What Happens When Transaction Costs Go Down?
Evidence from Return Boxes

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Abstract

The present paper tries to answer the following question: what happens when transaction costs go down in a common-pool resource? Employing a novel dataset related to more than 20,000 transactions in distinct libraries during a five-year period (2011/2015), I exploit variation in the timing of introduction of a cost-saving technology (return boxes) and its impacts on library performance measures. Contrarily to standard arguments based on transaction costs, I find a result in which the instauration of return boxes tend, on average, to raise the probability of delays and borrowings' effective durations.

Keywords: behavioral economics; common-pool resources; transaction costs.

1. INTRODUCTION

At least since Coase's (1937) seminal contribution, academics and policy makers incorporated transaction costs as an important ingredient in their analyses. These costs, defined as the ". . . cost of using the price mechanism" (Coase, 1937, p. 390) or ". . . the costs of running the economic system" (Arrow, 1969, p. 59), have played a fundamental role in several areas of knowledge, such as accounting, business strategy, economics, marketing, and law, just to cite a few (Macher & Richman, 2008). In theoretical terms, transaction costs constitute a major element in explanations related to vertical integration (Klein, Crawford, & Alchian, 1978; Williamson, 1985, 1996). In empirical terms, the literature on transaction costs' measurement is currently recognized as a "success story", since many studies attested the importance of such costs by employing alternative methods and proxies to a variety of settings and time periods (Macher & Richman, 2008; Masten, 1996; Ruester, 2010; Williamson, 2000)[i].

Despite all the progress made on the last decades, few studies evaluated the impacts of transaction costs on common-pool resources in a field setting. The present paper takes an alternative route to understand the effects of transaction costs in this kind of setting. In particular, I try to answer the following question: what happens when transaction costs go down in a common-pool resource? I investigate the importance

of transaction costs in a specific type of common-pool resource, by exploiting variation in the timing of introduction of a cost-saving technology. I consider such an introduction a *proxy* for lower transaction costs, since return boxes correspond to a practical and faster way for returning items in libraries, saving time for both library users and staff.

According to Huck and Rasul (2010, p. 1), transactions costs may ". . . be related to the time costs of decision making". I follow a similar approach in this paper and propose that the introduction of return boxes may reduce transaction costs by lowering users' time costs when returning specific items to the library. Users could save time by directly returning items through the boxes, instead of going to the library's front desk. Alternatively, users could save time by not going to the library, which can be located, several floors above university entrance, for instance. Given these possibilities, I hypothesize that return boxes, by reducing transaction costs, would enhance performance measures in the libraries that decide to adopt them. For example, one could expect that the number of delayed items would reduce after the introduction of a return box. Alternatively, one could expect a rise in the number of early devolutions, since users would have more opportunities to return items they borrowed from the library.

I exploit variation in the introduction of a cost-saving technology (return boxes) in different libraries, located at distinct *campi* of the same university. Before 2012, users had to return books in person in each library. After that year, one library introduced a return box in the university campus where it operated, while another library did the same one year later, in 2013. This unique feature of the data allows me to employ a difference-in-differences research design to evaluate the effects of the policy implemented in each library. If lower transaction costs were relevant in this setting, then one would expect to find a significant effect of such costs on library users' performance measures, as predicted by standard theories of vertical integration, for instance. Contrarily to the previous rationale, I uncon a result in which the instauration of return boxes either increases some measures, such as borrowings' effective duration and users' delays, at the same time that it does not exert any significant effect on item counts. These results have important implications for theories based on transaction costs, with a special emphasis on common-pool resources.

The remainder of the paper proceeds as follows. Section 2 contains a selective description of the related literature, as well as its relation to the present paper. Section 3 describes the data and research design employed in the empirical analysis below. Section 4 contains the analysis' main empirical results, while section 5 reports sensitivity analysis tests. Finally, section 6 concludes.

2. RELATED LITERATURE

This paper dialogues with distinct literatures. First, the results reported in the paper represent a new way to watch the importance of transaction costs in a field setting, when compared to a long tradition in the literature focused on vertical integration issues, only (Klein, 1990; Klein et al., 1978; Williamson, 1985, 1991)[ii]. For example, in the case of previous contributions related to vertical integration, it may be hard to disentangle firms' decisions based either on transaction costs or on other features of the data, such as contracts' incompleteness. One advantage of the present setting is that I am able to isolate the effects of lower transaction costs on behavior in a field setting. A related point is that, while most of the empirical literature focused on the consequences of such costs for vertical integration processes in firms (Joskow, 1987; Parmigiani, 2007; Poppo & Zenger, 1998, 2002), I present an analysis based on their effects on users' behavior in an information commons, a library. To the best of my knowledge, this is one of the first papers to relate transaction costs to an information commons in a field setting[iii].

