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The Impact of Personal Experience on Behavior: Evidence from Video-Rental Fines

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Personal experience matters. In a field setting with longitudinal data, we disentangle the effects of learning new information from the effects of personal experience. We demonstrate that experience with a fine, controlling for the effect of learning new information, significantly boosts future compliance. We also show that experience with a large fine boosts compliance more than experience with a small fine, but that the influence of experience with both large and small fines decays sharply over time.

Key words: behavioral economics; decision making; backward-looking behavior; decisions following descriptions versus experience; learning
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1. Introduction

After renting the movie *Apollo 13* Reed Hastings misplaced his video cassette. He found the cassette six weeks later and faced a $40 late fee. The experience of paying this late fee was so aversive for Hastings that it motivated him to take an action that would fundamentally change the entire video-rental industry: In 1997, Hastings founded Netflix (Zipkin 2006).

Even though Hastings was aware of the late-fee policy, it was the experience of paying the fine that motivated him to change his behavior. In this paper, we examine the unique influence of personal experience on subsequent decision making and behavior.

Economic models typically assume that new information changes behavior (e.g., Becker 1976). These models have considered the content and reliability of new information, but have largely ignored the influence of how new information is obtained. Recent work, however, has found that how individuals receive information matters. In particular, an emerging body of research suggests that information gained from *experience* may be particularly influential in changing judgments and decisions (e.g., Agarwal et al. 2008, Simonsohn et al. 2008, Harvey 2005, Weber et al. 2004, Barron and Erev 2003). For example, a prospective diner may be more likely to avoid a restaurant after experiencing poor service there than after reading a review of the poor service others have had at that restaurant.

Important questions, however, remain about the influence of personal experience on behavior. Prior work has explored this link in laboratory settings (e.g., Simonsohn et al. 2008), but it is possible that the impact of personal experience evident in the very near term fails to persist for days or weeks into the future. Without a clean field test of this relationship, it is difficult to know if personal experience impacts markets. Other limitations characterize extant research as well. Several scholars have argued that much of the existing research that examines how personal experience changes behavior has confounded how information is acquired with the nature of the information that is acquired (e.g., Rakow et al. 2008, Newell and Rakow 2007, Fox and Hadar 2006). For example, the experience of being arrested deters criminals from reoffending (e.g., Smith and Gartin 1989), but it is not clear whether the experience itself (i.e., the personal experience of getting arrested) or new information (e.g., new information about the subjective probability of being caught) deters crime.

In the current work, we explore how personal experience influences decision making and behavior in a field setting, controlling for the effects of learning new information. We examine personal experience with one of the most ubiquitous policy tools—the monetary fine.

We report results from a field setting with approximately 10,000 customers who made video-rental decisions over a two-year period. We test the effects of personal experience with a late fee on future rental behavior. Specifically, we examine how paying a late fee influences how punctual people will be in returning their next rental. We use a semiparametric econometric method to compare the behavior of renters who experience a late fee with those who do not while controlling for individual-specific effects.

Our results indicate that paying a late fee reduces the probability that the customer will pay a late fee on their next visit by 8.8% (1.3% off a base rate of 14%). The deterrent effect of paying a late fee falls to 4.3% on the second visit after paying the fee. This finding documents a sharp decay of the effect of personal experience over time. We also find that the deterrent effect of personal experience with a fine is larger when the fee is more, rather than less, expensive.

In this setting, the late-fee policy is simple and explicit. Furthermore, many of the customers in our sample are very familiar with the policy (they have paid several late fees over our sample period). We conducted a separate set of analyses for individuals who paid many late fees in the past and still found evidence that recent experience with a fine impacts behavior. This enables us to rule out the possibility that our findings are driven by information gleaned from the personal experience.

Our results highlight the limitations of the common approach of providing consumers factual information to influence financial decision making. Merely providing consumers with information about late fees, bankruptcy, or projected retirement savings is likely to impact behavior less than more salient approaches to communicating and experiencing specific consequences. In addition, our results suggest that individuals may be most likely to change their behavior shortly after receiving a fine or penalty.

