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Research Note

Migration of Service to the Internet: Evidence from a Federal Natural Experiment

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Previous research into consumer choice of service channels studied the impact of online access as an addition to conventional service. Here, we study the impact of a compulsory migration to an online channel. We exploit a natural experiment in the implementation of a new federal government service to identify the causal effect of access channel on consumer choice. The government served western states through the Internet and telephone at all times. However, for the first 10 days, the government served the East through the Internet only. Comparing consumer responses in the East (only Internet service available) and West (both Internet and telephone service available), we find robust evidence that some consumers preferred telephone access. The unavailability of telephone service in the first 10 days resulted in a 4.3% loss of consumers who were otherwise interested in the service.

Keywords: service migration; channel choice; Do Not Call Registry; natural experiment
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1. Introduction

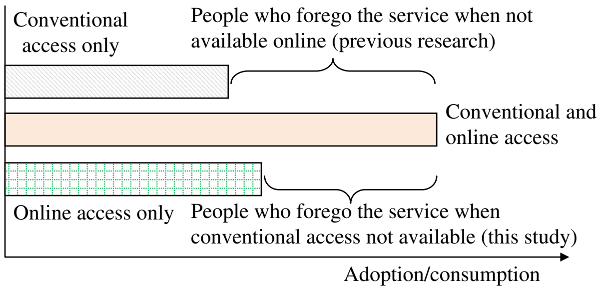
All over the world, businesses and governments are aggressively steering consumers away from counter and telephone toward online service. In 2011, the state of Florida enacted a law that required application for unemployment benefits through the Internet, an online skills test, and regular reports on search for work. The German airline Lufthansa charges American customers US$20 more for bookings through its call center than those through its website. Lufthansa's British reservations center charges 10 pence a minute for incoming calls. In Massachusetts, Bank of America charges a fee of US$12 in any month that "eBanking" checking account customers use a (human) teller or request a paper statement. How would such migration to online service affect consumers?

Consumers who prefer the service through the Internet would benefit from the convenience. Others, who prefer in-person or telephone service, might lose. Some might cope, incurring some monetary or psychic cost to use the Internet service. Still, others would be unable to cope and be completely excluded from the service. For management practice and public policy, it is important to identify the effects of migration to a digital platform. Managers need to know the loss of sales and profit. Policy makers need to know the loss of consumer welfare. Both managers and policy makers need to know how to address the consumers who suffer loss.

To identify and quantify the losses from service migration, we need to compare the outcomes in a setting with *only online access* against those in an otherwise identical setting with both conventional and online access. Referring to Figure 1, the experiment should be designed so that the control group has both conventional and online access, whereas the treatment group is allowed only online access. It is not easy to observe such an experiment in the field, because few organizations would move all service to the Internet.

Here, we exploit a natural experiment in the administration of the federal Do Not Call (DNC) Registry. The Federal Trade Commission (FTC) opened the DNC Registry to consumers on June 27, 2003. From the start, *all* consumers could register through the Internet. However, for the first 10 days, up to July 6, 2003, only people in states west of the Mississippi River could register by calling a toll-free line. From July 7 onward, all people could register through the toll-free line. This exogenous difference in the treatment of people living in the East and West, and before and

Figure 1 (Color online) Experimental Design



after the first 10 days, provides a unique opportunity to identify the *causal* effect of conventional access on actual consumer choice.[1]

Our empirical strategy applies a difference-in-differences (DID) design using only the counties immediately east and west of the Mississippi River. We contribute toward a better appreciation of the implications of service migration to Internet channels in several ways. First, we find robust evidence that some consumers cannot access service on the Internet. These people are only able or willing to access service through conventional means. Such consumers, however, are less prevalent among better-educated people.

Second, we find evidence of intertemporal substitution in the registration channel. In the first 10 days, registrations were 27% lower in areas without toll-free telephone registration, but the majority of lost registrations were recovered once toll-free registration became available after 10 days. Apparently, some consumers waited to register by telephone. If toll-free registration was not available, some of these "waiters" might have registered through the Internet, whereas others might have been lost.

Finally, we show that newspaper reports influence consumers in their choice of service channel. Where and when consumers could register by toll-free call, news reports including the toll-free number were associated with higher registrations. Where and when consumers could not register by toll-free call, news reports that they could register by telephone after 10 days were associated with lower registrations. Whereas previous research (Goh et al. 2011) showed that general newspaper publicity affects consumer behavior, our results go further to show the effect of specific newspaper content on consumer behavior.

2. Related Literature

This study is related to three streams of research. The first stream broadly considers the welfare effects of new information and communication technologies, for instance, personal computers among entrepreneurs (Fairlie 2006) and students (Goolsbee and Klenow 2002), mobile phone service among fishermen (Jensen 2007), and Internet information kiosks among farmers (Venkatesh and Sykes 2013). The main contribution of these studies is to identify the sources and gauge the magnitude of the benefit from the new technologies.

The second related stream is research into the "digital divide." This literature has addressed differences in access to new technology (Forman 2005, Forman et al. 2005), usage (Dewan and Riggins 2005, Goldfarb and Prince 2008), and outcomes (Wei et al. 2011), as well as the technological, social, and geographical factors, and government policies that affect the differences in access, usage, and outcomes (Agarwal et al. 2009, Arora et al. 2010, Dewan et al. 2010, Hsieh et al. 2011). The digital divide research implicitly assumes that all users would benefit from new technology, and so focuses on barriers to access or use.

A common thread among these two streams of research has been to compare situations with both new technology and conventional alternatives against the situations with only the conventional means. Referring to Figure 1, the experimental design in these studies focuses on people who *need or prefer the new technology*. As Jensen (2007, p. 920) so aptly remarked, this design actually characterizes the "*digital provide*."