Second, and related to the latter point, the results in this paper add to a well-established literature in social dilemmas, with a special emphasis on common-pool resources' management (Demsetz, 1967; Hardin, 1968; Olson, 1965; Ostrom, 1990, 1999, 2010). While there exists a large volume of evidence related to social dilemmas in artificial settings – such as laboratory experiments (Andreoni, 1988; Fehr & Gächter, 2000; J. J. Murphy & Cárdenas, 2004) – the present paper reports results related to a social dilemma in a unique field setting, an information commons. This setting has the advantage of not needing external interventions from the researcher, as well as not being subject to "demand effects", a common problem reported in the experimental literature (Al-Ubaydli, List, & Suskind, 2017; Fréchette, 2015; Kagel, 2009). Although there were previous research efforts related to common-pool resources' management in field settings, most of them focus on studying environmental themes such as forests, fisheries, and wildlife, in general (Cárdenas & Ostrom, 2004; Fehr & Leibbrandt, 2011; Rustagi, Engel, & Kosfeld, 2010). This

paper differs from previous contributions by expanding the scope of analysis and focusing on a specific type of common-pool resource, an information commons (Hess & Ostrom, 2007).

Third, this paper dialogues with other papers presenting the results of field experiments involving transaction costs. I specifically refer to the contributions by Funk (2007, 2010) and Huck and Rasul (2010), in which the authors test the importance of lower transaction costs in distinct field settings. In the first paper, when evaluating the effects of the instauration of a cost-saving technology (postal voting) on voter turnout, the author cannot find significant effects of such an instauration. On the other hand, in the second paper, the authors find significant effects of lower transaction costs on fundraising campaigns in Germany. The present paper differs from these contributions by empirically exploring the impacts of transaction costs in a common-pool resource. Similarly to Funk (2010), this paper contributes to a new body of evidence which questions the importance of transaction costs in specific settings[iv].

Finally, the results described here also add to the growing evidence related to processes of institutional change in distinct settings. I see the introduction of a cost-saving technology as a change in the "rules of the game", as originally proposed by North (1990, 1991)[v]. In this sense, the present paper contributes to the understanding of institutional change in a very specific setting (Ostrom, 2007). As a consequence, the results here reported add to a well-established literature related to the impacts of institutional change at distinct levels of aggregation, both in the short and long run (Aoki, 2007; Greif, 1998; Greif & Laitin, 2004; North, 1990, 1994)[vi].

3. DATA AND METHOD

3.1. Institutional Background and Data

I have access to confidential daily data related to library users of a private university in São Paulo, Brazil, for the 2011-2015 period. This rich dataset corresponds to the detailed transactions of distinct libraries located in different university *campi* of the same university. The data contain detailed information on 1,950 individual users, covering more than 20,000 daily transactions for the entire sample period. This

corresponds to an unbalanced panel, since each library user may borrow different numbers of specific library items at distinct moments.

The data contain information on users' socioeconomic characteristics – such as gender, date of birth, and address – as well as library's confidential information, with each user's identification number, university category (high school, undergraduate, master's, MBA, former student, professor, and employee) and area of study (management, accounting, economics, international relations, advertising, and secretariat). For each user in the data, I am able to identify her department and category. The data also contain the dates when each user borrows specific items from the library, as well as each item's code, and title. Based on each title, I am able to build a measure of area of expertise for each book in the sample, such as management, accounting, economics, and law.

One important information regarding the libraries studied in this paper relates to their location and size. As stated above, these libraries belong to different *campi* of the same university, two located in central neighborhoods in São Paulo (Liberdade and Largo do São Francisco), and one located in an upper-class neighborhood (Pinheiros). The Liberdade unit is the oldest and largest library of the three: founded in 1902, it contained 31,193 books in the 2015 year. In the case of the Largo do São Francisco (San Fran) unit, it dates from 2006, containing 2,883 books, in 2015. Finally, the Pinheiros unit dates back to 2011, having 883 books. Although these different locations may affect user behavior in each library, it is worth noting that two of these libraries (Largo do São Francisco and Pinheiros) serve the same type of user, MBA students, mainly. Because of this feature, I only consider MBA students in the estimations below. In doing so, I want to make both treatment and control groups more comparable, and to respect the identification condition of a difference-in-differences research design, namely parallel trends[vii].

Libraries also differ in terms of dates of instauration of the cost-saving technology studied in this paper. In the beginning of the 2012 academic year, two libraries (Liberdade and Pinheiros) introduced return boxes in their facilities. At first, the Liberdade unit introduced two return boxes, while the Pinheiros unit introduced one return box, only. One year later, the university decided to relocate the box from Liberdade to the Pinheiros unit. This institutional setting provides me with the opportunity to compare distinct situations involving reduced transaction costs: first, by comparing similar libraries (Largo do São

Francisco and Pinheiros), which received treatment at different points in time; second, by comparing distinct libraries (Liberdade and Largo do São Francisco), which differ in their treatment intensities (two return boxes *versus* one box). I follow this strategy in the robustness section by comparing distinct pairs of libraries on time, after the instauration of their respective return boxes[viii].