The remainder of this paper proceeds as follows. Section 2 provides a brief review of the related economic and psychology literatures. Section 3 describes the data used in our analysis and our empirical strategy. Section 4 presents our empirical results. Section 5 provides a discussion and conclusion.

2. Related Literature

The importance of information in changing behavior is well recognized in the economics and the psychology literatures. As a practical matter, information campaigns are often used to change individual decisions and behavior, and an extensive body of research suggests that individuals, as rational actors, will respond to new information. For example, Cutler et al. (2004) found that the introduction of a hospital "report card" system influenced patient decisions; cardiac admissions fell by 10% at hospitals that received a "high mortality" label. Similarly, Jin and Leslie (2003) found that publicizing the hygiene ratings of Los Angeles restaurants led consumers to shift their dining preferences in favor of the most hygienic restaurants.

A surprising number of studies, however, have found that people are often insensitive to information. For example, health workers in Africa claimed that "we could talk about germs until we were blue in the face, and it didn't change behavior" (Duhigg 2008). In a different domain, college administrators tried to curtail alcohol consumption by providing students with new information, but these attempts completely failed to influence drinking behavior (Clapp et al. 2003). Other informational campaigns, ranging from listing nutritional information of food in supermarkets to spreading awareness of the hazards of smoking, have had only modest effects on behavior (McKenna and Williams 1993, Russo et al. 1986).

These discrepant findings regarding the efficacy of providing individuals with information have prompted scholars to investigate conditions under which people are more or less likely to react to new information. For example, Chu and Chu (1990) found that feedback consistency is important in determining whether new information will affect judgments and decisions. Others have considered how social-cognitive factors, such as goals and norms, moderate the influence of new information (e.g., Cialdini 2003, Lampel and Shapira 2001, Kunda 1990). More recent work has begun to consider how the mode of communication moderates the influence of new information.

In practice, people can learn information in several different ways. For example, a driver may learn about the hazards of receiving a speeding ticket by hearing someone tell a story about how she received a fine for speeding (information via description), by witnessing another driver receive a fine for speeding (information via observation), or by actually receiving a fine for speeding (information via personal experience). Each of these sources (description, observation, or personal experience) may convey the same factual information. Although most information studies (e.g., Di Tella and Schargrodsky 2003, Kessler and Levitt 1999) have focused on the informational *content* of the message (e.g., whether or not an individual learns about a speed trap and the prospect of paying a $100 fine), recent work suggests that the mode of communication matters (Simonsohn et al. 2008). In particular, information gained from *experience* may be particularly powerful in influencing judgments and behavior.

Researchers have identified a number of reasons why information gained from personal experience might be particularly powerful. For example, experience-based information may be less abstract compared to information gained through other sources (e.g., Weber et al. 1993, Borgida and Nisbett 1977). A would-be criminal making the decision of whether to commit a crime may understand that they will face a "loss of freedom" (i.e., going to jail) if they are caught, but may not have a concrete understanding of all that a loss of freedom entails (e.g., unable to see friends and family, loss of privacy, etc.) unless they actually experience it firsthand.

In other cases, personal experience may not only convey the same factual information, but may also convey affective information that other sources of information lack (Nisbett and Ross 1980). Although preferences and judgments are significantly influenced by affective reactions (e.g., Odean et al. 2010, Rottenstreich and Hsee 2001, Hsee and Rottenstreich 2004), people often make mistakes when they forecast how they are likely to feel about specific outcomes in the future (Mellers 2000, Loewenstein and Schkade 1999, Gilbert et al. 1998). As a result, the affective information people gain from personal experience is likely to be very different from the affective forecasts people make based on observational or secondhand accounts. For instance, a driver who learns about someone else's speeding ticket may mispredict just how awful she will feel when she receives a speeding ticket of her own.

