Direct and Spillover Effects of a Paperless Billing Randomized Communication Campaign*

Julian Amendolagine Guillermo Cruces Juan Luis Schiavoni
Dario Tortarolo Gonzalo Vazquez-Bare

October 7, 2020

1 Introduction

This project explores the direct and network effects of communication messages on tax compliance and paperless billing sign-up. We conduct a randomized communication campaign in a large municipality of Argentina where neighbors are required to pay a monthly fee on their real estate, locally known as *Tasa por Servicios Generales* (TSG), which accounts for most of the local own revenues in Argentine municipalities. The municipality where the experiment takes place has recently enabled a paperless billing option for taxpayers to receive their monthly bill by e-mail *in lieu* of the regular paper bill. Our campaign consists of sending letters to randomly selected dwellings where we remind neighbors about the paperless option, how to sign up, and we also include information about the status of the account, due dates, past due debt, etc. Our goal is to study the effects on monthly payments and the sign-up to digital billing, and also to analyze whether the campaign creates spillover effects on neighbors that live nearby but that do not receive a letter. In this Pre-Analysis Plan we describe the administrative data to be used and we explain the experimental design, treatment effect estimation, power calculations, and balance checks.

2 Data Description

We will use administrative data provided by the revenue agency of the municipality where the experiment takes place. The unit of observation is an account (*cuenta*) which coincides with a dwelling unit. The data contains the following variables for the billing details and some individual demographic characteristics of the account holder (*titular*):

- account number (unique ID)
- address of the account
- block number
- odd or even side of the block

*Corresponding author: Guillermo Cruces, E-mail: guillermo.cruces@nottingham.ac.uk

- name of locality (neighborhood)
- year and month of the fee (12 fees per year)
- monthly fee (in pesos)
- paid fee (amount in pesos)
- indicator for monthly fee payment
- indicator for early annual fee payment
- date of payment
- due date of fee
- means of payment
- type of account (residential house, apartment, retail store, factory, etc.)
- indicator for electronic payment method
- days overdue
- indicator for email address
- indicator for an account also appearing in the provincial property registry
- gender of the account holder
- age of the account holder
- linear front meters of the lot/property
- assessed value of the property
- delivery date of the campaign letter

These billing details and payments are available on a monthly basis for current and previous billing cycles.

2.1 Baseline data

For the randomization, power calculation, and simulations, we use baseline data from the year 2019. The data set is restricted to blocks with size between 8 and 50 accounts. See Figure 1 and Table 1.
We make use of three different outcomes:

- `pago_todas`: dummy variable equal to 1 if the account paid the twelve bills in 2019,
- `pago_alguna`: dummy variable equal to 1 if the account paid at least one bill,
- `pago_seis`: dummy variable equal to 1 if the account paid six bills or more.

Table 1 shows some descriptive statistics for the year 2019. Our sample size consists of 68,808 accounts distributed in 3,982 blocks. About 45 percent of the accounts paid the twelve bills and about 35 percent did not pay any bill. We call these two core groups *always payers* and *never payers*, respectively. The perfect compliance rate of 45 percent is presumably low, and therefore leaves space for potential behavioral responses from non-compliant and partially-compliant neighbors.