I also have access to the library's official yearly reports. These reports contain rich institutional information related to the library's internal workings on the 2005-2015 period. Based on this information, I am able to build predicted devolution dates for each user in the sample. Each user can renew books after the predicted devolution date expires, conditional on a waiting list managed by library staff. Although I do not have access to information on such lists' content, I can observe when users renew library items by comparing the dates of borrowings of the same item on time. This information allows me to build additional performance measures for each user in the sample, such as renew rates, the number of items that each user borrows every time she goes to the library, as well as measures of delays on time (equal to the difference between predicted and effective devolution dates for each item borrowed). Finally, I build measures of early returns (in the case of users who return books before the predicted date), and books' usage (equal to the number of times that users pick a specific book). I complement the data with academic calendar information related to exam weeks occurred in the university on time, as well as holydays, vacations, and weekends.

3.2. Empirical Strategy

A unique feature of the libraries studied in this paper is the fact that one of them introduced a return box in its daily operations in the beginning of 2012 (Largo do São Francisco), while the other did the same one year later (Pinheiros). By estimating econometric specifications in which I control for users' observed characteristics, I am able to compare the behavioral responses of library users who had access to the boxes (treatment group) to users who did not (control group). This institutional feature allows me to employ a difference-in-differences research design to test the main hypothesis I pose in the paper. This hypothesis is the following:

> H_1: *the introduction of a return box in a library, by lowering transaction costs, would improve user performance measures. Specifically, such an introduction would lower delays, and borrowings' effective durations, at the same time that it would raise early returns and item counts*.

To test such a hypothesis, I run different versions of the following econometric specification:

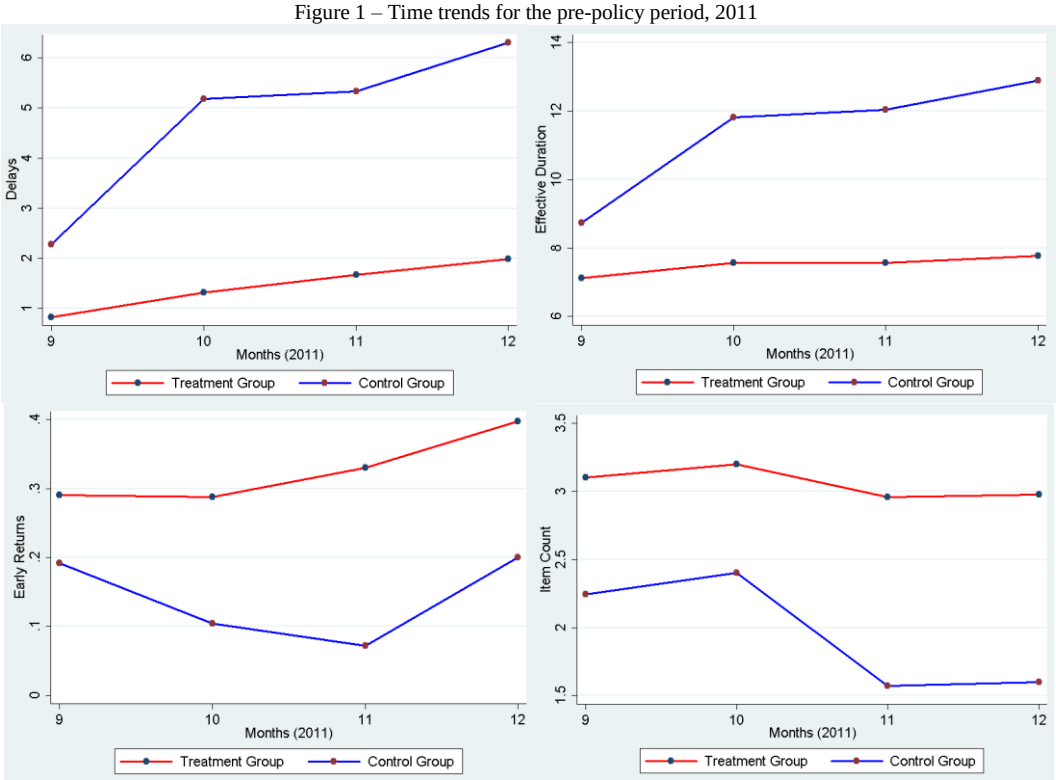
(4.1) $Y_{ist} = \alpha + \gamma(\text{Treatment}) + \lambda(\text{Post-Policy}) + \beta(\text{Treatment } x \text{ Post-Policy}) + \Sigma_t(\psi_t) + \varepsilon_{ist}$

Here, Y_{ist} represents potential outcomes for individual *i* (equal to 0 or 1), in library *s* (treatment or control), at time *t*. In most specifications below, Y_{1ist} corresponds to the proportion of delays by user *i* and period *t*, after the introduction of a box in the library, while Y_{0ist} corresponds to the same proportion before such an introduction. The term "*Treatment*" corresponds to an indicator variable, which assumes unity value in the case of the library that received a box, and 0, otherwise. The term "*Post-Policy*" corresponds to an indicator variable that assumes unity value for the period posterior to 1/1/2012, and 0, otherwise. I also include academic year, book, and user fixed-effects as controls in the regressions below (captured by the term α), as well as time effects, such as days of the week, weeks in the year, and specific years, in some specifications (ψ_t). In the case of the term ε_{ist}, it has a conditional mean of zero ($E(\varepsilon_{ist} \mid s, t) = 0$). The parameter of interest, representing a causal estimate in this context, is β, which corresponds to a difference-in-differences estimate.

