EFFECTS OF MONETARY SANCTIONS ON BEHAVIOR
Evidence from Library Fines

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Abstract

Discussions comparing fines and imprisonment suggest that, under certain conditions, the former might be preferred over the latter, especially in terms of economic efficiency. The present paper contributes to this debate by studying the behavioral responses to monetary sanctions in a unique field setting: a university library. I evaluate the behavior of library users during a ten-year period (2005-2015), covering more than 800,000 daily transactions. In doing so, I want to answer the following question: how does the introduction of a monetary sanction affect observed behavior in this specific setting? By exploiting variation in the introduction of a monetary sanction (fine) in the library, I find that such an introduction reduced users' delays, as predicted by standard models of law enforcement. However, when analyzing the dynamic effects of such an introduction, I find that the fine lost efficacy over time, since its nominal value remained the same after its instauration. These results not only have important implications for the design of sanctioning systems, but they also shed light on related issues, such as incentives, social norms, and corruption in real-world settings.

Key words: enforcement; fines; law and economics.

1. INTRODUCTION

One important discussion in the field of law and economics focus on the merits of alternative ways to deter illicit behavior. The classical economic model of crime predicts that, either monetary sanctions – such as fines – or imprisonment, can work as a deterrence factor for illicit activities (Becker, 1968; Stigler, 1974). While most contributions in the economics literature focused on the deterrent effects of imprisonment (Di Tella & Schargrodsky, 2004; Kline, 2012), the tradeoff among distinct types of punishment has received far less attention (Piehl & Williams, 2011). In fact, discussions comparing fines and imprisonment suggest that, under certain conditions, the former might be preferred over the latter, especially in terms of economic efficiency, since they correspond to mere transfers of money across society (Polinsky & Shavell, 2000).

The present paper presents novel evidence related to the effects of fines in a field setting. I study the behavior of users in a university library during a ten-year period (2005-2015), covering more than 800,000 daily transactions. In doing so, I want to answer the following question: how does the introduction of a monetary sanction affect behavior in this specific setting? I exploit variation in the introduction of a monetary sanction (fine) in the library in a specific year to uncover a causal effect of such a sanction over user behavior. Before 2006, the library studied in this paper had a sanctioning system based on nonmonetary sanctions (daily suspensions) for all of its users. After this period, the library started charging monetary sanctions (fines) for students with late items, while maintaining a nonmonetary sanction for professors and university employees. One particular advantage of this setting is that the change in the type of sanction was not the same for all library users. This unique feature of the data allows me to employ a difference-in-differences research design in order to evaluate the effects of the policy implemented by the library. In this case, the identification condition is that, in the absence of treatment, both groups – control and treatment – would display similar trends over time.

I uncover two main results. First, in aggregate terms, the instauration of a monetary sanction in the library reduced users' delays, as predicted by standard law enforcement models. Second, since the nominal value of the fine remained the same (R$ 2.00) during a ten-year period (2006/2015), this specific type of sanction lost efficacy over time. I report estimates of the dynamic effects of fines, which suggest that the fine deterred illicit behavior in the short run, only. While these results corroborate some of the main predictions derived from standard economic models, I explore possible related mechanisms, which could explain the behavioral response of users over time.

The remainder of the paper proceeds as follows. Section 2 presents a brief discussion of the related literature, as well as this paper's main contributions. Section 3 describes the data and research design employed in the empirical analysis below. Section 4 reports the main empirical results. Finally, section 5 concludes.

2. RELATED LITERATURE

There is not a clear consensus among economists, legal scholars and psychologists in terms of the superiority of monetary sanctions over other forms of punishment. For instance, there exists some evidence from laboratory experiments suggesting that different types of punishment can affect behavior through distinct channels (Fehr & Gächter, 2000; Masclet, Noussair, Tucker, & Villeval, 2003)[1]. In terms of field settings, the available evidence presents mixed results. In a famous study, Gneezy and Rustichini (2000a) uncovered a result in which the institution of fines in daycare centers in Israel would actually increase the number of late-incoming parents, instead of decreasing it. According to the authors, this result is a consequence of contracts' incompleteness. On the other hand, most previous contributions suggest that fines can deter illicit behavior, as predicted by standard models of law enforcement. When reporting the results of a study analyzing the impacts of personal experience with fines faced by video-rental store customers during a two-year period, Haselhuhn, Pope, Schweitzer, and Fishman (2012) conclude that monetary fees boost users' compliance. Bar-Ilan and Sacerdote (2004) report the results of a series of field experiments in the United States and Israel where traffic violations decrease in response to higher fines and the probability of being caught, as predicted by Becker's (1968) economic model of crime.

[1] Gneezy, Meier, and Rey-Biel (2011) and Kamenica (2012) discuss the importance of monetary and non-monetary sanctions on behavior, as well as the related evidence.