Figure 1: Distribution of accounts per block

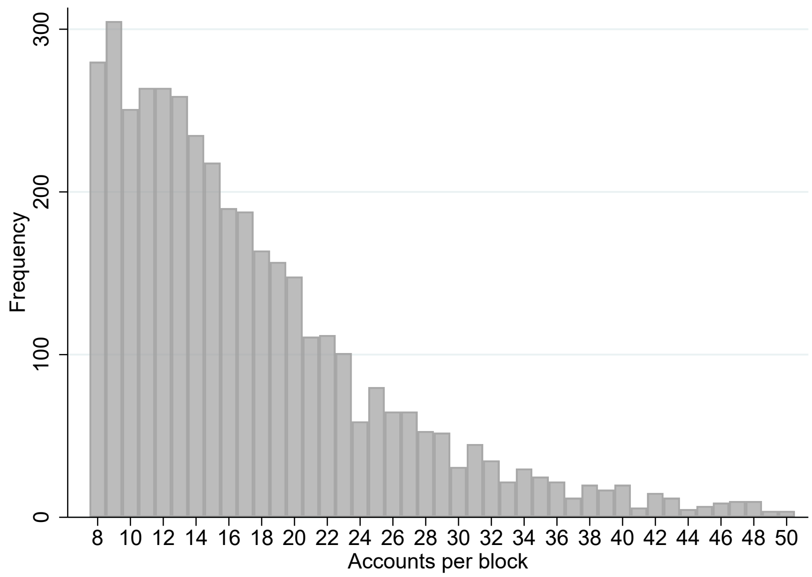


Table 1: Descriptive statistics

	Blocks	Obs	Mean	SD	ICC
`pago_todas`	3982	68808	0.449	0.497	0.062
`pago_alguna`	3982	68808	0.650	0.477	0.071
`pago_seis`	3982	68808	0.572	0.495	0.073

3 Experimental Design

The experiment was run on the universe of residential dwellings present in the municipality in 2020. Randomization took place in two stages (first at the block level and then at the account level). In the first stage, we randomly assigned blocks to four groups with different intensity of treatment: pure control blocks where no accounts are notified, blocks with 20% of the accounts treated, blocks with 50% of the accounts treated, and blocks with 80% of the accounts treated. In the second stage, we randomly selected accounts within the last three groups of blocks to receive the treatment letter.

Approximately 25,000 account holders who are billed monthly were sent a letter containing the treatment messages. The letters were delivered between September 28th and October 7th, 2020, corresponding to payments due in the October billing period of the same year (month 10) as well as past due debt (if any). The outcomes of interest are fee payments (in levels, logs, and an indicator), overdue days, and an indicator for whether neighbors sign up to digital billing. To study the total effect, we measure the change in outcomes among those targeted by the intervention relative to the control group. To study spillover effects, we analyze the behavior of non-targeted neighbors within treated blocks and test for non-linearities based on the differential exposure of blocks to the communication campaign.

A potential third stage of the project would seek to invite neighbors to respond to a brief survey to gauge the take-up of the experiment and also to understand the financial constraints and other behavioral factors that might affect their level of compliance.

3.1 Treatment assignment

Let n_g indicate the number of units (accounts - *cuentas*) per group (block - *cuadra*) with $g = 1, \ldots, G$ and let $N = \sum_g n_g$ be the total sample size. The group-level treatment indicator is denoted by $T_g \in \{0, 1, 2, 3\}$ with distribution $\mathbb{P}[T_g = t] = q_t$ for $t = 0, 1, 2, 3$ where $T_g = 0$ indicates the pure control group, $T_g = 1$ indicates the groups with 20% treated, $T_g = 2$ indicates groups with 50% treated and $T_g = 3$ indicates groups with 80% treated. The unit (account) treatment indicator is $D_{ig} \in \{0, 1\}$. We have that:

$$\mathbb{P}[D_{ig} = 1 | T_g = t] = p_t = \begin{cases} 0 & \text{if } t = 0 \\ 0.2 & \text{if } t = 1 \\ 0.5 & \text{if } t = 2 \\ 0.8 & \text{if } t = 3 \end{cases}.$$

There is a total of seven assignment cells of interest:

$$(D_{ig}, T_g) = \begin{cases} (0, 0) & \text{pure controls} \\ (0, 1) & \text{controls in 20\% groups} \\ (0, 2) & \text{controls in 50\% groups} \\ (0, 3) & \text{controls in 80\% groups} \\ (1, 1) & \text{treated in 20\% groups} \\ (1, 2) & \text{treated in 50\% groups} \\ (1, 3) & \text{treated in 80\% groups} \end{cases}$$

and the probabilities of each of these assignments are:

$$\mathbb{P}[D_{ig} = d, T_g = t] = \begin{cases} q_0 & \text{for pure controls} \\ 0.8 q_1 & \text{for controls in 20\% groups} \\ 0.5 q_2 & \text{for controls in 50\% groups} \\ 0.2 q_3 & \text{for controls in 80\% groups} \\ 0.2 q_1 & \text{for treated in 20\% groups} \\ 0.5 q_2 & \text{for treated in 50\% groups} \\ 0.8 q_3 & \text{for treated in 80\% groups} \end{cases}$$

3.2 Treatment effect parameters and estimators

Given an outcome Y_{ig}, our goal is to estimate:

$$\theta_t = \mathbb{E}[Y_{ig} | D_{ig} = 0, T_g = t] - \mathbb{E}[Y_{ig} | D_{ig} = 0, T_g = 0]$$

for $t = 1, 2, 3$, which can be seen as spillover effects on untreated units in groups with $T_g = t$ compared to pure controls, and

$$\tau_t = \mathbb{E}[Y_{ig}|D_{ig} = 1, T_g = t] - \mathbb{E}[Y_{ig}|D_{ig} = 0, T_g = 0]$$

which are total effects on treated units in groups with $T_g = t$ compared to pure controls. Note that the direct effect of the treatment is not identified in this design because in principle there are no groups in which only one person is treated.