4. RESULTS

The main empirical challenge in the present setting is to find an appropriate counterfactual, that is, a control group that would present behaviors consistent with the behavior of the treatment group, given the absence of the treatment. Specifically, one important but untestable hypothesis in a difference-in-differences research design is that both control and treatment groups follow parallel trends on time. Figure 1 presents time trends for distinct library performance measures during the pre-treatment period:

Figure 1 – Time trends for the pre-policy period, 2011



Source: author's calculations, based on library data.

In the figure, I plot the means of each library performance measure, for the period between the months of September and December 2011[ix]. For both groups (treatment and control), I only consider the behavior of MBA students, by excluding other user categories from the sample. In terms of library performance measures, I consider borrowings' delays (upper left graph), borrowings' effective duration (upper right), early returns (lower left), and item count (lower right). Since I employ these measures as dependent variables in the difference-in-differences specifications below, I check the visual adequacy of each of them, in terms of the parallel trends assumption. A first look at the graphs suggest that all variables conform to such a condition. Table 1 presents summary statistics for selected variables for both groups (treatment and control) in the pre-policy period:

Table 1 – Descriptive statistics – pre-policy period (2011)

VARIABLE	Control Group	Treatment Group	Total Sample
Age	32.70	32.70	32.70
	(7.29)	(7.35)	(7.35)
Female	0.72	0.60	0.60
	(0.45)	(0.49)	(0.49)
Scholarship	0.18	0.29	0.29
	(0.38)	(0.45)	(0.45)
Business Book	0.55	0.39	0.40
	(0.50)	(0.49)	(0.49)
Accounting Book	0.24	0.25	0.25
	(0.43)	(0.43)	(0.43)
Economics Book	0.08	0.04	0.04
	(0.27)	(0.19)	(0.19)
Law Book	0.01	0.08	0.08
	(0.07)	(0.28)	(0.27)
Management	0.45	0.27	0.27
	(0.50)	(0.44)	(0.45)
Accounting	0.54	0.73	0.72
	(0.50)	(0.44)	(0.45)
Observations	192	4,268	4,460

Source: author's calculations, based on library data.
Notes: (a) Standard deviations reported in parentheses.

One important feature of the data in the table relates to the imbalance between treatment and control groups, in this case. Specifically, when looking at the control group (Pinheiros), one notices that it contains far less observations (192) than the treatment group (Largo do São Francisco, with 4,268 observations). This imbalance between both groups translates in a total sample that directly reflects characteristics from the treatment group. I try to circumvent this problem in two different ways: first, I employ matching estimators to obtain a better balance between the two groups; second, I consider variation in the control and treatment groups to evaluate the robustness of main results. I discuss these alternative procedures in the robustness section below.

In table 2, I present the results of difference-in-differences estimations for the 2011-2012 period. In the table, the dependent variable corresponds to the proportion of delays in the period, that is, to the number of times that each user in the sample delays returning items to the library, given her total number of borrowings (named "Prob.(Late)"). This variable corresponds to a dummy variable that assumes unity value every time a user presents a positive value for her delays. I run this first specification to evaluate "extensive margin" effects of the introduction of return boxes on user behavior in the libraries. In the table's second to fifth columns, I progressively add covariates to the specifications in the table to control for fixed-effects that may bias the resulting estimates, a common practice in difference-in-differences

studies. I also consider alternative ways to control for the existence of distinct trends in the treatment and control groups: in the table's fifth column, I follow Besley and Burgess (2004) by including library specific time trends, while in the sixth column, I consider distinct weekly time trends for each group. Finally, I follow Bertrand, Duflo, and Mullainathan (2004), and cluster standard errors by the number of courses offered at the university[x].

Table 2 – Effects of return boxes on probability of delays

VARIABLES	(1) Prob.(Late)	(2) Prob.(Late)	(3) Prob.(Late)	(4) Prob.(Late)	(5) Prob.(Late)	(6) Prob.(Late)
DiD Coefficient	0.33***	0.33***	0.32***	0.33***	0.36***	0.36***
	(0.057)	(0.055)	(0.054)	(0.051)	(0.048)	(0.059)
Acad. Year Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Book Fixed Effects	No	No	Yes	Yes	Yes	Yes
User Fixed Effects	No	No	No	Yes	Yes	Yes
Time Trends	No	No	No	No	Yes	Yes
Mean Dep. Variable	0.53	0.53	0.53	0.53	0.53	0.53
Observations	12,993	12,993	12,993	12,993	12,993	12,993
Adj. R-squared	0.059	0.095	0.095	0.098	0.098	0.100