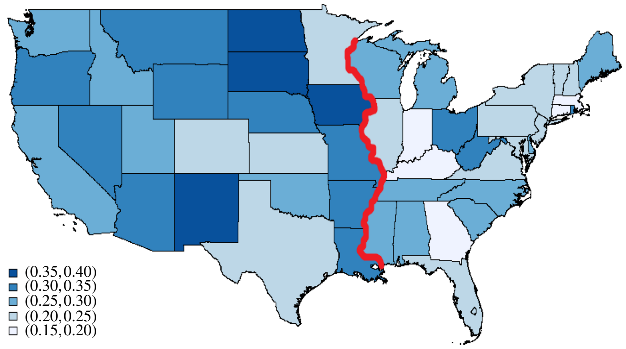
However, with voluntary adoption of new technology, there might be self-selection. The more sophisticated users would adopt the new technology, whereas the less sophisticated users and those who get less benefit from the new technology would continue using conventional methods. The self-selection may bias upward the estimated benefit of migration.

The third stream of research studies the implications of serving customers through online channels. In retail banking, the bank's profit may increase with customer use of online channels, although customer online efficiency depends on characteristics such as education and computer skills (Xue et al. 2007). In general retailing, some consumers prefer the conventional channel, so the opening of a physical store is associated with lower online purchases (Forman et al. 2009). In medical insurance services, providing extensive information online might generate uncertainty and lead some customers to increase telephone enquiries (Kumar and Telang 2012). This intriguing result suggests that steering consumers toward an online channel need not necessarily reduce the provider's costs.

In line with the third stream of research, we study the implications of compulsory migration to an online channel, especially the impact on consumers who cannot or prefer not to use online service. Such consumers may suffer if the new channel is the only way to access service. We aim to identify such losses and

[1] Varian et al. (2004) highlighted this administrative difference.

Figure 2 (Color online) Percentage of DNC Registration by Toll-Free Call, September 16, 2003



how to mitigate the losses. Furthermore, we are interested in the use of media publicity to influence consumers' choice of service channel.

3. U.S. Do Not Call Registry

The FTC contracted with AT&T Government Solutions to administer the DNC Registry (Federal Trade Commission 2003a). Registrations were accepted from June 27, 2003, through the website, www.donotcall.gov, as well as a toll-free number, (888) 382-1222. Concerned about the expected high volume of calls, the FTC decided to limit the toll-free access to states west of the Mississippi River, including Louisiana and Minnesota, until July 6 and open it nationwide only 10 days later, from July 7. The thick line in Figure 2 depicts the division of the country by registration method in the first 10 days, from June 27 to July 6.

The DNC webpage provides space to register up to three telephone numbers. The consumer must submit the numbers, wait for a confirmation email for each number, and then click on the confirmation to complete the registration. The toll-free service allows registration of only the number from which the call is placed. It uses automatic number identification to detect the number and then prompts the caller to confirm and complete the registration.

In its initial publicity, the FTC emphasized that people who preferred to register by telephone should simply wait until July 7 (Federal Trade Commission 2003b). Newspapers did convey the message. For instance, on June 27, 2003, the *St. Louis Post-Dispatch* reported: "In Missouri and all other states west of the Mississippi River, all you have to do to register your residential and wireless numbers is call 888-382-1222. Phone registration for Illinois and the other states east of the river begins July 7."

Within the first four days, by July 1, consumers had registered 15.3 million phone numbers, 12.1% of which were registered through the toll-free number (Federal Trade Commission 2003c). By September 16, consumers had registered 41.7 million phone numbers, 25.9% of which were registered through the toll-free number (Federal Trade Commission 2003d).[2] So, between July 1 and September 16, the proportion of telephone numbers registered through toll-free call more than doubled. These aggregate statistics are consistent with the expansion of toll-free registration to the entire country from July 7 onward.

Figure 2 shows the proportion of the telephone numbers registered through toll-free call as of September 16. A darker shade corresponds to a higher proportion of toll-free registration. It is clear that more numbers were registered through toll-free call in the western states. Figure 3(a) depicts the daily DNC registrations per household in the counties immediately to the *east* of the Mississippi. Clearly, there were two peaks in registration. The first peak occurred around days 2–5. Registrations jumped again on day 11 (July 7), when toll-free registration became available in the East. Registrations remained elevated and declined to the pre-toll-free (day 10) level only after day 15.

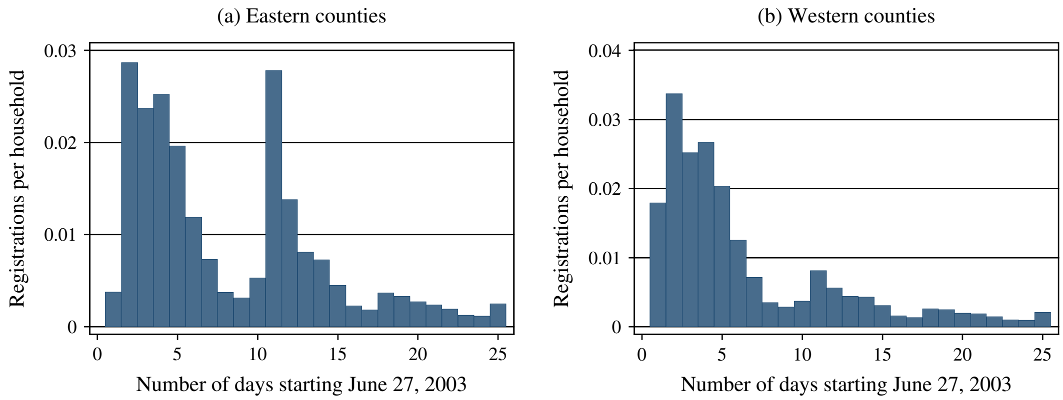
Figure 3(b) depicts the daily DNC registrations per household in the counties immediately to the *west* of the Mississippi. By contrast with the eastern counties, registrations started high on day 1, peaked on days 2–4, and declined thereafter. Apparently, consumers in the West were faster in registering than those in the East, which could be due to the earlier availability of toll-free registration. Interestingly, there was a small spike on day 11, possibly because people in western counties were influenced by publicity about when toll-free registration would become available in the East.

Comparing Figures 3(a) and 3(b), the profile of DNC registration over time was almost identical across the eastern and western counties except for the sharp difference on day 11 (when toll-free registration became available in the East) and a few days following. In particular, registrations peaked on day 2, dropped on day 3, increased again on day 4, declined gradually until day 9, and increased slightly on days 18 and 25.