This paper dialogues with several literatures. First, the results presented here corresponds to an empirical analysis related to the public enforcement of law, a situation in which victims may not know who is injuring them. In such situations, the imposition of monetary fines may be a preferred option when compared to incapacitation (Polinsky & Shavell, 2000). In this sense, the present paper contributes to a growing literature discussing the desirability of fines as an efficient means to punish illicit behavior (Gneezy & Rustichini, 2000a; Piehl & Williams, 2011). Specifically, the results here presented complement previous empirical analyses focused on the impacts of fines, such as Agarwal et al. (2013), Bar-Ilan and Sacerdote (2004), Fisman and Miguel (2007), and Haselhuhn et al. (2012). However, contrarily to these papers, the present setting allows me to study the causal effects of the instauration of a monetary sanction. By exploiting variation in the imposition of fines over time for distinct groups of users, I am able to uncover a causal effect of fines over illicit behavior. Additionally, the focus on the dynamic effects of the fine represents an innovation when compared to previous studies, which focused on shorter periods of time (Bar-Ilan & Sacerdote, 2004; Donna & Espín-Sánchez, 2015).

Second, this paper's results also relate to contexts in which agents might value nonmonetary factors, such as customs or social norms, making the instauration of monetary sanctions a less effective instrument to deter certain behaviors (Acemoglu & Jackson, 2017; Bénabou & Tirole, 2006, 2011; Ellingsen & Johannesson, 2007). In fact, there are situations where, due to contracts' incompleteness, agents may interpret monetary sanctions as a price to pay for additional goods and services. In such situations, monetary sanctions may actually backfire (Gneezy, Meier, & Rey-Biel, 2011; Gneezy & Rustichini, 2000a). The present paper contributes to this research, by comparing the behavioral responses of agents subject to distinct types of sanctions in a field setting.

Finally, the paper's findings relate to the literature in social dilemmas, such as common-pool resources' management, for instance (Hardin, 1968; Ostrom, 1999). While most of the previous contributions in the literature emphasized examples related to environmental themes such as forests, fisheries, and wildlife in general (Fehr & Leibbrandt, 2011; Rustagi, Engel, & Kosfeld, 2010; Zylbersztajn, 2010), I present an example related to an information commons, a library (Hess & Ostrom, 2007). As far as I know, this is probably the first attempt to study the effects of monetary sanctions in a specific type of common-pool resource, an information commons. In this sense, the present paper contributes to understanding the impacts of distinct sanctions over behavior, with an emphasis on social dilemmas involving collective action.

3. DATA AND METHOD

3.1. Institutional Background and Data

In this paper, I study the behavior of library users covering more than 800,000 transactions during a 10-year period (2005-2015). I have access to confidential daily data related to library users of a private university in São Paulo, Brazil, for the 2005-2015 period. The data contain detailed information on 17,498 individual users, covering more than 800,000 daily transactions. This corresponds to an unbalanced panel, since each library user may borrow different numbers of specific library items at distinct moments. For instance, one user might borrow two books on March 1st, and then borrow one more book on March 3rd, before returning previous items.

The main advantage of studying the 2005-2015 period is that I am able to investigate the dynamic effects of the instauration of a monetary sanction over time. However, since the data begins in 2005, I do not have additional data to test for pre-treatment behavior. This is the reason why I focus on the 2005 year when evaluating pre-policy events. Since I am interested in the immediate effects of the monetary sanction, I focus the analysis on the 2005-2006 period. 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 borrowed 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.

I also have access to the library's official yearly reports. These reports contain rich institutional information related to the library's internal workings over the 2005-2015 period (Choi, 2016). Based on this information, I am able to estimate the predicted devolution date for each user in the sample. In this specific case, the library's electronic system imposes a rule of 15 days for professors and masters' students, and seven days, for all other users. 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 over time. There are also differences in terms of the number of items that each user can borrow from the library: while professors and masters's students can borrow a maximum limit of seven items, students can borrow a maximum of five, while university employees can borrow three items, only. This information allows me to build additional performance measures for each user in the sample, such as the number of items that she borrows every time she goes to the library, as well as measures of delays over time (equal to the difference between the predicted and effective devolution dates for each item borrowed). I also build measures for 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). Finally, I complement the data with official calendar information related to exams' weeks occurred in the university over time.

4. RESULTS

4.1. Main 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 institutional change in the library. I try to circumvent this difficulty by considering as the treatment group users who suffered any kind of sanction (nonmonetary or monetary) during the entire sample period. For instance, users who present delays over time suffered either a nonmonetary sanction (daily suspension) in the pre-policy period or a monetary sanction (fine), afterwards. On the other hand, the control group corresponds to users who did not suffer any sanction. Initially, I exclude professors and employees from the treatment and control groups, since these categories were not subject to monetary sanctions in either period. It is worth noting that the latter categories present specific characteristics that make difficult comparisons to groups containing students. I return to these groups below, when presenting placebo tests for the identification condition used in the paper. Table 1 presents summary statistics for selected variables for both groups in the pre-policy period (the 2005 year):

Table 1
Descriptive Statistics – Pre-Policy Period (2005)

VARIABLE	Control Group	Treatment Group	Total Sample
Age	25.56	24.40	25.13
	(7.11)	(5.82)	(6.68)
Female	0.55	0.56	0.55
	(0.50)	(0.50)	(0.50)
First Year	0.29	0.31	0.29
	(0.45)	(0.46)	(0.46)
Predicted Duration	8.02	7.05	7.66
	(2.67)	(0.62)	(2.20)
Business Book	0.35	0.34	0.34
	(0.48)	(0.47)	(0.48)
Accounting Book	0.12	0.12	0.12
	(0.33)	(0.33)	(0.33)
Economics Book	0.16	0.17	0.16
	(0.37)	(0.38)	(0.37)
Observations	39,522	23,271	62,793

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

The table's first and second columns display results for the control and treatment group, respectively. The third column contains results for the total sample (treatment + control) during the pre-policy period. A visual inspection of the results in the table suggests that both groups (treatment and control) present very similar characteristics in the period before the instauration of a monetary sanction in the library. The only exception is for each group's predicted duration. At first, this result may reflect compositional differences between the groups, since distinct user categories face different limits in terms of predicted duration.