Source: author's calculations, based on library data.
Notes: (a) The dependent variable in the specifications corresponds to the probability of delays in the library. (b) Standard errors clustered by course (reported in parentheses). (c) "Acad. Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2012-year. (d) "Book Fixed Effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (e) "User Fixed Effects" correspond to a set of dummies for users' group ages (24-30, 31-40, 41-50, 51-60, 60+), gender (female), area of study (business and accounting), and time at school (0 to 4 years). (f) Sample Period: 2011-2012. (g) Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The table's first column corresponds to an econometric specification for equation (1) with no controls. In the table's second column, I add dummies for each week in the year, days of the week, and year. I do this to capture academic year fixed effects. In the third column, I add book dummies (accounting, management, economics, and law books) to capture differences in terms of specific items borrowed by library users. In the fourth column, I add a rich set of user-related covariates to capture users' fixed effects: their gender, age group, area of study, and time at school. In the fifth and sixth columns, I repeat the specification in the fourth column, but I consider distinct types of time trends.

One main result emerges from the table: the introduction of return boxes tends, on average, to raise the probability of delays among library users. Although there are differences in terms of the adequacy of each specification (given by the values of the coefficient of determination, R^2), I find a positive effect of the introduction of return boxes on delays. Specifically, such an introduction rises the probability of delays by 62%, approximately (= 0.33/0.53). This result contradicts the previous rationale based on the main hypothesis that I want to test in the present setting, i.e., that return boxes, by lowering transaction costs,

would improve library performance measures. If anything, boxes tend to raise delays in the period after its instauration.

In table 3, I present difference-in-differences estimates in which I substitute the dependent variable with alternative measures of library users' performance. These variables are the following: borrowings' effective duration, borrowings' delays, the proportion of early devolutions (named "Early Returns"), and the number of items that each user borrows every time she goes to the library ("Item Count"). In the case of each specification, I include a full set of covariates to capture distinct types of fixed effects, as well as variable time trends, in the spirit of Besley and Burgess (2004):

Table 3 – Effects of return boxes on library performance measures

VARIABLES	(1) Effective Duration	(2) Delays	(3) Early Returns	(4) Item Count
DiD Coefficient	3.48***	3.21***	-0.16***	-0.58
	(0.381)	(0.243)	(0.059)	(0.359)
Acad. Year Fixed Effects	Yes	Yes	Yes	Yes
Book Fixed Effects	Yes	Yes	Yes	Yes
User Fixed Effects	Yes	Yes	Yes	Yes
Time Trends	Yes	Yes	Yes	Yes
Mean Dep. Variable	8.50	2.27	0.24	2.97
Observations	12,993	12,993	12,993	12,993
Adj. R-squared	0.072	0.081	0.039	0.056

Source: author's calculations, based on library data.

Notes: (a) The dependent variable in the specifications corresponds to borrowings' effective duration (first column), borrowings' delays (second column), early returns (third column), and item count (fourth column). (b) Standard errors clustered by course (reported in parentheses). (c) "Acad. Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2012-year. (d) "Book Fixed Effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (e) "User Fixed Effects" correspond to a set of dummies for users' group ages (24-30, 31-40, 41-50, 51-60, 60+), gender (female), area of study (business and accounting), and time at school (0 to 4 years). (f) Sample Period: 2011-2012. (g) Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In the case of the alternative performance measures considered in the table, the results confirm the previous empirical pattern related to the probability of delays. In particular, the introduction of return boxes in one of the libraries tends, on average, to raise borrowings' effective durations and delays by a similar magnitude, in both cases (around three days). On the other hand, there is a negative correlation between boxes and early returns, suggesting that users delay returning books ahead of time, in this case. Finally, it is worth noting that the boxes do not affect the number of items that users borrow when they go to the library.

5. SENSITIVITY ANALYSIS

In the previous section, I reported a strong counterintuitive result: the introduction of a return box in a library tends, on average, to affect library performance measures in the opposite direction as predicted by standard transaction-cost theories. In the present section, I discuss the results of tests aimed at checking the robustness of the former empirical patterns reported above. I divide the section in two parts: a first section containing robustness checks, and a second section containing placebo tests.

5.1. Robustness checks

I present, in table 4, difference-in-differences estimates based on distinct samples. I do this to verify if the previous results are sensitive to alternative sample definitions. In the first three columns of the table, I exclude weekends, holydays, and vacations. In the fourth column, I exclude exam weeks from the sample, while in the fifth column, I exclude users who present delays higher than two standard deviations from average delays. I run this robustness check to avoid contamination of results by specific times of the academic year (columns 1-4) or unusual behavior (fifth column). Once again, results remain qualitatively the same.

Table 4 – Robustness: alternative samples

VARIABLES	(1) No Weekends	(2) No Holydays	(3) No Vacations	(4) No Exams	(5) No Outliers
DiD Coefficient	0.34***	0.33***	0.35***	0.32***	0.30***
	(0.056)	(0.061)	(0.055)	(0.058)	(0.069)
Acad. Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Book Fixed Effects	Yes	Yes	Yes	Yes	Yes
User Fixed Effects	Yes	Yes	Yes	Yes	Yes
Time Trends	Yes	Yes	Yes	Yes	Yes
Observations	31,731	35,065	34,166	32,806	33,133
Adj. R-squared	0.0742	0.0700	0.0667	0.0709	0.0654

Source: author's calculations, based on library data.