The higher registrations in the East on day 11 and several days following are consistent with some eastern residents waiting to register by toll-free call. If every easterner who postponed registration from days 1–10 did so by exactly 10 days each, then, the boost to eastern registrations would have begun on day 11 and ended on day 20. Indeed, our regression estimate reported below confirms that registrations in eastern counties were significantly higher than in western counties from day 11 up to day 16

[2] The total of 41.7 million excludes 8.6 million telephone numbers imported from state-level DNC registries.

Figure 3 **(Color online) Registrations per Household**



and on days 18 and 20, and not significantly different thereafter.

Apart from the shift of some eastern registrations from days 1–10 to days 11–20, the diffusion profiles were very close, suggesting that consumers in eastern and western counties had very similar preferences for the DNC Registry. To this extent, any differences in DNC registration before and after the first 10 days, and between eastern and western counties, were likely the outcome of exogenous differences in registration channel.[3]

4. Model and Empirical Strategy

Generally, commercial and government services fall into two categories. One is services to arrange or reserve something else, for instance, to buy a DVD, book airline travel, or arrange a driving test. The other category is where the service is the end use itself, for instance, news, entertainment, or education. The DNC Registry belongs to the former category—it helps consumers avoid telemarketing calls, but otherwise does not provide any consumption benefit in itself. Once registered, consumers obtain identical benefit from the DNC service regardless of the means of registration. Hence, any observed difference in DNC registration before and after the first 10 days, and between eastern and western counties, should be due to differences in the available registration channels.

As presented in Figure 1, the ideal experimental design provides the control group with both conventional and online access, whereas allowing the treatment group only online access. The FTC implementation of the DNC Registry does not completely fit the ideal experimental design. The misfit is that consumers in the East could register through toll-free call by waiting 10 days until July 7.

Suppose that there are three consumer segments: (i) prefer to register online, (ii) prefer to register by telephone, and (iii) do not want the DNC service. The behavior of segments (i) and (iii) does not depend on the toll-free access. Segment (ii)—consumers who prefer the telephone—will be affected if registration by telephone is not available.

Referring to Figure 4, in the eastern counties, consumers who prefer registration by telephone comprise three subsegments. Some cope without telephone registration and instead register online ("copers"). Some know that they can register by toll-free call later and so wait ("waiters"). The remainder are those who cannot cope and do not know that they can wait and register by telephone later. They do not get the service ("lost consumers").

In the ideal experimental design, there would be no option to register through toll-free call by waiting, and hence no subsegment of waiters. The waiters would either cope and use the online service or not get the service. So, any estimate of the subsegment of the lost consumers from the DNC experiment would *underestimate* the number of lost consumers. Similarly, the DNC experiment would underestimate the number of copers.

We employ a DID strategy and identify the effect of compulsory migration ("Internet only") both in cross section (between western and eastern counties in the first 10 days) and within eastern counties (before and after day 10). We focus on the counties immediately east and west of the Mississippi River to limit the unobserved heterogeneity in the sample.

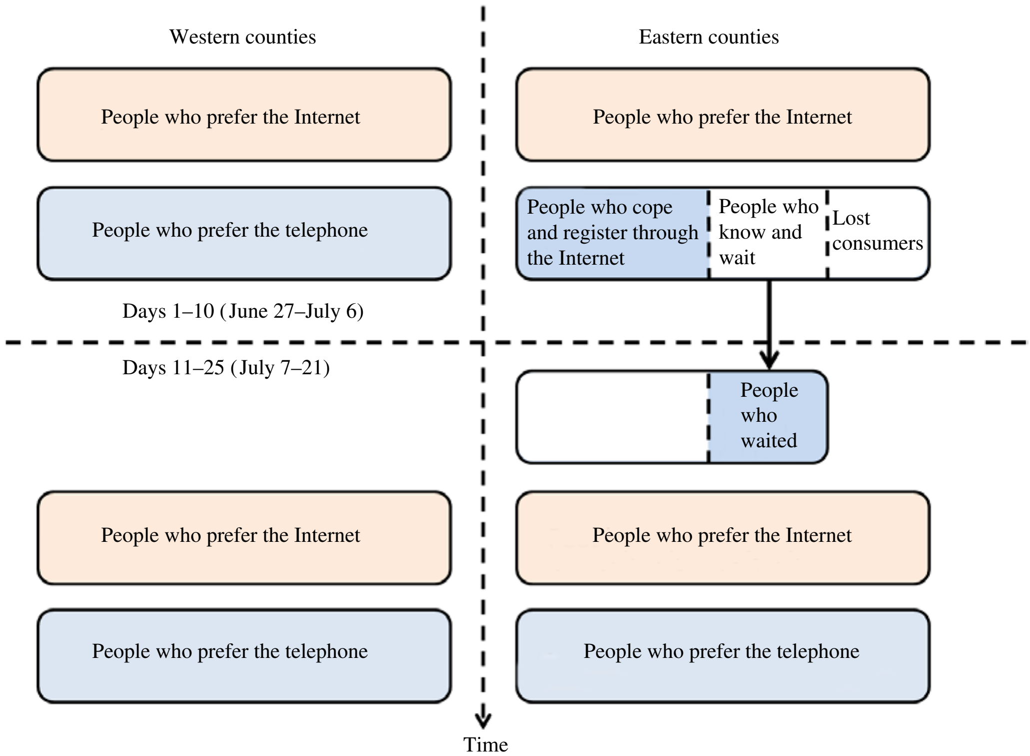
Econometrically, our basic model is a county-level DID model

$$\ln Q_{it} = \alpha + \beta I_{it} + \gamma X_{it} + \delta_i + \tau_t + \varepsilon_{it}, \tag{A}$$

where Q_{it} is the DNC registration rate, calculated as one plus the number of DNC registrations divided

[3] The FTC has not disclosed how it decided which areas received priority in toll-free registration. To the extent that the FTC gave priority to the states whose residents were less Internet capable, the difference in registration between states with and without toll-free access should be smaller. So, the FTC's choice, if deliberate, should bias against finding any effect of having only Internet registration.