I present the results of difference-in-differences estimations of equation (1) in Table 2. In this case, the dependent variable corresponds to the proportion of delays above average in the period. I progressively add covariates to the specifications in the table in order to control for fixed-effects that might bias the resulting estimates, a common practice in studies of this kind.

Table 2

Effects of Fines over Probability of Delays

VARIABLES	(1) Prob.(Late)	(2) Prob.(Late)	(3) Prob.(Late)	(4) Prob.(Late)	(5) Prob.(Late)
DiD Coefficient	-0.01	-0.03	-0.03	-0.04**	-0.04**
	(0.023)	(0.022)	(0.022)	(0.017)	(0.017)
Academic Year Fixed Effects	No	Yes	Yes	Yes	Yes
Book Fixed Effects	No	No	Yes	Yes	Yes
User Fixed Effects	No	No	No	Yes	Yes
Time Trends	No	No	No	No	Yes
Mean Dep. Variable	0.233	0.233	0.233	0.233	0.233
Observations	128,156	128,156	128,156	128,156	128,156
Adj. R-squared	0.467	0.511	0.511	0.515	0.516

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

Notes: (a) The dependent variable in the specifications corresponds to the probability of higher than average delays in the library. (b) Standard errors clustered by course (reported in parentheses). (c) "Academic Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks of the year and the 2006-year. (d) "Book fixed effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (d) "User fixed effects" correspond to a set of dummies for users' group ages (14-17, 18-23, 24-30, 31-40, 41-50, 51-60, 60+), category (high school student, undergraduate, graduate, and former student), area of study (business, accounting, and economics), and time at school (0 to 4 years). (d) "Time Trends" correspond to variable time trends for the control and treatment groups. (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, in order to capture seasonal effects over borrowings. I also add dummies for days of the week, month, and year. In the third column, I add book and library dummies to capture differences in terms of items borrowed. In the fourth column, I add a rich set of user-related covariates in order to capture users' fixed effects: their gender, age group, area of study, category, and time at school. Finally, in the fifth column, I add specific time trends for both the control and the treatment groups. I do this inclusion to allow distinct trends for each group over time, which corresponds to an alternative test of the parallel trends hypothesis in a difference-in-differences' research setting (Besley & Burgess, 2004)[2].

[2] There is one important, but untestable hypothesis, in a difference-in-differences research design. In this specific case, users' non-observable characteristics should follow similar time trends, either in the control or treatment groups. Although I am not able to test this hypothesis in the

The first column in the table finds no statistically significant correlation between the introduction of a monetary sanction and the proportion of delays. This result may be a consequence of a lack of inclusion of other variables, which might bias the reported estimates. Even after adding dummies for seasonal effects (second column), as well as books' and libraries' fixed effects (third column), I do not find a statistically significant effect of fines over the probability of delays. However, once I include users' fixed effects (fourth column), and specific time trends (fifth column), the estimated coefficients become statistically and economically significant at a 5% level of significance (-.04). This result suggests that, if the difference-in-differences identification hypothesis is valid, there is a negative causal effect of monetary sanctions over delays. Specifically, the introduction of fines reduces the probability of higher than average delays by 17% (= -0.04/0.23).

4.2. Dynamic Effects

One unique feature of the data used in this paper relates to its longitudinal dimension. I am able to observe 830,813 transactions by 17,397 library users, covering more than 10 years (2005-2015). In this section, I explore the data's longitudinal dimension in more detail. In doing so, I hope to gain a better understanding of the long-term impacts of the fine over behavior. I exploit the fact that the fine, once it began, remained with the same nominal value (R$ 2.00) for the entire period after the 2006 year. In this case, there are two related possibilities. First, if library users perceive the fine's constancy over time, they will probably respond to it by raising delays, since the fine's real value declines as time goes by (given a positive rate of inflation, in aggregate terms). Second, even with a fine with constant nominal value, there is the possibility that users' delays reduce over time, due to the psychological effects of a sanction of this kind. Figure 1 displays the dynamic effects of the introduction of the fine over the probability of higher than average delays for the 2006-2015 period. Figure 2 contains the dynamic effects for users' alternative measures of performance.

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present setting, I try to address such a possibility by including specific time trends in my estimations, following the suggestions contained in Besley and Burgess (2004). Given the additional possibility of severe serial correlation among users and library borrowings over time, as originally suggested by Bertrand, Duflo, and Mullainathan (2004), I cluster standard errors by the number of courses offered at the university (equal to 52).