Notes: (a) The dependent variable in the specifications corresponds to the probability of delays in the library. (b) Standard errors clustered by course (reported in parentheses). (c) "Acad. Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2012-year. (d) "Book Fixed Effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (e) "User Fixed Effects" correspond to a set of dummies for users' group ages (24-30, 31-40, 41-50, 51-60, 60+), gender (female), area of study (business and accounting), and time at school (0 to 4 years). (f) In each column of the table, I report estimates based on different samples. "No Weekends" refer to samples in which I exclude weekends. "No Holydays" refer to samples in which I exclude holydays (second column). "No Vacations" refer to samples in which I exclude vacations (third column). "No Exams" refer to samples in which I exclude exam weeks (fourth column). "No Outliers" refer to samples in which I exclude users who present delays corresponding to two standard deviations above average. (g) Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Another potential source of concern in the present setting relates to the specific choice of treatment and control groups. Given the existing imbalance between the two libraries that I use to compare the impacts of lower transaction costs in a field setting, one could argue that such a choice may affect the main results. I try to address this concern by considering distinct combinations of treatment and control groups. In this case, I take advantage of the fact that, besides the two libraries used in the previous analyses, there is also an additional university library in my sample. This library, located in the main university campus (Liberdade), is responsible for attending the entire university community, including high school students, undergraduate and graduate students, MBA candidates, university employees, and professors.

I exploit the fact that the libraries in the sample introduced return boxes at distinct points of time. Specifically, while the libraries located at the Liberdade and Largo do São Francisco *campi* introduced boxes in 2012, the Pinheiros library did the same in 2013, only. As stated above, Liberdade firstly introduced two boxes at the time, while Largo do São Francisco introduced one, in 2012. This unique feature of the data allows me to compare these different libraries in pairs. If transaction costs are relevant in this setting, then one should expect to find significant differences between performance measures when comparing these libraries, either in terms of extensive or intensive margins. In the first case, a comparison between a library that introduced a box and one that did not represents an empirical test of the extensive margin hypothesis. In the second case, a comparison between libraries involving different numbers of boxes represents a test of the intensive margin.

In table 5, I present the results of difference-in-differences specifications involving distinct combinations of treatment and control groups. I consider the following combinations of libraries in the table: (i) Liberdade (Treatment) *versus* Pinheiros (Control); (ii) Liberdade (Treatment) *versus* Largo do São Francisco (Control); (iii) Liberdade and Largo do São Francisco, combined (Treatment) *versus* Pinheiros (Control). In all cases, I estimate specifications involving distinct library performance measures, as well as a complete set of covariates and time trends.

Table 5 – Robustness: Distinct treatment and control groups

Treatment x Control	(1)	(2)	(3)	(4)	(5)
Liberdade x *Pinheiros*	Prob.(Late)	Effective Duration	Delays	Early Returns	Item Count
DiD Coefficient	0.27***	2.81***	2.46***	-0.13***	-0.61**
	(0.059)	(0.456)	(0.344)	(0.045)	(0.266)
Observations	25,225	25,225	25,225	25,225	25,225
Adj. R-squared	0.0667	0.0588	0.0557	0.0404	0.104
Liberdade x *SanFran*	Prob.(Late)	Effective Duration	Delays	Early Returns	Item Count
DiD Coefficient	-0.04	0.04	-0.04	0.00	0.23
	(0.027)	(0.195)	(0.150)	(0.017)	(0.176)
Observations	48,561	48,561	48,561	48,561	48,561
Adj. R-squared	0.0639	0.0406	0.0409	0.0275	0.0741
(*Liberdade* + *SanFran*) x *Pinheiros*	Prob.(Late)	Effective Duration	Delays	Early Returns	Item Count
DiD Coefficient	0.30***	2.91***	2.54***	-0.15***	-0.80***
	(0.060)	(0.478)	(0.372)	(0.042)	(0.224)
Observations	54,728	54,728	54,728	54,728	54,728
Adj. R-squared	0.0638	0.0378	0.0393	0.0240	0.0507
Acad. Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Book Fixed Effects	Yes	Yes	Yes	Yes	Yes
User Fixed Effects	Yes	Yes	Yes	Yes	Yes
Time Trends	Yes	Yes	Yes	Yes	Yes

Source: author's calculations, based on library data.

