] (ln)	−3.092*** (0.735)	−3.591*** (0.726)	−3.687*** (0.816)	−0.625*** (0.177)	−3.674*** (0.756)	−3.082*** (0.738)	−3.360*** (0.777)	−3.399*** (0.741)
<i>Enforcement</i> × <i>First wave</i> (ln)	2.932*** (0.738)	3.355*** (0.728)	3.425*** (0.811)		3.349*** (0.759)	2.972*** (0.723)	2.712*** (0.752)	3.199*** (0.743)
<i>Enforcement</i> × <i>Cumulative downloads Oct 31</i> (ln)		−0.111*** (0.027)						
<i>Enforcement</i> × <i>First wave</i> (ln) × <i>Cumulative downloads Oct 31</i> (ln)		0.016*** (0.002)						
<i>Cumulative downloads</i> [<i>t</i> − 1] (ln)			−0.558*** (0.130)					
<i>Enforcement</i> × <i>Cumulative downloads</i> [<i>t</i> − 1] (ln)			−0.016 (0.048)					
<i>First wave</i> (ln) × <i>Cumulative downloads</i> [<i>t</i> − 1] (ln)			0.042*** (0.011)					
<i>Enforcement</i> × <i>First wave</i> (ln) × <i>Cumulative downloads</i> [<i>t</i> − 1] (ln)			0.010* (0.004)					
<i>Enforcement</i> × <i>Registration Jul–Aug</i> (ln)				0.479** (0.184)				
<i>Enforcement</i> × <i>Total households</i> (ln)					−0.131*** (0.030)			
<i>Enforcement</i> × <i>First wave</i> (ln) × <i>Total households</i> (ln)					0.017*** (0.002)			
<i>Enforcement</i> × <i>Inequality of income</i>						0.478 (0.915)		
<i>Enforcement</i> × <i>First wave</i> (ln) × <i>Inequality of income</i>						−0.119 (0.141)		
<i>Enforcement</i> × <i>Educational heterogeneity</i>							−4.286*** (1.371)	
<i>Enforcement</i> × <i>First wave</i> (ln) × <i>Educational heterogeneity</i>							0.586** (0.225)	
<i>Enforcement</i> × <i>Racial heterogeneity</i>								−0.812*** (0.121)
<i>Enforcement</i> × <i>First wave</i> (ln) × <i>Racial heterogeneity</i>								0.119*** (0.021)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	109,200	109,200	109,200	109,200	109,200	109,200	109,200	109,200
<i>R</i> -squared	0.632	0.633	0.635	0.632	0.633	0.633	0.633	0.633
Counties	1,200	1,200	1,200	1,200	1,200	1,200	1,200	1,200

Notes. Estimated by ordinary least squares. Dependent variable is logarithm of (1 + *registration*). The key coefficients are displayed in bold for easy reference. Robust standard errors clustered by state in parentheses. Column (1): Preferred estimate including *enforcement* × *first wave* (cumulative registrations up to August 31). Column (2): Including telemarketing intensity (logarithm of cumulative telemarketer downloads up to October 31). Column (3): Including cumulative telemarketer downloads up to previous day. Column (4): Externality from registrations in July and August only. Column (5): Effect of market size (number of households) on externality. Column (6): Effect of income inequality on externality. Column (7): Effect of heterogeneity of education on externality. Column (8): Effect of heterogeneity of race on externality.

p* < 0.1; *p* < 0.05; ****p* < 0.01.

market size (measured by the number of households in the county). The coefficient of this interaction, 0.017 (s.e. 0.002), is positive and significant, suggesting that the externality was stronger in larger counties. Specifically, compared with a county with average size, a county that was one standard deviation, or 89,669

households, larger, the externality would have been larger by $0.017 \times [\ln(31,329 + 89,669) - \ln(31,329)] \times \ln(12,385) = 0.212$ (s.e. 0.029), which is about 6.3% of the coefficient, 3.349 (s.e. 0.759), representing the externality. This market size effect is statistically and economically significant.

The market size effect suggests the presence of a *diseconomy* of scale in telemarketing. To the extent that the externality was due to heterogeneity in consumer benefits, it suggests that, in larger markets, there was relatively more heterogeneity in consumer benefits that telemarketers had not addressed. So, the DNC registry led to relatively more consumer self-selection, and the pool of (unregistered) consumers available for telemarketing was relatively more favorable to telemarketing offers. Hence, telemarketers increased solicitations to them, which caused more of them to register with the DNC. The managerial implication is that the DNC benefited telemarketers relatively more in larger markets.

We then investigate three consumer demographics—income, education, and race—that vendors might use as bases of targeting (Waldfogel 2003, George and Waldfogel 2003). We estimate specifications including the interaction of enforcement with the measure of county-level heterogeneity (inequality of income or heterogeneity of education or race) as well as its interaction with the first wave. As reported in Table 5, columns (6)–(8), the externality was not significantly related to income inequality but was significantly and positively related to heterogeneity of education and race. Apparently, telemarketers had not fully segmented consumers by education and race, so the DNC benefited telemarketers relatively more in markets that were educationally or racially heterogeneous.