The parameters $\{\theta_t, \tau_t\}_{t=1}^3$ can be estimated jointly through the following saturated regression:

$$Y_{ig} = \alpha + \sum_{t=1}^{3} \theta_t \mathbb{1}(T_g = t)(1 - D_{ig}) + \sum_{t=1}^{3} \tau_t \mathbb{1}(T_g = t) D_{ig} + \varepsilon_{ig}$$

where we allow ε_{ig} to be correlated within blocks and use a cluster-robust variance estimator. Note also that since we will have access to panel data at the account-month level we could run a dynamic difference-in-differences specification comparing Y_{ig} for treatment and control groups in every month m before and after month m^* when the intervention takes place (relative to month $m^* - 1$). We will also explore heterogenous affects for different cuts of the variables listed in Section 2. For example, an exercise of interest consists of exploring non-linearities in the treatment effect depending on the baseline level of compliance of the block. For example, to test whether the effect is stronger or weaker in blocks with a higher share of *always payers* (defined with pre-intervention data).

3.3 Experimental design: choice of q_t

The expected number of treated units / letters sent is

$$N_1 = N(0.2q_1 + 0.5q_2 + 0.8q_3)$$

On the other hand, since the assignments $T_g = 1$ and $T_g = 3$ are symmetric, it makes sense to have $q_1 = q_3$. If the goal is to send L letters, the choice of q_t should satisfy:

$$\begin{aligned} q_0 + q_1 + q_2 + q_3 &= 1 \\ N(0.2q_1 + 0.5q_2 + 0.8q_3) &= L \\ q_1 &= q_3 \end{aligned}$$

Finally, to ensure that the variances of the estimators are similar across assignments, we need:

$$q_2 = Rq_3$$

where R depends on the intraclass correlation and the variance of the potential outcomes. See the appendix for details. We assumed an intraclass correlation between potential outcomes of 0.1 (which is slightly larger than the estimated ICC for the baseline data) and that the variances of the potential outcomes are approximately equal (see the appendix for details).

Using the sample sizes from the baseline data and setting $L = 25,000$ gives the probabilities shown in Table 2. Table 3 shows the expected (approximate) sample sizes.

Table 2: Assignment probabilities

	Prob
q_0	0.273
q_1	0.282
q_2	0.162
q_3	0.282

Table 3: Approximate sample sizes

	Blocks	Control Obs	Treated Obs
$T_g = 0$	1088	18808	0
$T_g = 1$	1124	15547	3886
$T_g = 2$	644	5565	5565
$T_g = 3$	1124	3886	15547
Total	3980	43806	24998

4 Power and minimum detectable effects

Figure 2 plots the power function for each estimator, using the following parameters:

- $\sigma^2(d, t) = 0.25$ for all (d, t). This gives a conservative estimate because 0.25 is the upper bound for the variance of a binary variable.
- $\mathsf{ICC} = 0.1$ which is close to (but larger than) the estimated intraclass correlation of the baseline outcome.
- The sample and group sizes given by the baseline data.

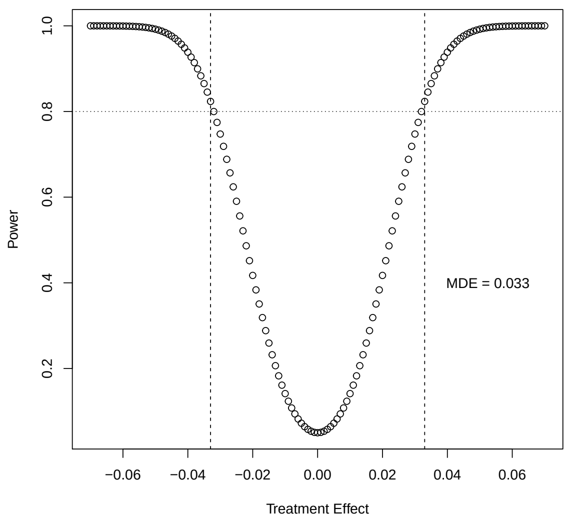
See the appendix for details on the power function formula. The power calculations give a minimum detectable effect between 2.6 and 3.3 percentage points.