Notes: (a) The dependent variable in the specifications corresponds to the probability of delays in the library (first column), borrowings' effective duration (second column), borrowings' delays (third column), early returns (fourth column), and item count (fifth column). (b) Standard errors clustered by course (reported in parentheses). (c) "Acad. Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2012-year. (d) "Book Fixed Effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (e) "User Fixed Effects" correspond to a set of dummies for users' group ages (24-30, 31-40, 41-50, 51-60, 60+), gender (female), area of study (business and accounting), and time at school (0 to 4 years). (f) Sample Period: 2011-2012. (g) In the table's first panel, I employ the Liberdade and Pinheiros units as treatment and control, respectively. In the table's second panel, I employ the Liberdade and Largo do São Francisco (denoted "San Fran") units as treatment and control, respectively. Finally, in the third panel, I employ the Liberdade and Largo do São Francisco units as treatment, and the Pinheiros unit as control. (h) Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results in the table suggest that the previous empirical patterns remain the same, when considering distinct combinations of treatment and control groups. In general, the present section suggests that the previous results are robust to modifications in the sample, as well as different treatment and control groups.

5.2. Placebo tests

In the present setting, I report a result in which the introduction of return boxes in libraries affect library performance measures in the opposite direction of standard transaction-cost theories. In table 6, I present the results of an alternative placebo test, in which I vary the pre-treatment period. In this case, if I correctly identify the mechanism by which return boxes translate into reduced transaction costs, affecting library performance measures, then these boxes should not have significant effects during pre-treatment periods. In order to test such a possibility, I consider distinct pre-treatment periods in each column of the table. In all cases, the dependent variable corresponds to the probability of delays ("Prob.(Late)"), with full specifications:

Table 6 – Placebo tests: Alternative pre-treatment periods

VARIABLES	(1) 2008	(2) 2009	(3) 2010	(4) 2011
DiD Coefficient	-0.02	-0.02	-0.01	0.00
	(0.031)	(0.021)	(0.018)	(0.018)
Acad. Year Fixed Effects	Yes	Yes	Yes	Yes
Book Fixed Effects	Yes	Yes	Yes	Yes
User Fixed Effects	Yes	Yes	Yes	Yes
Time Trends	Yes	Yes	Yes	Yes
Observations	107,685	107,685	107,685	107,685
Adj. R-squared	0.0572	0.0569	0.0568	0.0568

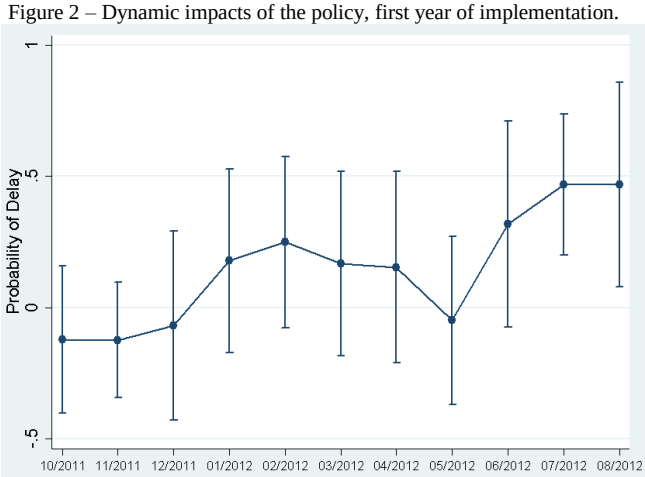
Source: author's calculations, based on library data.

Notes: (a) The dependent variable in the specifications corresponds to the probability of delays in the library. (b) Standard errors clustered by course (reported in parentheses). (c) "Acad. Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2012-year. (d) "Book Fixed Effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (e) "User Fixed Effects" correspond to a set of dummies for users' group ages (24-30, 31-40, 41-50, 51-60, 60+), gender (female), area of study (business and accounting), and time at school (0 to 4 years). (f) Sample Period: 2011-2012. (g) Alternative pre-treatment periods: 2008 (first column), 2009 (second column), 2010 (third column), and 2011 (fourth column). (g) Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results reported in the table lend confidence to the mechanism mentioned above. When looking at 'false' pre-treatment periods, I cannot reject the null hypothesis of no significance of the estimated difference-in-differences coefficient. Overall, the placebo tests reported in this section suggest that there is a meaningful effect of return boxes on library performance measures in the present context.

I also evaluate the dynamic impacts of the policy in its first year of implementation. I consider econometric specifications with time dummies (months) for the period between September 2011 and August 2012, as well as its interactions with the treatment dummy. In doing so, I want to evaluate the possible existence of anticipatory effects of the policy. If I am able to correctly identify the effects of the instauration of return boxes, than one would expect that such a policy exert no effect during the pre-treatment period. Figure 2 contains the results of this exercise:

Figure 2 – Dynamic impacts of the policy, first year of implementation.



Source: author's calculations, based on library data.

The graphical patterns suggest that the policy exerts no significant effects on the first eight months reported in the graph (10/2011 to 06/2012). In fact, the first policy effects occur in July 2012, six months after the instauration of return boxes in selected libraries. This result lends confidence to the identification of the main policy effects in the present setting.

6. CONCLUSION

Transaction-cost theory constitutes one of the most important contributions in social sciences today (Coase, 1992; Williamson, 2010). Previous research brought new insight to the mechanisms through which transaction costs affect important decisions in distinct areas, such as accounting, economics, management, and law (Macher & Richman, 2008). However, despite all the progresses made, few studies were able to evaluate the impacts of transaction costs on an information commons in the field. The present paper tries to fill this gap by studying the impacts of lower transaction costs in a specific type of information commons, a university library.