Recent research has attempted to isolate the effects of personal experience from other types of accounts. Much of this research contrasts the influence of information gained from personal experience with the influence of information gained from a description. This work has found that the informational source matters (e.g., Yechiam and Busemeyer 2005, Weber et al. 2004, Barron and Erev 2003). For instance, Hertwig et al. (2004) found that decision makers overweight small probabilities when they are given the actual probability distribution, but underweight these same probabilities when they gain information about the probability distribution from their own experience. Even when people receive information from multiple sources (e.g., when an outcome is first described, then experienced; Yechiam et al. 2005, Inzana et al. 1996) people tend to place a great deal of weight on their personal experience.

Although a growing body of evidence suggests that personal experience is important, this work has routinely confounded the source of the information with the factual information conveyed (Rakow et al. 2008, Newell and Rakow 2007, Fox and Hadar 2006, see Simonsohn et al. 2008 for an exception). For example, compared to peers who might hear secondhand accounts about street crime, victims of street crime are more likely to engage in actions to prevent future victimization (e.g., Skogan 1987). It is unclear, however, whether the personal experience of the crime is simply more concrete and emotionally charged, or whether it adds factual information as well, such as information about the subjective probability of being a victim. In a closely related paper, Agarwal et al. (2008) find that receiving a credit-card fee one month reduces the chance of paying a fee the following month. This important work, however, does not disentangle the impact of personal experience from the impact of new information. In fact, the authors argue that their effect is driven by consumer learning. In contrast, we are interested in situations in which consumers are not able to learn new information, and we test the impact of recent personal experience on behavior.

3. Data and Empirical Strategy

3.1. Data

We use a data set on video store transactions from a large, independent video store in northern California. The data set includes all transactions made by over 10,000 distinct customers during a two-year period from January 1, 2003, through December 31, 2004.

Each observation involves the set of transactions by an individual on a given day. For each observation, we have the account number, date, type of rental (new release, etc.), rental cost, the amount of money paid to cover a late fee for a past rental, and payment method (credit, cash, check, gift card). Using the account number, we are able to follow the rental behavior for a given individual over the two-year period. We are unable to identify which accounts have multiple users; the added noise with regard to who actually receives the late fee makes for a more conservative test of our hypotheses.

The video store for which we have data classifies movies into two categories: new and old releases. New releases have a one-day rental period; old releases are five-day rentals. Each additional day beyond the rental period for which a movie is not returned is associated with a late fee of $3.00 for new releases and $1.00 for old releases. For each visit to the video store, we observe whether the customer paid money to cover a late fee associated with a previous rental (as opposed to observing which movies were returned late). The policy at this particular video store is that customers are asked to pay any late fees accrued from the previous rental whenever attempting to rent videos. If a customer returns a movie late and rents another movie in the same visit, they are asked at that time to pay the late fee. Thus, we associate paying a late fee in period t with movies

Table 1 Summary Statistics by Individual

	Mean	Std. dev.	Median	Min	Max
Visits (two-year period)	21.40	29.60	9	1	320
Avg movies rented (per visit)	2.30	1.10	2	1	12
Fraction of time movies are returned late	0.14	0.20	0.04	0	1
Late fees paid ($ per visit, conditional on paying a late fee)	4.24	3.34	3.30	1	44
Late fees paid ($, two-year period)	16.50	45.10	2	0	1,335
Total number of customers	10,563	10,563	10,563	10,563	10,563

Notes. Summary statistics represent data from all video-store transactions made between January 1, 2003, and December 31, 2004. A visit represents all transactions that take place on a given day by a customer account number.

returned late in period $t-1$. Occasionally, customers will return a movie late and decide to pay the late fee without renting any additional videos (2.6% of late fees are paid in this manner). Because they did not rent a movie when they paid the late fee, it will be impossible for them to have to pay a late fee during their subsequent visit. This behavior would mechanically provide evidence in favor of a premium placed on personal experience. To address this problem, we drop all observations of visits to the video store in which a late fee was paid but no movie was rented.

Table 1 presents summary statistics for our data. On average, each person in our data set rents 2.3 movies per visit and visits the video store 21 times during the two-year period. The movies are returned late 14% of the time, causing the average individual to pay $16.50 in late fees over the two-year period.