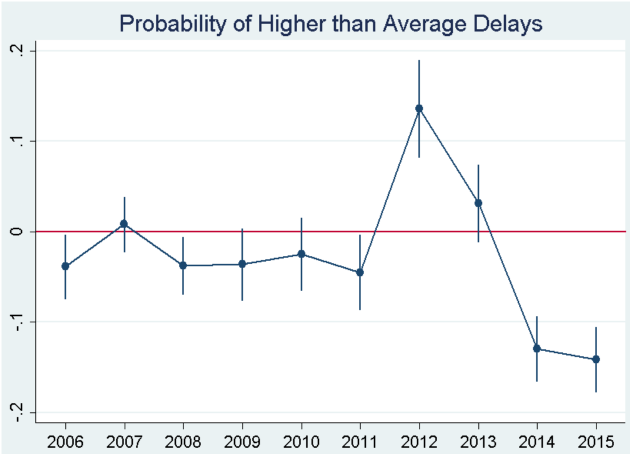

Figure 1. Dynamic Effects of a Fine over the Probability of Higher than Average Delays, 2006-2015.
Source: author's calculations, based on library data.

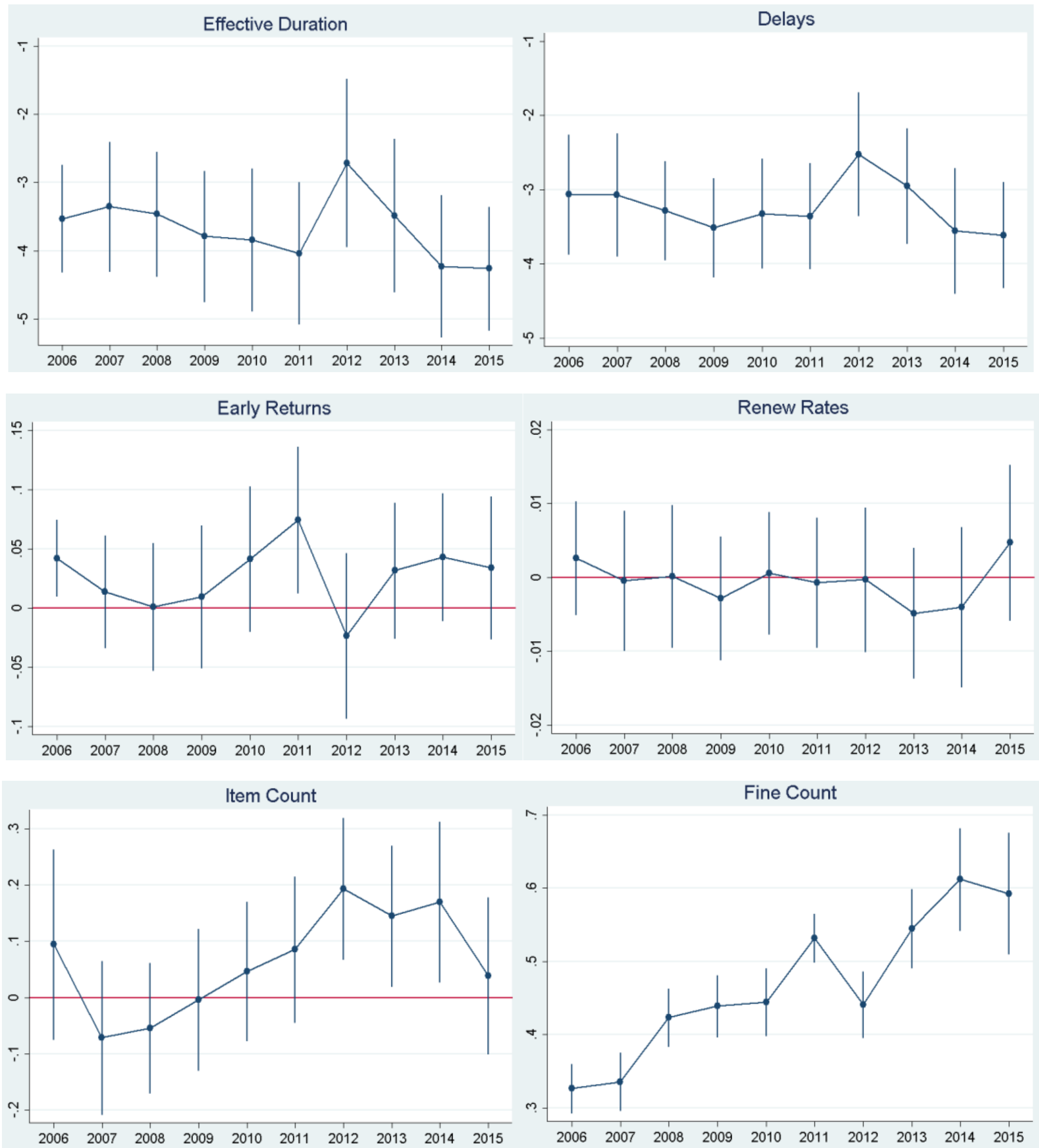

Figure 2. Dynamic Effects of a Fine over Distinct Library Performance Measures, 2006-2015.
Source: author's calculations, based on library data.

When looking at the dynamic effects of the fine over other variables, I uncover two distinct sets of results. First, according to the patterns in the graph, the fine has an initial negative impact of – 4 percentage points (p.p.) over the probability of delays, in the year following its introduction (2006). However, after that period, there is not a statistically significant impact in the following five years (2007-2011). There is a positive impact in 2012, no impact in 2013, and negative impacts for the 2014-2015 period. While it seems unlikely that the fine impacts still occur five years past its instauration, this dynamic evidence suggests that the deterrent effects of the fine are short-lived, at best. This is probably a direct consequence of the fine's constant nominal value.