5 Simulations

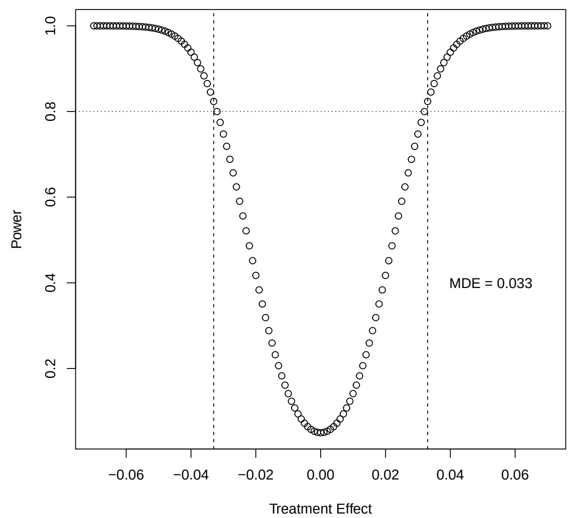
In each simulation, we use the baseline outcome from June 2019 as the potential outcome for pure controls, and construct the remaining potential outcomes adding the corresponding direct or spillover effects. See the appendix for details. The results are shown in Table 4. The last parameter is set to zero to simulate the probability of type I error.

The simulation results are in line with the ones from the analytical calculations in the previous section, with slightly lower MDEs because some statistics such as the ICC are in fact lower in the sample. The last row in the table confirms that the probability of incorrectly rejecting the null of no effect is around 5% as expected.

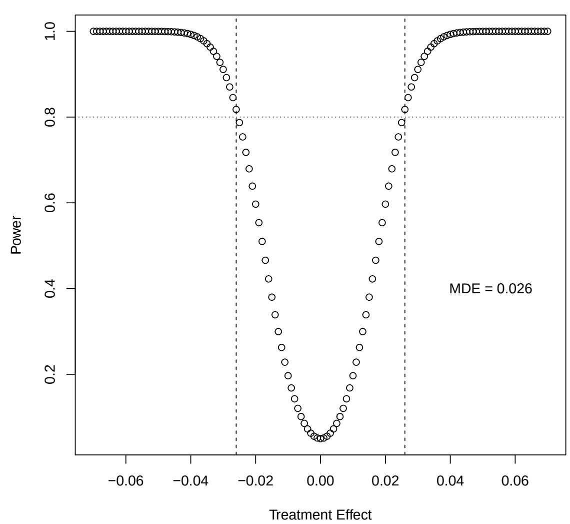
Figure 2: Power functions



(a) $(d,t)=(0,3)$ or $(d,t)=(1,1)$



(b) $(d,t)=(0,2)$ or $(d,t)=(1,2)$



(c) $(d,t)=(1,3)$ or $(d,t)=(0,1)$

Table 4: Simulation results

	True value	Prob(reject)
θ_1	0.021	0.812
θ_2	0.026	0.798
θ_3	0.027	0.791
τ_1	0.028	0.801
τ_2	0.026	0.800
τ_3	0.000	0.045

6 Balance Checks

We will run balance checks verifying comparability of the treatment and the control group in terms of demographic and account-related characteristics in 2019. We will run Ordinary Least Squares regressions

$$X_{ig} = \alpha + \beta D_{ig} + \epsilon_{ig}$$

where X_{ig} is one of the account holder or dwelling characteristics listed in Section 2 and D_{ig} is a dummy variable equal to 1 if the taxpayer was assigned to a group that received the communication letter, and equal to 0 otherwise. We will allow ϵ_{ig} to be correlated within blocks and use cluster-robust standard errors. Note that the balance should also hold when we run OLS regressions pooling the accounts from the different groups of blocks and comparing treatment and control.

Appendices

A Treatment assignment

For the simulations I assume $(T_1, T_2, \dots, T_G)$ are iid with distribution: $\mathbb{P}[T_g = t] = q_t$ and the variable is constructed as:

$$T_g = \mathbb{1}(q_0 < U_g \leq q0 + q_1) + 2\mathbb{1}(q_0 + q_1 < U_g \leq q_0 + q_1 + q_2) + 3\mathbb{1}(U_g > q_0 + q_1 + q_2)$$

with $U_g \sim \mathsf{Uniform}(0, 1)$.