3.2. Empirical Strategy

We specify a simple model for late-fee behavior,

$$Paid\ Fee_{it} = \alpha_i + \gamma\, Paid\ Fee_{it-1} + \mu_{it}, \tag{1}$$

where $Paid\ Fee_{it}$ is an indicator that equals one if individual i paid a late fee during video-store visit t, $Paid\ Fee_{it-1}$ is an indicator that equals one if individual i paid a late fee during her previous video-store visit $(t-1)$, α_i is an unobserved individual-specific effect, and μ_{it} is a random disturbance that is independent and identically distributed (i.i.d.) over time. This model implies that after controlling for the type of each individual and last period's outcome, late fees are determined by transitory shocks. A premium on information gained through experience represents a decrease in the probability of receiving a late fee in the current period because of the experience of a late fee in the previous period. Thus, the hypothesis of greater weight placed on experienced information implies that $\gamma < 0$.

We address two specific questions regarding the model specification. First, are fixed effects needed in this situation, especially considering the increased difficulties they cause in estimation? The video-store data used in this analysis suggest substantial customer heterogeneity in late-fee rates, implying the existence of unobserved individual-specific effects. In the appendix (column (1)), we present the results from the simple regression of $Paid\ Fee_{it}$ on $Paid\ Fee_{it-1}$ using a linear probability model with no fixed effects. As would be expected if unobserved effects were an issue (and contrary to the hypothesis of a premium on personal experience), receiving a late fee during the previous visit increases the chance of paying a late fee during the current visit by 15.4%.

Second, we have assumed μ_{it} to be independent and identically distributed over time as opposed to allowing for serial correlation. Our intuition suggests that after controlling for unobserved individual-specific effects, serial correlation is a minor issue. However, one might imagine that if individuals have certain periods in their life that are particularly busy or relaxed (e.g., holidays), returning videos late may be positively correlated across time. If there is positive serial correlation in our data, we will be underestimating the effect of an experience premium (negative state dependence). To overstate the case of greater weight being placed on experienced information, the less plausible story of negative serial correlation is required. Even negative autocorrelation, however, may be worrisome if, for example, video renters return movies late at certain times of year (e.g., a holiday) and then are not late again until the next holiday. In the results section we discuss this type of behavior in more detail and show that our results are robust to events like holidays.

Econometricians have devoted much attention to the estimation of dynamic linear models with an additive unobserved effect. Ordinarily, a fixed-effects framework would be ideal to control for a situation in which there exists individual heterogeneity. However, because a lagged dependent variable is used as an explanatory variable, including dummy variables for each customer mechanically results in a negative coefficient on the lagged dependent variable (see Nickell 1981). Anderson and Hsiao (1981) were the first to show that the problem with the within estimator can be solved by differencing to eliminate the unobserved effect. Instrumental variables can then be used on the differenced variables to estimate unbiased coefficient values.

Estimating dynamic models with an unobserved effect has proven to be more challenging for nonlinear models such as the case of binary response. No transformation has been found that is able to consistently eliminate the fixed effect in the same way as the Anderson and Hsiao (1981) procedure for the linear case. Thus, two fundamental challenges that arise include assumptions regarding the distribution of the unobserved individual effects and assumptions regarding the initial conditions of the dynamic process (Heckman 1981). In this paper, we use a semiparametric method for estimating dynamic, binary-response models originally proposed by Cox (1958) and Chamberlain (1985) and more recently studied by Honore and Kyriazidou (2000). Unlike random-effects estimators, this fixed-effects method imposes less structure on the estimation. Most notably, it requires no assumptions to be made on the initial conditions of the process or on the distribution of the unobserved effects.