Second, while the fine has a clear contractionary effect over borrowings' effective durations and delays, it does not present a robust pattern in the case of early returns, renew rates, and the number of items each user borrows. Interestingly, when looking at the number of fines each user receives over time, I uncover a result in which the number of fines increases over time. That is, contrarily to standard arguments concerning the efficacy of fines as a deterrence factor for illicit behavior, fines increase over time.

5. SENSITIVITY ANALYSIS

In the previous section, I reported a result in which the instauration of a monetary sanction reduced illicit behavior in a statistical and economic meaningful way. Although this is an intuitive and interesting result, it may present different types of bias for several reasons. In this section, I present results from distinct tests to validate the main results reported before. I divide the section in three parts: a first section containing placebo tests, a second section containing robustness checks, and a third section, in which I explore alternative mechanisms.

5.1. Placebo Tests

In the case of the present setting, fines make delays more expensive and this affects users' behavior, on average, lowering delays. If this basic mechanism applies to the library I study, then it should not affect variables that would not correlate with monetary sanctions, at first. Given this reasoning, I present, in Table 3, estimates of difference-in-differences regressions, where the dependent variables correspond to variables that the fine should not affect. Specifically, I consider specifications in which the dependent variable is either users' gender (first column) or borrowings' predicted duration (second column). Additionally, I consider specifications where I substitute the treatment group with professors (third column) or university employees (fourth column), given that none of these categories were subject to monetary sanctions during the 2005-2015 period. Once again, I employ complete specifications, controlling for the same fixed-effects as before, as well as specific time trends.

Table 3
Placebo Tests

VARIABLES	(1) Female	(2) Predicted Duration	(3) Professors	(4) Employees
DiD Coefficient	0.01	0.65*	-0.06	0.04
	(0.009)	(0.384)	(0.056)	(0.161)
Academic 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	128,156	126,352	128,156	128,156
Adj. R-squared	0.0745	0.286	0.458	0.456

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

Notes: (a) The dependent variable in the specifications corresponds to users' gender (first column), and borrowings' predicted duration (second column). In the case of the table's third and fourth columns, I substitute the treatment group either by professors (third column) or by university employees (fourth columns). (b) Standard errors clustered by course (reported in parentheses). (c) "Academic Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2006-year. (d) "Book fixed effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (d) "User fixed effects" correspond to a set of dummies for users' group ages (14-17, 18-23, 24-30, 31-40, 41-50, 51-60, 60+), category (high school student, undergraduate, graduate, and former student), area of study (business, accounting, and economics), and time at school (0 to 4 years). (d) "Time Trends" correspond to variable time trends for the control and treatment groups. (g) Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results reported above confirm that monetary sanctions do not affect the dependent variables considered in the table. When looking at users' specific characteristics, such as gender and age, I cannot reject the null hypothesis of no significance of the estimated difference-in-differences coefficient. Similarly, when employing either professors or employees as the treatment group, I cannot find a significant difference-in-differences effect. The only exception to such a pattern is borrowings' predicted duration, which is marginally significant, in this case. Overall, the placebo tests reported in this section suggest that there is a meaningful effect of monetary sanctions over behavior in this context.

5.2. *Robustness Checks*

In Table 4, I present difference-in-differences estimates based on different samples. I do this in order to verify if the previous results are sensitive to alternative sample definitions.

Table 4
Robustness: Alternative Samples

VARIABLES	(1) Legal Counts	(2) No Vacations	(3) No Holydays	(4) No Exams
DiD Coefficient	-0.04**	-0.03**	-0.04**	-0.04**
	(0.016)	(0.015)	(0.017)	(0.018)
Academic 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	122,676	112,663	127,544	110,940
Adj. R-squared	0.517	0.526	0.516	0.512

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

Notes: (a) The dependent variable in the specifications corresponds to the probability of higher than average delays in the library. (b) Standard errors clustered by course (reported in parentheses). (c) In each column, I exclude a specific part of the sample: no vacations (first column), no book counts outside the library's legal rules (second column), no holydays (third column), and no exams' weeks (fourth column). (d) "Academic Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2006-year. (d) "Book fixed effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (d) "User fixed effects" correspond to a set of dummies for users' group ages (14-17, 18-23, 24-30, 31-40, 41-50, 51-60, 60+), category (high school student, undergraduate, graduate, and former student), area of study (business, accounting, and economics), and time at school (0 to 4 years). (d) "Time Trends" correspond to variable time trends for the control and treatment groups. (g) Statistical significance: * p < 0.10, ** p < 0.05, *** p < 0.01.

In the first column of the table, I exclude from the sample users whose items' count surpass the library's legal limits. Although the library has clear limits concerning each user category, it is possible that some users have special permission from librarians to take more items during specific times of the academic year. For instance, some professors may be able to take more than seven books in the beginning of the semester in order to organize courses[3]. In the second column, I exclude vacation periods. I do this because one could speculate that the main results concerning the effects of fines over delays might present biases due to longer delays during periods when classes are over. Similarly, in the third and fourth columns, I exclude holidays and exam weeks from the sample, in order to avoid contamination of the results by specific times of the academic year. In the latter case, one possible concern would be that users could exhibit a different behavior during exam weeks. For instance, some users could hold books for longer time during exam weeks, given the library's rivalry property. Note that the exclusion of these specific periods (vacations, holydays and exam weeks) correspond to indirect tests of the importance of users' behavioral biases, such as inattention during holydays, or opportunistic behaviors during exam weeks. I

[3] I confirm this empirical pattern by looking at the data. In certain occasions, a few users in the library took more items than allowed over time. However, the vast majority of the sample (99.96%) conforms to the library's legal rules concerning maximum limits per user.

return to these points below, when I discuss in detail possible mechanisms affecting the main results. In all cases of the above table, the previous results remain robust to minor modifications in the sample.