Figure 4 (Color online) Identification Strategy



by the number of households in county i on day t; I_{it} is an indicator of only Internet registration in county i on day t (equal to 1 for counties in the East from June 27 to July 6, and 0 otherwise); X_{it} represents information about the DNC Registry, which we operationalize by the logarithm of one plus the number of newspaper reports divided by the number of households; δ_i represent county fixed effects; τ_t represent day fixed effects; and ε_{it} captures any residual random errors. The county fixed effects account for non-time-varying factors such as Internet access, telephone penetration, number of households, and income on DNC registration. The day fixed effects account for time-varying factors such as congestion at the DNC website that affected registrations in all counties.[4]

In all specifications, we estimate the standard errors, ε_{it}, clustered by county to control for intertemporal correlations in DNC registrations within counties (Bertrand et al. 2004). Together with the county effects, δ_i, and the day effects, τ_t, our analysis focuses on explaining differences in registrations from county and daily averages and, particularly, the incremental differences due to registration being available only through the Internet.

In all regressions but one, we specify DNC registrations per household and newspaper reports per household in logarithms.[5] For brevity, in the discussion below, we simply refer to the variable itself and omit mention of the logarithm.

5. Data

The FTC provided us with DNC registration data from the beginning of the registry on June 27, 2003. Prior to the opening of the federal DNC Registry, 27 states had already established state-level DNC registries (Federal Trade Commission 2003a, Varian et al. 2004). From July 22 onward, some of these states added their lists to the federal registry. Because we could not identify which telephone numbers were added from a state registry, we limit our analysis to registrations between June 27 and July 21. This provides a 25-day window of analysis.

The FTC records provide registrations by redacted telephone number for each area code and exchange, e.g., (617) 363-xxxx, by date of registration. We match

[4] By comparison with the standard DID design, the DNC experiment is "inverted." The "treatment" is registration through the Internet only, whereas the control is registration both by telephone and through the Internet. The unusual aspect of the DNC experiment is that the treatment applies in the first stage. In the typical DID design, the treatment applies in the second stage.

[5] Empirical analyses often fit better with economic variables specified in logarithm (Wooldridge 2006, pp. 197–200). In one robustness check, we specify the dependent variable as DNC registrations per household in its native form rather than as a logarithm.

Table 1 Summary Statistics

Variables (per household per day $\times 10^{-3}$)	West *N*	West Mean	West Std. dev.	East *N*	East Mean	East Std. dev.	*t*-statistic
			Days 1–10				
DNC registrations	600	10.448	10.301	490	8.506	7.715	3.554***
No. of news reports of "do not call"	600	0.132	0.462	490	0.254	1.421	−1.815*
with toll-free number	600	0.082	0.395	490	0.141	1.076	−1.150
that mentioned 10-day wait	600	0.070	0.304	490	0.197	1.379	−2.011**
			Days 11–25				
DNC registrations	900	2.482	2.826	735	4.724	6.241	−9.015***
No. of news reports of "do not call"	900	0.071	0.391	735	0.048	0.250	1.451
with toll-free number	900	0.040	0.311	735	0.023	0.182	1.389
that mentioned 10-day wait				—n.a.—			
			Overall				
DNC registrations	1,500	5.668	7.901	1,225	6.237	7.112	−1.976**
No. of news reports of "do not call"	1,500	0.096	0.422	1,225	0.130	0.925	−1.213
with toll-free number	1,500	0.057	0.348	1,225	0.070	0.697	−0.608
that mentioned 10-day wait	1,500	0.028	0.195	1,225	0.079	0.877	−2.000**

Note. All variables are computed by county and day. News report variables are weighted by the circulation in the county.
*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

Table 2 Correlations

		1	2	3	4
1	DNC registrations	1			
2	No. of news reports of "do not call"	0.17	1		
3	with toll-free number	0.18	0.83	1	
4	that mentioned 10-day wait	0.02	0.88	0.77	1

all DNC registrations to the respective counties, and so identify the registrants' geographical locations. We then merge the registrations with daily measures of newspaper reports of the DNC Registry by county. Because many newspapers circulate in multiple counties, we weight the number of reports by the circulation of the newspaper in the respective county (Goh et al. 2011). For example, in Orleans Parish, a report of the DNC Registry in the *Times-Picayune* (based in New Orleans) is weighted higher than a report in the *Advocate* (based in Baton Rouge).[6]

We organize the data set, including DNC registrations and newspaper reports, by county and day. Table 1 presents summary statistics of the data. Table 2 reports correlations.

6. Estimates

Using ordinary least squares regression with standard errors clustered by county, we estimate Model A including the indicator of only Internet registration (equal to 1 in counties east of the Mississippi River from June 27 to July 6 and 0 otherwise). As reported in Table 3, Column (1), the coefficient of *Internet only*, −0.659 (s.e., 0.057), is negative and significant. In counties and at times when consumers could only register through the Internet, DNC registrations were about 49% lower than in other counties and at other times.[7] In our sample, the mean of DNC registration is 0.006 per household per day. So, without the toll-free line, DNC registrations were lower by around $0.006 \times 0.49 = 0.003$ per household per day.

The coefficient of the weighted number of newspaper reports of the DNC Registry, 0.038 (s.e. 0.017), is positive and significant, suggesting that a 1% increase in reports was associated with a roughly 0.038% increase in DNC registration.[8]

We next estimate a daily version of the DID model

$$\ln Q_{it} = \alpha + \beta_t EAST_i \times \tau_t + \gamma X_{it} + \delta_i + \tau_t + \varepsilon_{it}, \quad \text{(B)}$$

where $EAST_i$ is an indicator of an eastern county (equal to 1 if county i is east of the Mississippi and 0

[6] Please refer to the online appendix (available as supplemental material at http://dx.doi.org/10.1287/isre.2015.0580) for details of the variables and construction.

[7] The dependent variable is the logarithm of 1 plus the registrations divided by the number of households. The elasticity with respect to only Internet registration is roughly $100 \times (e^{-0.659} - 1) = -48.3\%$, and hence the precise elasticity (removing the "1" added to the registrations) is −49.4%. In all discussions below, including the computation of differences in daily registrations and estimates of consumer subsegments, we report the precise estimates after adjusting for the added 1.

[8] Applying the spirit of regression discontinuity (Lee and Lemieux 2010), in unreported estimates, we progressively expand the sample to all of the counties in the states along the Mississippi, and then to the entire United States. In all estimates, the coefficient of *Internet only* is negative and significant, but smaller in magnitude with each expansion of the sample. Apparently, the effect of having only Internet registration (so, lacking toll-free registration) was sharpest along the Mississippi.