If four or more observation periods are available for each individual, it is possible to identify first-order state dependence while controlling for unobserved effects. Specifically, Cox (1958) showed that if the random disturbances are logistically i.i.d., there exists a set of sufficient statistics $B \equiv \{y_{i1}, y_{iT}, s\}$, where $s = \sum_{t=1}^{T} y_{it}$, that can absorb both the individual effects and the initial conditions. Thus, for the logit model,

$$P(y_{it} \mid \alpha_i, y_{i1}, \ldots, y_{it-1}) = \frac{\exp(\gamma y_{it-1} + \alpha_i)}{1 + \exp(\gamma y_{it-1} + \alpha_i)}, \qquad (2)$$

the following conditional probability can be specified:

$$P(y_{i1}, \ldots, y_{iT} \mid B) = \frac{\exp(\gamma \sum_{t=2}^{T} y_{it} y_{it-1})}{\sum_{d \in B} \exp(\gamma \sum_{t=2}^{T} d_t d_{t-1})}. \qquad (3)$$

Note that the conditional probability does not depend on the parameter, α_i. Furthermore, conditioning on y_{i1} and y_{iT} solves the problems associated with the initial conditions. (Incidentally, controlling for the initial and final conditions also controls for any problems with selective attrition in the sample.)

The intuition for this result is clear. Within a sufficiency class and in the absence of first-order state dependence, we would expect all sequences of events to occur with equal probability. The parameter γ will be estimated to be different than zero when certain sequences occur more frequently in the data than others of the same sufficiency class. For example, when $T = 4$, γ is identified by examining the following pairs of sequences: 1100 versus 1010 and 0011 versus 0101, where 1 represents a late-fee-paid visit and 0 represents a visit with no late fee paid. Notice that the unobserved effects are controlled for because the same number of 1s and 0s occur in each sequence. Furthermore, initial conditions are controlled for by comparing sequences with the same starting and ending values. The only difference between these sequences is the "path" that is taken between the initial and final points. First-order state dependence suggests that 1010 and 0101 will occur more frequently in the data than 1100 and 0011, respectively. An estimate of γ can be obtained by maximizing the sample log-likelihood analog of Equation (3). Similar intuition holds when comparing sequences with more than four observations.

Chamberlain (1985) derives an estimator for second-order state dependence when at least six observation periods are available for each individual. If the random disturbances are logistically i.i.d., the set of sufficient statistics is $B \equiv \{y_{i1}, y_{i2}, y_{iT-1}, y_{iT}, s, s_{11}\}$, where $s_{11} = \sum_{t=1}^{T} y_{it} y_{it-1}$. Thus for the logit model,

$$P(y_{it} \mid \alpha_i, y_{i1}, \ldots, y_{it-1}) = \frac{\exp(\gamma_1 y_{it-1} + \gamma_2 y_{it-2} + \alpha_i)}{1 + \exp(\gamma y_{it-1} + \gamma_2 y_{it-2} + \alpha_i)}, \qquad (4)$$

the following conditional probability can be specified:

$$P(y_{i1}, \ldots, y_{iT} \mid B) = \frac{\exp(\gamma_2 \sum_{t=3}^{T} y_{it} y_{it-2})}{\sum_{d \in B} \exp(\gamma_2 \sum_{t=3}^{T} d_t d_{t-2})}. \qquad (5)$$

It is noteworthy that this conditional probability does not depend on either α_i or γ_1. The intuition for this conditional probability is similar to that described above for testing first-order state dependence. When $T = 6$, the following pairs of sequences give conditional probabilities that contribute to the estimation of γ_2: 101000 versus 100100, 000101 versus 001001, 010111 versus 011011, and 111010 versus 110110. All of these pairs fall within the same sufficiency class and thus control for initial conditions and the unobserved individual-specific effects in the model. Second-order negative state dependence predicts that the second sequence in each of these pairs will occur more frequently in the data than the first.

For our analysis, we generate sequences of six observations so that both first-order and second-order state dependence can be estimated. This data set is created by extracting the first six observations for each movie-rental customer and then continuing to extract the subsequent six observations for each customer provided that six additional observations exist. After obtaining these sequences, the data set is further restricted to include only the 44 sequences of six observations that are useful for the testing of state dependence. This procedure leaves us with 7,650 usable sequences of six observations. These sequences represent movie-rental behavior for 2,735 distinct customers.