By exploiting variation in the introduction of a cost-saving technology (return boxes) in distinct libraries on time, I evaluate the impacts of lower transaction costs in a field setting. The main advantage of the present context is the fact that I am able to isolate the influence of transaction costs on behavior, as opposed to most previous contributions in the literature. Contrarily to standard arguments based on transaction costs, I find a result in which the instauration of return boxes tends, on average, to raise the

probability of delays, and borrowings' effective durations, at the same time that it lowers early returns and does not affect item counts.

In terms of limitations, the results here presented may lack external validity, since I study user behavior in different libraries of the same university. Although there seems to be considerable diversity among library users and university *campi*, one may argue that these results could reflect a very specific institutional setting. One way to circumvent this argument would be to evaluate the behavior of users in distinct types of libraries or departments, for instance. Although I do not pursue such a strategy here, I suspect that the main qualitative results would remain unchanged, in this case. Another caveat in the present setting refers to the details of the introduction of return boxes in the libraries. Having had several personal reunions with the libraries' staff, I learned that each library had, in most cases, introduced one box at a time, in specific locations of each campus (such as its entrance). A possible extension of this paper would be to run an experiment to verify if the boxes' locations could influence library performance measures on different time spans. In the end, one important question that remains unanswered is whether the empirical relevance of transaction costs depends on the specific context in which they are embedded or not.

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[i] Anderson and Schmittlein (1984), Gooroochurn and Hanley (2007), Joskow (1987), Lyons (1994), Muris, Scheffman, and Spiller (1992), Parmigiani (2007), Poppo and Zenger (1998, 2002), and Zylbersztajn and Lazzarini (2005) correspond to examples of empirical studies related to transaction-cost theories. See Foss and Klein (2006), Hart (1989), and Milgrom and Roberts (2011) for surveys related to theories of the firm.

[ii] This tradition is still present in modern discussions of the theme, such as standard textbooks in the areas of organizational economics and strategy (Besanko, Dranove, Shanley, & Schaefer, 2013).

[iii] It is important to note that there is a well-established theoretical literature relating transaction costs to common-pool resources (North, 1990; Ostrom, 2005). The present paper distinguishes itself from these contributions by empirically measuring the impacts of lower transaction costs on behavior in a specific type of common-pool resource, an information commons.

[iv] Aragón (2015) also tests the effects of lower transaction costs in a field setting. However, the author focus his analysis on the impacts of property rights' improvements on local communities in Canada. See also Alston and Mueller (2011), who study the importance of insecure property rights in Brazil. Foss and Foss (2005) discuss the importance of property rights for strategy theories, emphasizing that these theories "… paid little attention to

transaction cost-reducing practices" (p. 542). DellaVigna and Malmendier (2006) report a result in which health club members delay cancelling contracts, despite the presence of small transaction costs.

[v] Although I adopt North's (1990, 1991) definition, I am aware that this is a very specific definition of institutions. See Eggertsson (1990) and Hodgson (2006) for alternative definitions and related discussions on the theme. Joskow (1995) and Williamson (2000) describe some of the main concepts related to the New Institutional Economics. Commons (1931) corresponds to a seminal contribution related to Institutional Economics.

[vi] There is also an established literature related to the long-term effects of specific events. Acemoglu et al. (2001), Acemoglu and Robinson (2006), Alston and Mueller (2011), Aragón (2015), Baumol (1990), Becker and Woessmann (2009), La Porta, Lopez-de-Sillanes, and Shleifer (2008), Murphy, Shleifer, and Vishny (1991), Nunn (2008), Nunn and Qian (2010), Nunn and Wantchekon (2011), and Sokoloff and Engerman (2000) correspond to examples of studies of this kind. Nunn (2009) summarizes part of this literature, with an emphasis on the importance of historical effects. See also North, Wallis, and Weingast (2006), who emphasize the importance of political economy arrangements for long-run outcomes. Jones and Romer (2010) and Nelson and Sampat (2001) correspond to examples of studies which regard institutions as an important ingredient for growth models.

[vii] It is worth noting that there are considerable differences, in terms of sample size, between the treatment and control groups in the pre-treatment period. In the first case, I have a sample containing 4,268 observations, while I have only 192 observations, in the second case (both for the 2011 year). I try to circumvent this problem by implementing alternative estimation strategies. More details in the robustness section below.

[viii] Appendix B contains pictures of the libraries studied in this paper, as well as their respective return boxes.

[ix] I choose the period between September and December 2011 because it corresponds to the pre-treatment period in which I observe parallel trends for both groups. I do not observe the same result in the case of periods containing more months of the same year.

[x] There are 47 courses in the university during the 2011-2015 period. These courses differ from the areas of study (management, economics, accounting, international relations, advertising, and secretariat) that a student may choose when she enrolls in the university.