Table 3 **Daily Registrations**

Variables	(1) Baseline estimate	(2) Daily effects	(3) Reports of toll-free number	(4) Reports of 10-day wait
Reports of DNC	0.038**	0.054***	−0.046**	0.058***
	(0.017)	(0.014)	(0.018)	(0.014)
Internet only	−0.659***		−0.618***	−0.593***
	(0.057)		(0.056)	(0.059)
East × *Day* 1		−2.230***		
		(0.123)		
East × *Day* 2		−0.360***		
		(0.129)		
East × *Day* 3		−0.150		
		(0.132)		
East × *Day* 4		−0.158		
		(0.132)		
East × *Day* 5		−0.241*		
		(0.139)		
East × *Day* 6		−0.145		
		(0.136)		
East × *Day* 7		0.135		
		(0.143)		
East × *Day* 8		0.095		
		(0.139)		
East × *Day* 9		−0.049		
		(0.131)		
East × *Day* 10		−0.097		
		(0.122)		
East × *Day* 11		0.696***		
		(0.120)		
East × *Day* 12		0.764***		
		(0.119)		
East × *Day* 13		0.626***		
		(0.117)		
East × *Day* 14		0.498***		
		(0.118)		
East × *Day* 15		0.316**		
		(0.145)		
East × *Day* 16		0.332***		
		(0.124)		
East × *Day* 17		0.112		
		(0.144)		
East × *Day* 18		0.262**		
		(0.126)		
East × *Day* 19		0.235*		
		(0.122)		
East × *Day* 20		0.437***		
		(0.128)		
East × *Day* 21		0.263*		
		(0.138)		
East × *Day* 22		0.162		
		(0.141)		
East × *Day* 23		0.234*		
		(0.140)		
East × *Day* 24		0.163		
		(0.151)		
Reports with toll-free no.			0.182***	
			(0.026)	
Internet only × *toll-free reports*			−0.177***	
			(0.037)	
Reports with 10-day wait				0.025
				(0.030)
Internet only × *10-day reports*				−0.211***
				(0.042)
Observations	2,725	2,725	2,725	2,725
Adjusted R-squared	0.815	0.862	0.822	0.822
Counties	109	109	109	109
States	10	10	10	10

Notes. The dependent variable is the log DNC registrations per household by county and day. All specifications include county and day fixed effects. Column (1) gives the baseline estimate of Model A, characterizing the effect of registration channel by Internet only (1 for counties in the East from June 27 to July 6, and 0 otherwise). Column (2) shows Model B, characterizing the effect of registration channel on a daily basis by an *East* × *Day* dummy, omitting day 25. Column (3) includes the weighted number of reports of the toll-free number and interaction with *Internet only*. Column (4) includes the weighted number of reports mentioning the 10-day wait for toll-free registration in the East and interaction with *Internet only*. Robust standard errors clustered by county are in parentheses.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

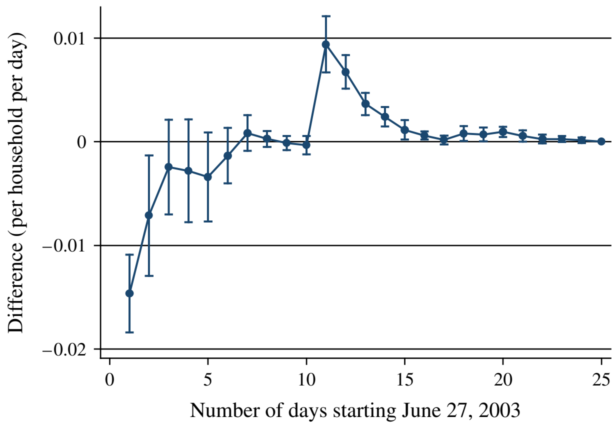
otherwise). The evolution of the coefficient, β_t, characterizes differences in DNC registration between eastern and western counties on a *daily* basis.

As reported in Table 3, Column (2), DNC registration was significantly *lower* in eastern counties on days 1 and 2, but not significantly different on days 3–10. Then, on days 11–16, DNC registration was significantly *higher* in eastern than western counties. From day 21 onward, there was no significant difference between eastern and western registrations. Figure 5 depicts the evolution of the differences in daily registrations, and their corresponding 95% confidence intervals, to provide a more intuitive appreciation of the differences.[9]

In the following analyses, for parsimony, we revert to Model A, which characterizes the effect of registration channel by differences between eastern and western counties in days 1–10 and within eastern counties before and after day 10.

[9] To compute the differences in daily registrations, we first calculate the counterfactual daily registrations in the eastern counties by subtracting the respective $\beta_t EAST_i \times \tau_t$ from the *predicted value* from the estimate of Model B. Then, we exponentiate the predicted and counterfactual registrations and calculate their difference for each day in the sample. Such exponentiation introduces bias (Goldberger 1968, Triplett 1989). Accordingly, following Wooldridge (2006, Example 6.7), we regress the daily registrations (not in logarithm) on the exponentiated predicted daily registrations from Model B to obtain the bias correction factor and apply this bias correction factor to all exponentiated predicted and counterfactual registrations before computing their differences. We apply this procedure in all subsequent estimates in §7.

Figure 5 (Color online) Daily DNC Registrations: East–West Difference



6.1. Additional Identification Strategies

To buttress our identification of the impact of registration channels, we allow the impact of the registration channel to vary with the content of newspaper reports. We compile two measures of content. One is mention of the toll-free number, and the other is mention that eastern residents could register by toll-free call from July 7 onward.

Table 3, Column (3), reports an estimate including the weighted number of newspaper reports per household of the toll-free number for DNC registration. The coefficient of *Internet only*, −0.618 (s.e., 0.056), is negative and significant. The coefficient of the weighted number of news reports of the toll-free number, 0.182 (s.e., 0.026) is positive and significant. Importantly for our identification strategy, this positive effect is almost completely nullified in counties and at times with only Internet registration. Specifically, the coefficient of the interaction between *Internet only* and the weighted number of news reports of the toll-free number is −0.177 (s.e., 0.037), and so the *net* effect of news reports with the toll-free number in counties and at times with only Internet registration is merely $0.182 - 0.177 = 0.005$, and not statistically significant ($p = 0.91$). Accordingly, news reports of the toll-free number affected registrations only when the toll-free line was available.