5.3. Alternative Mechanisms

Up to this point, I uncovered a result in which the instauration of a monetary sanction (fine) in the library under study affects illicit behavior in the way predicted by standard models of law enforcement. However, when examining the dynamic effects of the fine over the period 2006-2015, I notice that, although the fine affects effective duration and delays permanently, it does not do so with other variables, such as items' counts and renew rates. More importantly, when looking at the number of fines per user, there is a consistent increasing pattern over the entire period after the fine instauration. In this section, I explore possible mechanisms underlying the last result, related to the lack of long-term effects of this sanction. Specifically, I explore three possibilities, based on different driving forces, that I call "opportunism", "inattention", and "learning". I discuss each of these mechanisms in detail below.

5.3.1. Opportunism

As mentioned above, one potential source of bias in the present context relates to the possibility of opportunistic behaviors by library users. Given the library's rivalry property, some users might retain books during exam weeks, in order to harm other users, for instance. If opportunistic behaviors play a significant role in explaining the effects of the fine in this context, then one would expect that delays are higher in exams' weeks, or in dates near them. I test such a prediction in table 9, by considering the interaction of difference-in-differences coefficients with specific periods of the academic calendar year. Specifically, by having access to official university information, I build specific dates for exams, as well as dates nearby (one day, three days, and seven days before exams' weeks).

Table 9
Mechanisms: Opportunism

VARIABLES	(1) Prob.(Late)	(2) Prob.(Late)	(3) Prob.(Late)	(4) Prob.(Late)
DiD Coefficient	-0.01	-0.02	-0.02	-0.02
	(0.014)	(0.015)	(0.014)	(0.014)
DiD Coefficient x Exams	-0.06***			
	(0.011)			
DiD Coefficient x Exams (1day before)		-0.02		
		(0.015)		
DiD Coefficient x Exams (3days before)			-0.01	
			(0.012)	
DiD Coefficient x Exams (7days before)				0.01
				(0.014)
Academic 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	830,813	830,812	830,810	830,806
Adj. R-squared	0.508	0.507	0.507	0.507

Notes: (a) The dependent variable in the specifications corresponds to the probability of higher than average delays in the library. (b) Standard errors clustered by course (reported in parentheses). (c) "Academic Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2006-year. (d) "Book fixed effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (d) "User fixed effects" correspond to a set of dummies for users' group ages (14-17, 18-23, 24-30, 31-40, 41-50, 51-60, 60+), category (high school student, undergraduate, graduate, and former student), area of study (business, accounting, and economics), and time at school (0 to 4 years). (d) "Time Trends" correspond to variable time trends for the control and treatment groups. (g) Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

As the table's results suggest, there are no significant effects of dates near exam weeks over delays. Actually, the probability of delays is smaller during exams' weeks, not otherwise. This result suggests that opportunistic behaviors are not affecting the main results reported above.

5.3.2. Inattention

Another potential source of bias in the present setting relates to users' behavioral biases. Library users may present cognitive limitations in the sense suggested by Simon (1955), which could affect borrowings' delays. One related example in the present context is that users could lack attention in terms of predicted devolution dates. When surveying part of the evidence related to behavioral economics, DellaVigna (2009) states that pre-weekend dates could proxy for inattention. I build on this insight to test if the probability of delays is higher in specific days of the week, such as Fridays, for instance. If users' inattention plays an important role in explaining the previous results, the one should expect to see higher delays in days before weekends. I test this hypothesis in table 10, by presenting results of difference-in-differences specifications where I consider the effects of the fine for each day of the week:

Table 10
Mechanisms: Inattention

VARIABLES	(1) Prob.(Late)	(2) Prob.(Late)	(3) Prob.(Late)	(4) Prob.(Late)	(5) Prob.(Late)	(6) Prob.(Late)
DiD Coefficient	-0.03*	-0.02	-0.03*	-0.01	-0.01	-0.02
	(0.013)	(0.014)	(0.014)	(0.013)	(0.015)	(0.016)
DiD Coefficient x Monday	0.02*					
	(0.014)					
DiD Coefficient x Tuesday		0.01				
		(0.014)				
DiD Coefficient x Wednesday			0.03**			
			(0.013)			
DiD Coefficient x Thursday				-0.07***		
				(0.014)		
DiD Coefficient x Friday					-0.06***	
					(0.016)	
DiD Coefficient x Saturday						0.06***
						(0.018)
Academic Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Book Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
User Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	830,813	830,813	830,813	830,813	830,813	830,813
Adj. R-squared	0.495	0.494	0.492	0.490	0.509	0.503

Notes: (a) The dependent variable in the specifications corresponds to the probability of higher than average delays in the library. (b) Standard errors clustered by course (reported in parentheses). (c) “Academic Year Fixed Effects” correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2006-year. (d) “Book fixed effects” correspond to a set of dummies for books’ area of study (business, accounting, economics, and law). (d) “User fixed effects” correspond to a set of dummies for users’ group ages (14-17, 18-23, 24-30, 31-40, 41-50, 51-60, 60+), category (high school student, undergraduate, graduate, and former student), area of study (business, accounting, and economics), and time at school (0 to 4 years). (d) “Time Trends” correspond to variable time trends for the control and treatment groups. (g) Statistical significance: * p < 0.10, ** p < 0.05, *** p < 0.01.