7. Implications and Conclusions

The FTC began to enforce the DNC registry against telemarketers on October 1, 2003. Thereafter, daily DNC registrations were higher in counties where more consumers had registered with DNC by the deadline of August 31, 2003. This empirical relation presents a puzzle. From October 1, telemarketers would have curtailed their calling to consumers who had *registered* with DNC. But why should that have changed DNC registrations among *unregistered* consumers? And why should the increase in DNC registrations after October 1 be positively correlated with the first wave of consumers (who had registered up to August 31)?

We interpret this empirical relation—that postenforcement DNC registrations were positively correlated with the first wave—as an externality among consumers. Our empirical strategy hinges on the exogenous timing of enforcement of registrations before and after August 31. We could rule out explanations based on diffusion and social influence. Our interpretation of the empirical relation as an externality stands up to multiple robustness tests, as well as validation and falsification exercises.

Two mechanisms possibly underlie the externality. First, consumers might differ in their expected net benefit from telemarketing offers. Those with lower benefit registered with DNC, so the unregistered consumers were relatively more interested in telemarketing offers. Then, telemarketers increased calls, which prompted some of the previously unregistered consumers to register as well (Hann et al. 2008, Johnson 2013). Second, after October 1, consumers who had registered with DNC noticed a distinct reduction in telemarketing calls and shared their experience with others, who then registered with DNC.

The empirical evidence on the underlying mechanism is suggestive rather than conclusive. It points toward the explanation in terms of differential consumer benefit, which is consistent with theoretical analyses of marketing avoidance (Hann et al. 2008, Johnson 2013). By contrast, the empirical evidence points away from the other explanation.

Our results provide insight and guidance to both public policy and managerial practice. Direct marketing takes many forms, including personal selling, direct mail, telemarketing, email marketing, and mobile text messaging. Direct marketing imposes harm on consumers and direct marketing vendors compete for consumers' limited attention. In this context, previous research has variously recommended Pigouvian taxes (Shiman 1996, Van Zandt 2004, Anderson and de Palma 2009), and attention fees (Ayres and Funk 2003, Loder et al. 2006, Shiman 2006). However, in practice, policy makers and managers have much preferred to regulate or self-regulate the industry through opt-out ("do not contact") facilities. Our results suggest that opt-out facilities give rise to externalities between consumers. Such externalities may cause marginal consumers who otherwise may be interested in direct marketing offers to also opt out of direct marketing.

To the extent that the externality is due to marketers perceiving that unregistered consumers are relatively more receptive of marketing offers than registered consumers, so increasing their solicitations, our results provide further insight and guidance. Department store magnate John Wanamaker is famously credited with the lament, "Half the money I spend on advertising is wasted; the trouble is I don't know which half." By allowing consumers to opt out, managers can increase the marginal effectiveness of their marketing expenditures. Furthermore, our results also suggest that businesses can expect relatively larger gains from such consumer self-selection in markets that are larger and more educationally or racially heterogeneous.

With regard to public policy, our findings provide support for opt-out facilities as a way to address congestion in advertising and direct marketing. By contrast with Pigouvian taxes (Shiman 1996, Van Zandt

2004, Anderson and de Palma 2009), opt-out facilities might increase social welfare by enabling consumers to self-select by their expected net benefit. The major limitation of opt-out facilities is that they do not allow consumers to choose between offers that interest them relatively more or less. Such facilities reduce welfare to the extent that marginal consumers are actually interested in some of the marketing offers.

The weaknesses of the blanket opt-out facilities could be addressed through selective opt-out facilities or attention (receiver) fees (Ayres and Funk 2003, Loder et al. 2006). Unfortunately, such schemes are also distortionary. In selectively opting out or setting the attention fees, consumers ignore the vendors' profit and distort the volume of solicitations below the welfare maximum (Hann et al. 2008, Anderson and de Palma 2009). So, in principle, neither Pigouvian taxes, opt-out facilities, nor attention fees achieve the welfare optimum.

Another limitation of opt-out facilities is that the externalities between consumers might lead to excessive opt-out, and therefore, to deadweight losses. Our empirical inference suggests that strategic marketing by sellers may have been one cause of such externalities. Mitigating sellers' incentives to overly solicit (remaining) consumers who have not opted out may be a challenge to a policy of using such facilities.

Finally, although our empirical analysis focuses on the federal DNC registry, the implications extend to Internet advertising. Consumers browsing the World Wide Web have limited time and processing—clicking and reading advertising messages and solicitations is costly. Despite advances in information technology, advertising and solicitations through the Internet are not perfectly targeted, and consumers do avoid marketing (Goldfarb and Tucker 2011). So, any “do not track” facility would facilitate consumer self-selection by their expected net benefit. Indeed, to the extent that the harm caused by Internet tracking, the “creepy” feeling of being followed (*New York Times* 2012), is less tangible and smaller than the nuisance of direct marketing, the do not track facility would lead to relatively less sorting by consumer harm. Hence, as a mechanism to sort consumers by their expected benefit, the opt-out facility should be even more effective in the online context than telemarketing. This reasoning supports so-called do not track bills that have been introduced in the U.S. House of Representatives by Jackie Speier and in the California Senate by Alan Lowenthal.

Supplemental Material

Supplemental material to this paper is available at <http://dx.doi.org/10.1287/mnsc.2014.2051>.

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