Table 3, Column (4), reports an estimate including the weighted number of newspaper reports per household that eastern residents could register by toll-free call from July 7 onward. The coefficient of *Internet only*, −0.593 (s.e., 0.059), is negative and significant. The coefficient of the weighted number of news reports that eastern residents could register by toll-free call from July 7 onward is not significant. Importantly for our identification strategy, the coefficient of the interaction between *Internet only* and the weighted number of news reports that eastern residents could register by toll-free call from July 7 onward, −0.211 (s.e., 0.042), is negative and precisely estimated. Apparently, these news reports reduced DNC registration in counties and at times when consumers could only register online through the Internet. These striking results are consistent with the existence of consumers who preferred to register by telephone and learned that they could register by telephone by waiting, and so delayed their registration.

These results also provide evidence that publicity through newspapers influences consumers in their choice of service channel. Importantly for policy and management, the effect was very specific to the message. Reports of the toll-free number only affected registrations in counties where and at times when toll-free registration was available. Reports that people could register through toll-free call after July 7 only affected registrations in counties where and at times when toll-free registration was not yet available.

6.2. Robustness

Our empirical model accounts for cross-sectional heterogeneity using county fixed effects, and the data set spans only 25 days, so it is unlikely that the empirical relation between Internet only registration and lower registrations is due to county demographics, such as Internet penetration. One alternative explanation of the lower DNC registrations in counties and at times with only Internet registration is difficulties that consumers faced with online registration. Initially, the DNC website was congested, and Yahoo's mail server inadvertently classified the DNC confirmation emails as spam (*Los Angeles Times* 2003). Western residents who encountered difficulty with online registration could register by toll-free call, but eastern residents could not. So, there would be a negative correlation between early registrations and the lack of toll-free registration.

However, the initial difficulties with online registration cannot account for the positive impact of newspaper reports, as reported in Table 3, Column (1). If limited registration capacity were the explanation, additional newspaper reports would simply add to congestion and have no significant effect on registration. Furthermore, by day 5, congestion at the DNC website and difficulties with Yahoo mail had been resolved.[10] So, difficulties with the Internet cannot account for the spike in registrations in eastern counties on day 11, upon the opening of toll-free registration in the East (Figure 3).

[10] By day 2, Yahoo had tuned its spam filter to recognize the DNC confirmation email as legitimate, and on July 1 (day 5), the FTC issued a news release explaining the online registration process. Furthermore, in an unreported estimate of Model A, excluding the first five days, the coefficient of *Internet only* is still negative and significant.

Table 4 Robustness

Variables	(1) MSA	(2) State time trends	(3) Prior cumulative registrations	(4) County-specific diffusion	(5) Different diffusion after day 10	(6) Lagged news reports	(7) Registration rate	(8) Falsification Y: Eastern/Central	(9) Falsification Z: Central/Mountain
Reports of DNC	0.037**	0.044**	0.036**	0.056***	0.033*	0.034*	0.360**	0.038**	0.186***
	(0.018)	(0.018)	(0.017)	(0.014)	(0.017)	(0.017)	(0.173)	(0.018)	(0.062)
Internet only	−0.690***	−0.655***	−0.713***	−0.253***	−0.720***	−0.662***	−4.215***		
	(0.063)	(0.073)	(0.065)	(0.065)	(0.061)	(0.057)	(0.694)		
Internet only × *MSA*	0.128								
	(0.104)								
Cumulative registrations ($t-1$)			−0.107***		−0.090**				
			(0.038)		(0.041)				
After day 10 × *Cumulative registrations* ($t-1$)					−0.115**				
					(0.052)				
Reports of DNC ($t-1$)						0.020*			
						(0.010)			
Reports of DNC ($t-2$)						0.002			
						(0.012)			
Reports of DNC ($t-3$)						0.030***			
						(0.011)			
Placebo								0.027	0.049
								(0.058)	(0.082)
Observations	2,725	2,725	2,725	2,725	2,725	2,725	2,725	2,900	1,900
Adjusted R-squared	0.815	0.817	0.816	0.858	0.817	0.815	0.631	0.848	0.669
Counties	109	109	109	109	109	109	109	116	76
States	10	10	10	10	10	10	10	9	9

Notes. The dependent variable is log DNC registrations per household by county and day, except in Column (7). All specifications include county and day fixed effects. Column (1) shows urban/rural heterogeneous effects, with county classified as urban if within an MSA. Column (2) includes state-time trends. Column (3) includes cumulative DNC registration in counties up to the day before. Column (4) includes county-specific cumulative DNC registration in counties up to the day before (coefficients omitted for brevity). Column (5) includes cumulative DNC registration in counties up to the day before interacted with day 11 and after. Column (6) shows the effect of lagged news reports. In Column (7), the dependent variable is absolute DNC registrations per household (not logarithm), multiplied by 1,000. Column (8) shows Placebo Y, for eastern and central time zones, effective day 11, over 25 days. Column (9) shows Placebo Z, for central and mountain time zones, effective day 11, over 25 days. Robust standard errors clustered by county are in parentheses.

$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

To assess the robustness of our findings, we perform additional estimates with different covariates and specifications as well as several falsification exercises.

Our main analysis focuses on the counties immediately east and west of the Mississippi. Although the design reduces unobserved geographical heterogeneity, it might mask urban–rural differences on the two banks of the river. Our first robustness check allows heterogeneity in effects between urban and rural communities. Table 4, Column (1), reports an estimate including an indicator for counties within a metropolitan statistical area (MSA). The effect of *MSA* itself is correlated with county fixed effects and so cannot be separately identified. The coefficient of the interaction between *Internet only* and *MSA* is not significant, suggesting that the effect of only Internet registration was not significantly different between urban and rural areas.

A typical concern in DID analysis is that the treatment might be confounded by some omitted trending variable. In the DNC context, some states had already established state-level DNC registries, which might affect the trend of federal DNC registration. So, following Angrist and Pischke (2008, §5.2.1), we check robustness by including state time trends. As reported in Table 4, Column (2), the coefficient of *Internet only* is much the same (for brevity, we do not report the coefficients of the state time trends).