Contrarily to the previous intuition, I cannot find a “Friday-effect” for library’s delays. For most specifications in the table, there is not a robust effect of fines over behavior for specific days of the week. The probability of delays is higher on Saturdays, but it presents a similar magnitude and opposite sign for Fridays, which seems hard to reconcile with arguments centering inattention as the main driver of the reported results. This is an intuitive result; since the library’s electronic system (*Pergamum*) sends reminders to late users, with such reminders being even sent in advance, in some occasions[4].

5.3.3. Learning

When evaluating the dynamic effects of fines in the library setting studied in this paper, I uncovered a result in which the number of fines faced by users raised over time. In this section, I test the possible occurrence of

[4] In personal reunions with the library manager, she informed me that *Pergamum* automatically sends electronic reminders to users even before the borrowings’ due date is over. Apesteguia, Funk, and Iriberri (2013) and Chetty et al. (2014) correspond to examples of field experiments focusing on the importance of reminders in distinct settings. See Wiederholt (2010) for a brief survey of inattention theories.

learning effects among users. Specifically, I try to verify the occurrence of social learning in this context. When analyzing learning and peer effects in a sample of movie releases during the 1982-2000 period, Moretti (2011) defines social learning as "…a situation where consumers in week *t* update their prior based on feedback from others who have seen the movie in previous weeks" (p. 357). While I am not able to observe how library users personally interact in the library, I can employ a similar empirical strategy to Moretti's (2011) in order to evaluate the relative importance of learning effects[5].

To test for the occurrence of learning in this context, I employ two alternative strategies. First, I present differences-in-differences estimations in table 11, where I allow for heterogeneous effects based on book usage, previous delays, and the number of fines per user. If learning effects are relevant in this setting, than one could expect that interactions involving either one of these variables would present a negative sign. For instance, users who borrow specific books more often would learn the importance of monetary sanctions faster than users who do otherwise. Similarly, users who faced monetary sanctions in the past – either in the form of previous delays or positive fines – would learn not to delay in future occasions.

Table 11

Mechanisms: Learning

VARIABLES	(1) Prob.(Late)	(2) Prob.(Late)	(3) Prob.(Late)
DiD Coefficient	-0.01	-0.02*	-0.01
	(0.019)	(0.014)	(0.020)
DiD Coefficient x Book Usage	-0.00		
	(0.000)		
DiD Coefficient x Late Before		-0.00	
		(0.014)	
DiD Coefficient x Fine Count			0.04**
			(0.014)
Academic Year Fixed Effects	Yes	Yes	Yes
Book Fixed Effects	Yes	Yes	Yes
User Fixed Effects	Yes	Yes	Yes
Time Trends	Yes	Yes	Yes
Observations	830,813	830,813	830,813
Adj. R-squared	0.507	0.508	0.561

Notes: (a) The dependent variable in the specifications corresponds to the probability of higher than average delays in the library. (b) Standard errors clustered by course (reported in parentheses). (c) "Academic Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2006-year. (d) "Book fixed effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (d) "User fixed effects" correspond to a set of dummies for users' group ages (14-17, 18-23, 24-30, 31-40, 41-50, 51-60, 60+), category (high school student, undergraduate, graduate, and former student), area of study (business, accounting, and economics), and time at school (0 to 4 years). (d) "Time Trends" correspond to variable time trends for the control and treatment groups.

[5] Agarwal, Driscoll, Gabaix, and Laibson (2013) report a result in which monetary fee payments can induce learning in a large sample of credit card statements.

(g) Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Contrarily to the previous intuition, the results presented in the table do not confirm the hypothesis that library users learn from monetary sanctions. There are no effects in terms of book usage, suggesting that library users who borrow more often do not learn about the deterrent effects of fines, as time goes by. In the case of users who presented delays before, the estimated coefficient is not significant, while it is positive, in the case of the number of fines each user faces.

In terms of the second empirical strategy used to test the importance of learning, I follow Moretti (2011) by using age as a *proxy* for social networks. Specifically, one could expect that younger users would have larger social networks, which could induce faster learning by them. I do this to test the hypothesis that social learning should be stronger for those users who have large social networks. In fact, there is a large literature describing the benefits of social networks for information dissemination, thus allowing for significant gains in terms of learning[6]. In table 12, I present difference-in-differences estimates allowing for heterogeneous effects based on users' age groups.

[6] See Granovetter (1973, 1985) for pioneering contributions on the theme, with an emphasis on the importance of social ties for information diffusion. Granovetter (2005), Jackson (2008, 2014), and Munshi (2014) correspond to surveys on the growing importance of networks for economic outcomes. Allcott, Karlan, Möbius, Rosenblat, and Szeidl (2007), Costa and Kahn (2007), and Marmaros and Sacerdote (2006) correspond to empirical analyses related to the importance of social networks in distinct contexts.