The individual treatment indicator is assigned according to the rule:

$$D_{ig} = \mathbb{1}(U^1_{ig} \leq 0.2)\mathbb{1}(T_g = 1) + \mathbb{1}(U^2_{ig} \leq 0.5)\mathbb{1}(T_g = 2) + \mathbb{1}(U^3_{ig} \leq 0.8)\mathbb{1}(T_g = 3)$$

where $U^k_{ig} \sim \mathsf{Uniform}(0, 1)$ for $k = 1, 2, 3$, independent of each other.

B Formulas and derivations for power calculations

Estimators for each assignment (d, t) with $t \neq 0$ are defined as:

$$\hat{\mu}(d, t) - \hat{\mu}(0, 0) = \frac{\sum_{g=1}^{G} \sum_{i=1}^{n_g} Y_{ig}\mathbb{1}_{ig}(d, t)}{\sum_{g=1}^{G} \sum_{i=1}^{n_g} \mathbb{1}_{ig}(d, t)} - \frac{\sum_{g=1}^{G} \sum_{i=1}^{n_g} Y_{ig}\mathbb{1}_{ig}(0, 0)}{\sum_{g=1}^{G} \sum_{i=1}^{n_g} \mathbb{1}_{ig}(0, 0)}$$

where $\mathbb{1}_{ig}(d, t) = \mathbb{1}(D_{ig} = d, T_g = t)$. The variance of this estimator can be approximated by:

$$\begin{aligned} V(d, t) \approx{}& \frac{\sigma^2(d, t)}{G\bar{n}\pi(d, t)} \left\{1 + \rho_{dt,dt} \frac{\pi((d, t), (d, t))}{\pi(d, t)} \left(\frac{\bar{n}_2}{\bar{n}} - 1\right)\right\} \\ &+ \frac{\sigma^2(0, 0)}{G\bar{n}\pi(0, 0)} \left\{1 + \rho_{00,00} \frac{\pi((0, 0), (0, 0))}{\pi(0, 0)} \left(\frac{\bar{n}_2}{\bar{n}} - 1\right)\right\} \end{aligned}$$

where $\bar{n} = \sum_{g=1}^{G} n_g/G$, $\bar{n}_2 = \sum_{g=1}^{G} n_g^2/G$, $\pi(d, t) = \mathbb{P}[D_{ig} = d, T_g = t]$, $\pi((d, t), (d, t)) = \mathbb{P}[D_{ig} = d, D_{jg} = d, T_g = t]$ and $\rho_{dt,dt}$ is the intraclass correlation between potential outcomes, $\mathsf{cor}(Y_{ig}(d, t), Y_{jg}(d, t))$. In this case we have that:

$$\begin{aligned} \pi(d, t) &= \mathbb{P}[D_{ig} = d, T_g = t] = \mathbb{P}[D_{ig} = d|T_g = t]\mathbb{P}[T_g = t] = p_t^d(1 - p_t)^{1-d}q_t \\ \pi(0, 0) &= q_0 \\ \pi((d, t), (d, t)) &= \mathbb{P}[D_{ig} = d, D_{jg} = d|T_g = t]\mathbb{P}[T_g = t] = p_t^{2d}(1 - p_t)^{2(1-d)}q_t \\ \pi((0, 0), (0, 0)) &= q_0 \end{aligned}$$

and thus

$$\frac{\pi((d,t),(d,t))}{\pi(d,t)} = p_t^d(1-p_t)^{1-d}$$
$$\frac{\pi((0,0),(0,0))}{\pi(0,0)} = 1$$

The variance formula is then:

$$V(d,t) \approx \frac{\sigma^2(d,t)}{G\bar{n}p_t^d(1-p_t)^{1-d}q_t}\left\{1+\rho_{dt,dt}p_t^d(1-p_t)^{1-d}\left(\frac{\bar{n}_2}{\bar{n}}-1\right)\right\}$$
$$+\frac{\sigma^2(0,0)}{G\bar{n}q_0}\left\{1+\rho_{00,00}\left(\frac{\bar{n}_2}{\bar{n}}-1\right)\right\}.$$