The previous estimates did not account for prior registrations. Obviously, the number of residents who can possibly register for DNC at date t would be reduced by the number who had already registered. To some extent, the day fixed effects already account for previous registrations. However, as Figures 3(a) and 3(b) show, the time profile of DNC registrations resembles a diffusion process. We conduct three robustness checks to account for possibly different diffusion processes across counties: (i) including cumulative registrations in the county up to the day before, date $t-1$; (ii) allowing the effect of cumulative registrations to vary across counties; and (iii) allowing the effect of cumulative registrations to differ after day 10. As reported in Table 4, Columns (3)–(5), in all three estimates, the coefficient of *Internet only* is negative and significant.[11]

Next, consumers might respond to past news reports (Goh et al. 2011). To account for the effect of cumulative news reports, we include the weighted number of news reports of the DNC Registry in each of the preceding three days. As reported in Table 4, Column (6), all of the lagged reports had a positive effect on DNC registration, but with varying degrees of significance. This is consistent with previous research showing that the effect of advertising tends to depreciate quickly (Boyd and Seldon 1990). The coefficient of *Internet only*, −0.662 (s.e., 0.057), is almost identical to the baseline estimate, and significant.

All of the estimates so far specify the dependent variable, DNC registrations per household by county and day, in logarithm. We check the robustness of this specification by estimating Model A with the dependent variable in native form (not in logarithm). As reported in Table 4, Column (7), the coefficient of *Internet only*, −4.215 (s.e., 0.694), is negative and precisely estimated. The estimate suggests that in the absence of the toll-free line, DNC registrations were lower by $4.215 \div 1{,}000 \cong 0.004$ per household per day, which is close to the effect implied by the baseline estimate in Table 3, Column (1).

To further check our finding that DNC registration was influenced by Internet-only registration rather than some other unobserved geographical heterogeneity, we conduct two falsification exercises with the borders drawn between counties immediately east and west of the boundaries between the eastern and central and the central and mountain time zones. For brevity of presentation, we label the variables as placebos, with the meaning of the placebo defined in the respective column heading.

As reported in Table 4, Columns (8) and (9), the effects of both placebos are insignificant. Overall, there is a significant and negative effect of *Internet only* on DNC registration only with the treatment defined geographically by the Mississippi River.

Finally, Figure 3 and the estimate in Table 3, Column (2), show that DNC registration increased sharply on day 11, particularly in the eastern counties. These results provide a falsification of "timing"—the effect of toll-free access did not arise before or after the expected date. Together with the two geographical tests reported in Table 4, Columns (8) and (9), the falsification analyses lend support to the causal interpretation of the FTC treatment.

7. Consumer Subsegments

Having shown that the unavailability of registration by toll-free call did affect DNC registrations, we now turn to quantify the subsegments of people who preferred service by telephone. Referring to Figure 4, the subsegments are those who could cope and register online through the Internet (copers), those who knew that toll-free registration would be available from July 7 and delayed (waiters), and those who could not adapt and did not register later (lost consumers).[12]

Referring to §6.1, our estimate in Table 3, Column (4), suggests that some consumers waited for toll-free registration to become available in the eastern counties. If the DNC registration indeed picked up in the eastern counties after day 10 (when toll-free registration opened), then the increase is consistent with the existence of waiters. We use the estimate of Model B to quantify the subsegment of lost consumers.

Specifically, we compute the counterfactual daily registrations in the eastern counties *if the toll-free line had been available*, i.e., the predicted daily registrations with $EAST_i = 0$ (see Footnote 9). Summing over the first 10 days, the counterfactual registrations in the eastern counties are around 0.1189 (s.e., 0.0082) per household.

Then, we compute the differences between the predicted and counterfactual registrations in the eastern counties. The predicted registrations are simply the predicted values from the estimate of Model B in Table 3, Column (2). Summing the differences in the first 10 days, the average registrations were 0.0325 (s.e. 0.0155) *lower* in the East. Similarly, summing the differences for days 11–24, the registrations were 0.0275 (s.e., 0.0018) *higher* in the East.[13] The registration pattern in the East—lower in the first 10 days and higher in the subsequent days—is consistent with some consumers waiting to register.

Comparing our estimates of the loss in eastern registrations between days 1–10 and the gain in eastern registrations from day 11 onward, our estimates suggest the net loss of consumers to be around $0.0325 - 0.0275 = 0.0051$ (s.e., 0.0005) per household and significant ($p < 0.01$). Relative to the counterfactual registration rate in the eastern counties if residents could register through both Internet and telephone in days

[11] The cumulative registration up to date $t-1$ in Table 4, Columns (3)–(5), may correlate with the DNC registrations at other times, which might cause bias in the estimates. However, these are not the main estimates, but rather checks of robustness to alternative diffusion processes. Nevertheless, they should be interpreted with caution. Furthermore, in an unreported estimate of Model A, including cumulative registration up to date $t-1$ and its square, the coefficient of *Internet only* is negative and significant.

[12] We emphasize that the following discussion is based on reduced-form estimates rather than the structural model of equilibrium behavior. Accordingly, our estimates should be interpreted with caution.

[13] To avoid singularity, the estimate excludes $EAST_i \times \tau_{25}$; hence, we can only estimate daily effects up to day 24.

Table 5 County Demographics

	West			East			
Variables	*N*	Mean	Std. dev.	*N*	Mean	Std. dev.	*t*-statistic
% age 65 *& over*	60	13.813	2.891	49	14.637	3.055	−1.422
% with at least a high school education	60	75.923	10.159	49	77.040	9.173	−0.597
Median household income (US$'000)	60	35.115	10.032	49	34.412	8.370	0.396
% unemployed	60	3.699	1.178	49	3.940	1.462	−0.925
Urban county (*MSA*)	60	0.283	0.451	49	0.245	0.430	0.450
% Hispanic	60	1.874	0.278	49	1.421	0.233	1.248
% African American	60	16.504	2.816	49	18.630	3.759	−0.453

Note. All variables are computed by county.