Table 12
Mechanisms: Learning

VARIABLES	(1) Prob.(Late)	(2) Prob.(Late)	(3) Prob.(Late)	(4) Prob.(Late)	(5) Prob.(Late)	(6) Prob.(Late)	(7) Prob.(Late)
DiD Coefficient	-0.02	-0.02	-0.01	-0.02**	-0.02	-0.02	-0.02
	(0.014)	(0.022)	(0.017)	(0.011)	(0.012)	(0.014)	(0.014)
DiD Coefficient x Age (14-17)	-0.05*						
	(0.030)						
DiD Coefficient x Age (18-23)		-0.00					
		(0.022)					
DiD Coefficient x Age (24-30)			-0.02*				
			(0.012)				
DiD Coefficient x Age (31-40)				0.06*			
				(0.037)			
DiD Coefficient x Age (41-50)					0.08**		
					(0.036)		
DiD Coefficient x Age (51-60)						-0.18	
						(0.113)	
DiD Coefficient x Age(60+)							-0.31***
							(0.079)
Academic Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Book Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
User Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	830,813	830,813	830,813	830,813	830,813	830,813	830,813
Adj. R-squared	0.507	0.507	0.507	0.507	0.507	0.507	0.507

Notes: (a) The dependent variable in the specifications corresponds to the probability of higher than average delays in the library. (b) Standard errors clustered by course (reported in parentheses). (c) "Academic Year Fixed Effects" correspond to a set of dummies for 6 days of the week, 51 weeks for each year, and the 2006-year. (d) "Book fixed effects" correspond to a set of dummies for books' area of study (business, accounting, economics, and law). (d) "User fixed effects" correspond to a set of dummies for users' category (high school student, undergraduate, graduate, and former student), area of study (business, accounting, and economics), and time at school (0 to 4 years). (d) "Time Trends" correspond to variable time trends for the control and treatment groups. (g) Statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The evidence presented in the table does not support the existence of learning effects in the present context. Although the estimated coefficient presents the expected sign and magnitude for the youngest cohort in the sample (users in the 14-17 age group), the same does not occur in the case of the two following cohort (18-23 age group), which would be expected to have larger than average social networks. More than that, it is worth noting that, while older cohorts present higher values for the probability of delays, the oldest cohort (60+ age group) presents the strongest negative effect among age groups. The latter result seems implausible in the present context: if stronger social networks inversely related to age, than one would expect that older users present higher values for the probability of delays, not otherwise. These results could be reflecting the wealth status of such age groups, since both might present higher elasticities to the effects of a monetary fine. To sum up, I cannot find convincing evidence regarding the importance of social learning in the present context. If anything, learning effects happen in the opposite direction: library users, by noticing the constancy of the fine's nominal value over time, raise delays, instead of diminishing them.

6. CONCLUSION

Different types of sanctions can affect behavior in varied (and unexpected) ways. In this paper, I try to answer the following question: how does the introduction of a monetary sanction affect behavior? Using a unique field setting and a longitudinal dataset covering more than 800,000 daily transactions from a university library during a 10-year period (2005-2015), I analyze how the introduction of a monetary sanction (fine) affects users' delays.

By exploiting the fact that the library instituted, in a given year, a fine for students only, I estimate the causal effects of the introduction of a monetary sanction over behavior. Three main results emerge. First, in aggregate terms, the introduction of the monetary sanction reduces delays by 34%, approximately. Second, there is considerable heterogeneity in terms of the effects over distinct groups of library users. Third, when considering the dynamic effects of this type of sanction, I uncover a result in which the sanction lost efficacy over time, since its nominal value remained the same for the entire period. The constancy of the fine's nominal value not only affects its efficacy over time, but it also affects users in differentiated ways, since those who are wealthier tend to be less sensitive by this monetary sanction. These results are robust to several specification issues, such as variations in sample definitions and periods, as well as the use of distinct library performance measures.

Overall, the results reported in this paper suggest that monetary sanctions can influence behavior in the desired direction, as predicted by standard models of law enforcement. More than that, these results emphasize the dynamic effects of this kind of sanction, since the imposition of fines impacts behavior over time. The uniqueness of the library setting studied in this paper call attention for two important points related to the impacts of fines over time. First, the results reported in this paper suggest the importance of adjusting fines' values to users' wealth, as predicted by previous contributions in the area. According to some authors (Polinsky & Shavell, 2000), one important aspect related to the instauration of fines is adjusting its value to users' wealth. By not adjusting fines in this manner, the library under study ended up punishing poorer users harder. Second, the dynamic effects' results emphasize the symbolic effects of fines, since the fine's nominal value remained constant for a 10-year period. Given that library users observe these values, as well as the enforcement done by the library, they can adapt their decisions to such an environment.

These results have important implications, both in practical and theoretical grounds. In terms of practice, the results provide valuable insight for important organizational issues, such as agency and teamwork issues, for example. In terms of theory, the results in this paper not only provide a better understanding of the impacts of fines over illicit behavior, but they also shed light on related issues, such as economic incentives, social norms, and corruption in real-world settings. In particular, an interesting route of future research would be to explore some of the results predicted by models emphasizing the interaction between social norms and laws.

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