Given this variance, the power function is given by:

$$\beta(\theta) = 1-\Phi\left(\frac{\theta}{\sqrt{V(d,t)}}+z_{1-\alpha/2}\right)+\Phi\left(\frac{\theta}{\sqrt{V(d,t)}}-z_{1-\alpha/2}\right).$$

B.1 Choice of q_t

The "hardest" effect to estimate correspond to the assignments $(d,t)=(1,1)$, i.e. treated in 20% groups, and $(d,t)=(0,3)$, i.e. controls in 80% groups. To ensure the variance of these estimators is similar to the variance of the $(d,t)=(0,2)$ estimator, and using that $q_1=q_3$, we need:

$$\frac{\sigma^2(0,3)}{0.2q_3}\left\{1+0.2\rho_{03,03}\left(\frac{\bar{n}_2}{\bar{n}}-1\right)\right\} = \frac{\sigma^2(0,2)}{0.5q_2}\left\{1+0.5\rho_{02,02}\left(\frac{\bar{n}_2}{\bar{n}}-1\right)\right\}.$$

We will assume that all the variances are the same, $\sigma^2(0,3)\approx\sigma^2(0,2)=\sigma^2$ and that all the intraclass correlations are the same and equal to 0.1, which is larger than the one estimated for the baseline data. Then we have that after some simplifications:

$$q_2\left\{1+0.02\left(\frac{\bar{n}_2}{\bar{n}}-1\right)\right\} = 0.4q_3\left\{1+0.05\left(\frac{\bar{n}_2}{\bar{n}}-1\right)\right\}.$$

C DGP for simulations

The simulations rely on seven potential outcomes $Y_{ig}(d,t)$ for $d=0,1$ and $t=0,1,2,3$. Based on the baseline June 2019 outcome Y_{ig}^{base}, the potential outcomes are constructed in the following way:

$$Y_{ig}(0,0) = Y_{ig}^{base}$$
$$Y_{ig}(d,t) = \mathbb{1}(U_{dt}\le c_{dt})(1-Y_{ig}(0,0))+\mathbb{1}(\tilde{U}_{dt}\le c_{dt}+k)Y_{ig}(0,0)$$

for $(d,t) \neq (0,0)$, where U_{dt} and $\tilde{U}_{dt}$ are independent uniforms. According to this model,

$$\begin{aligned}\mathbb{E}[Y_{ig}(0,0)] &= \mu_0 \\ \mathbb{E}[Y_{ig}(d,t)] &= c_{dt} + \mu_0 k \\ \mathbb{C}\mathrm{ov}(Y_{ig}(0,0), Y_{ig}(d,t)) &= k\mu_0(1-\mu_0)\end{aligned}$$

Therefore, we can set:

$$c_{0t} = \theta_t + \mu_0(1-k), \quad c_{1t} = \tau_t + \mu_0(1-k)$$

and

$$k = \frac{\rho}{\mu_0(1-\mu_0)}$$

where ρ is some specified level for the covariance.

C.1 Model parameters

$$\begin{aligned}\mu_0 &= \bar{Y}^{base} \approx 0.568 \\ \rho &= 0.2\end{aligned}$$

A value of $\rho = 0.2$ implies a correlation between $Y_{ig}(0,0)$ and $Y_{ig}(d,t)$ between 0.6 and 0.8. The implied intraclass correlation for all potential outcomes is approximately $\mathsf{ICC} = 0.05$.

D Subgroup analysis and stratification on group size

For the subgroup analysis, we divide the blocks into three categories:

- Small: group size below 15,
- Medium: group size between 16 and 25,
- Large: group size larger than 25.

Table 5 shows descriptive statistics for the outcomes of interest in each group size category. Table 6 shows that the assignment probabilities for each subgroup are very similar. The sample sizes in each subgroup are shown in Table 7.

Finally, Figure 3 plots the power functions for the three group size categories and for the assignment $(0,3)$. Due to the smaller sample sizes, the MDEs are larger.