1–10, the percentage of lost consumers is around $0.0051 \div 0.1189 = 4.3\%$.

This estimate of lost consumers is conservative. If the migration of service to the Internet had been compulsory, consumers would not have had the option of waiting to access the conventional channel. Then, some waiters would have coped and registered online, whereas others would not have gotten the service and become lost consumers.

For management and public policy, it is helpful to understand the profile of the waiters and lost consumers. Who are more likely to lose from a compulsory migration to Internet-based services?

We focus on the demographic characteristics shown previously to affect access to and usage of the Internet (Goldfarb and Prince 2008). Table 5 reports demographics from the 2000 U.S. Census. There were no significant demographic differences between the counties immediately east and west of the Mississippi.

Table 6 reports an estimate of Model A including interactions between *Internet only* and the various demographic variables. The coefficient of the interaction between *Internet only* and the percentage of people with at least a high school education is significant and positive. This suggests that the lack of toll-free registration had a *smaller* impact among more highly educated people. Specifically, if the percentage of people with at least a high school education was 1% higher, the DNC registration in counties and at times with only Internet registration would have decreased by 1.436% *less*. One explanation of this result is that, among people who preferred to register by telephone, the better-educated people were more able to switch to online registration.[14]

Finally, we discuss the number of copers. We estimate that the loss in DNC registrations in the first 10 days, relative to the counterfactual registration rate in the eastern counties if residents could register through both the Internet and telephone, to be around $0.0325 \div 0.1189 = 27\%$. Referring to Figure 4, if we know the number of consumers who prefer the telephone in the eastern counties, then we can calculate the number of copers by subtracting the 10-day loss of 27% from those who prefer the telephone.

In 2003, the average Internet penetration rate in the states along the eastern shore of the Mississippi was 59%, so 41% of the people lacked home Internet access (U.S. Census Bureau 2003). If we assume that the number of consumers who prefer the telephone is at least as large as the number of people who lack

Table 6 Daily Registrations: Demographic Variation

Variables	(1) Demographics
Reports of DNC	0.036**
	(0.018)
Internet only	−0.654***
	(0.063)
Internet only × *% age* 65 *& over*	−3.081
	(2.426)
Internet only × *% with at least a high school education*	1.401**
	(0.622)
Internet only × *Median household income*	−0.293
	(0.340)
Internet only × *% unemployed*	−0.085
	(0.055)
Internet only × *Urban county* (*MSA*)	0.004
	(0.144)
Internet only × *% Hispanic*	−0.003
	(0.021)
Internet only × *% African American*	0.007*
	(0.004)
Observations	2,725
Adjusted *R*-squared	0.817
Counties	109
States	10

Notes. The dependent variable is log DNC registrations per household by county and day. County and day fixed effects are included. Column (1) includes interactions of *Internet only* with the demographic variables. Robust standard errors clustered by county are in parentheses.
$^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

[14] The main effects of the demographics are collinear with the county fixed effects and so cannot be separately estimated. We do not include Internet or telephone penetration because these statistics are available only at the state level and so do not provide sufficient variation.

home Internet access, and that, ceteris paribus, households with or without Internet access have an equal propensity to register in the first 10 days, then the percentage of copers among those who had registered in the eastern counties in the first 10 days would be around $41\% - 27\% = 14\%$.

This back-of-the-envelope calculation depends crucially on the two assumptions and also on the accuracy of the Internet penetration rate, which is only available at the state level for the period of the study. To better estimate the number of copers, we need the actual channel—Internet or telephone—used for each DNC registration, or at least the proportion of Internet and telephone registrations, in the first 10 days. Unfortunately, the FTC could not provide such information.

8. Conclusions

Exploiting a natural experiment in the FTC's implementation of the DNC Registry, we find robust evidence that compulsory migration of service to the Internet may result in a loss of customers. In the first 10 days, registrations were 0.0325 per household, or 27% lower in areas without toll-free telephone registration. Once toll-free registration became available, most of the first 10 days' loss of registrations was recovered, but 0.0051 per household or 4.3% were still lost after two weeks. This is a conservative estimate of the number of consumers who were *excluded*. Our findings are bolstered by multiple identification strategies and robust to differences in covariates and specifications and falsification exercises.

Interestingly, the initial loss of registration due to unavailability of telephone registration, 27%, is less than the proportion of consumers lacking home Internet access, 41%. We cannot estimate the number of copers with precision, but it seems that home Internet access is not a good indicator of the loss of consumers due to compulsory migration. This finding suggests that policy makers, managers, and scholars need not be so pessimistic about migrating customer service to the Internet. Focusing on raw statistics of Internet penetration and usage might overstate the impact of the migration.

For public policy and management practice, an important direction for future research is how to manage the segment of consumers who cannot use online service. One choice is simply to cut them off, as the state of Florida did with applications for unemployment benefits. An alternative is to provide conventional access selectively—targeting conventional access to consumers who cannot adapt to Internet service. The challenge is how to implement the self-selection. Lufthansa offers lower fares for online booking (an incentive) and charges a premium for telephone booking in the United Kingdom (a disincentive). The service provider can limit the service capacity of the conventional channel, which would naturally induce a delay in service time, and so encourage consumers to use the online channel.

Another important direction for future research is to estimate the size of and characterize the segment of consumers who can cope with Internet delivery despite preferring the conventional channel and investigate how to steer them to the digital channel. Our empirical results in Table 3, Columns (3) and (4), show that publicity is effective in persuading consumers to use the conventional channel. It is quite plausible that publicity would also raise consumers' satisfaction and customer online efficiency (Xue and Harker 2002), and so persuade them to use the digital channel.

Finally, although our study is based on a natural experiment in 2003, we believe that the policy and managerial implications continue to be valid. In 2003, the proportion of households without home Internet access in the eastern states bordering the Mississippi was 41%. In 2010, the average in the United States was 29% (U.S. Census Bureau 2012). Even with a 25% increase in homes having Internet access over the seven-year period, the proportion of households without home Internet access is still substantial, so the implications from this study should continue to apply. Of course, our findings and implications should apply to public policy and management practice in countries where Internet penetration is lower.

Supplemental Material

Supplemental material to this paper is available at http://dx.doi.org/10.1287/isre.2015.0580.

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