Table 5: Descriptive statistics

	Blocks	Obs	Mean	SD	ICC
Small					
pago_todas	2076	23494	0.418	0.493	0.070
pago_alguna	2076	23494	0.618	0.486	0.079
pago_seis	2076	23494	0.539	0.499	0.081
Medium					
pago_todas	1310	25665	0.441	0.496	0.060
pago_alguna	1310	25665	0.641	0.480	0.065
pago_seis	1310	25665	0.562	0.496	0.068
Large					
pago_todas	596	19649	0.497	0.500	0.046
pago_alguna	596	19649	0.700	0.458	0.049
pago_seis	596	19649	0.623	0.485	0.051

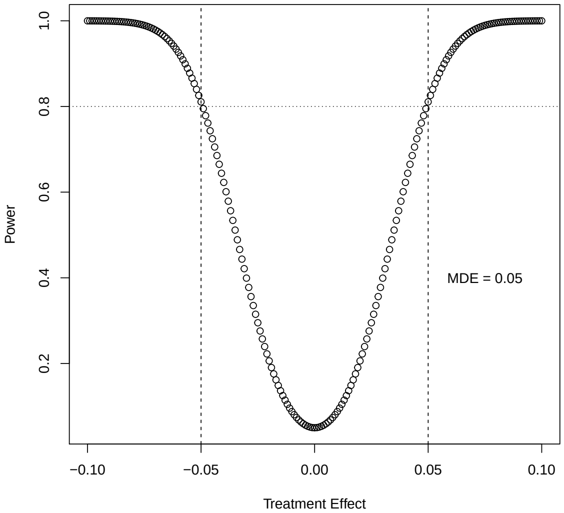
Table 6: Assignment probabilities

	Small	Medium	Large
q_0	0.273	0.273	0.273
q_1	0.282	0.282	0.282
q_2	0.162	0.162	0.162
q_3	0.282	0.282	0.282

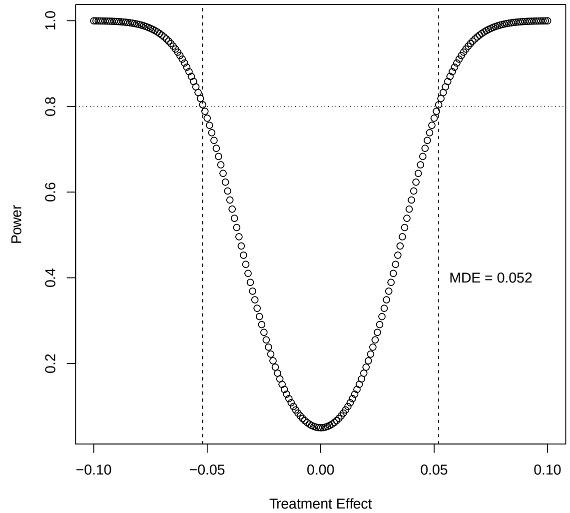
Table 7: Sample sizes

	Blocks	Control Obs	Treated Obs
Small			
$T_g = 0$	567	6421	0
$T_g = 1$	586	5308	1327
$T_g = 2$	335	1900	1900
$T_g = 3$	586	1327	5308
Total	2074	14956	8535
Medium			
$T_g = 0$	358	7015	0
$T_g = 1$	370	5799	1449
$T_g = 2$	211	2075	2075
$T_g = 3$	370	1449	5799
Total	1309	16338	9323
Large			
$T_g = 0$	162	5370	0
$T_g = 1$	168	4439	1109
$T_g = 2$	96	1589	1589
$T_g = 3$	168	1109	4439
Total	594	12507	7137

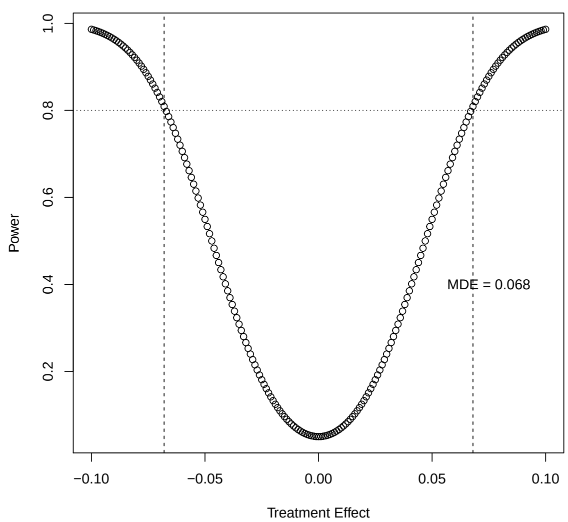
Figure 3: Power functions



(a) $(d, t) = (0, 3)$, small groups



(b) $(d, t) = (0, 3)$, medium groups



(c) $(d, t) = (0, 3)$, large groups