In our analysis, bootstrapped standard errors are computed using 1,000 repetitions of the full sample with replacement. All chains of six are assumed to be independent, and all tests are two tailed. Computing standard errors using this bootstrap routine is standard for this methodological approach (see, for example, Chay et al. 1999).

Table 2 Counts of Different Sequence Types Used For Testing First-Order State Dependence

(1)	110000	266	(27)	011100	114
(2)	101000	307	(28)	001110	117
(3)	100100	317	(29)	010110	146
(4)	100010	288	(30)	011010	149
(5)	000011	287	(31)	111100	59
(6)	010001	322	(32)	111010	74
(7)	000101	339	(33)	110110	82
(8)	001001	345	(34)	101110	85
(9)	011000	300	(35)	001111	87
(10)	001100	330	(36)	011101	75
(11)	000110	341	(37)	010111	83
(12)	001010	328	(38)	011011	101
(13)	010010	346			
(14)	010100	347	(39)	100111	71
			(40)	110011	80
(15)	111000	103	(41)	111001	82
(16)	110100	120	(42)	110101	70
(17)	110010	123	(43)	101101	77
(18)	100110	125	(44)	101011	100
(19)	101100	128			
(20)	101010	137			
(21)	000111	123			
(22)	001011	112			
(23)	010011	135			
(24)	011001	137			
(25)	001101	138			
(26)	010101	154		Total no. of sequences:	7,650

Notes. Each sequence type represents six consecutive visits by the same individual. Ones indicate that a late fee was paid during that visit and zeros indicate no late fee was paid. Types (1)–(44) illustrate all sequences of six visits that are usable to test for first-order state dependence. Sequence types are separated into groups ((1)–(4), (5)–(8), etc.) that represent given sufficiency classes. The third and sixth columns provide counts for the number of times the sequence occurs in our data.

Table 2 presents counts for each of the 44 different sequences used to test for first-order state dependence. The sequences are spaced such that each group represents a sufficiency class. Under the null hypothesis of no state dependence, the number of times that each sequence appears in the data should be statistically equivalent to all other sequences in the same sufficiency class. A comparison of the counts for sequences within a sufficiency class suggests that negative state dependence is present in these data.

4. Results

We hypothesized that personal experience would have a larger effect on rental behavior than would other sources of information and test this prediction by maximizing the sample log likelihood analog of Equation (3) with respect to γ using the 7,650 usable sequences. The estimate of first-order state dependence provides support for our hypothesis (see Table 3, column (1); $\gamma = -0.1067$, $p < 0.01$). This coefficient represents a log-odds estimate (logit estimate). We can interpret this coefficient as a marginal effect for each possible value of α. For an α that is approximately equal to the average α for the entire sample, this coefficient suggests that an individual is 1.3% (in absolute terms) less likely to pay a late fee during a visit if a late fee was paid during the last visit.[1] This represents an 8.8% reduction from mean late-fee rate of 14%. These results are smaller, yet similar, to those from a linear regression analysis (see the appendix).

Previous research suggests that the effects of personal experience decay over time. In Table 3 (column (2)), we report estimates of second-order state dependence using the 1,648 sequences that include sets of rentals involving a late return followed by an on-time return. Our estimate, $\gamma_2 = -0.0510$, suggests that having paid a late fee two visits ago decreases the probability of paying a late fee during the current visit by 0.6% (4.3% reduction from the base rate of 14%). However, given the reduced sample size for testing second-order state dependence, this effect is not significantly different from zero ($p = 0.27$).

A potential concern with the results in column (1) is the possibility that negative autocorrelation may occur for reasons other than a behavioral response of having just experienced a late fee. For example, it is possible that renters only return movies late on holidays. This could naturally create negative state dependence. To investigate this possibility, in column (3) of Table 3 we restrict the sample to times of year when long vacations are less common. Specifically, we focus on chains of six that did not include video rentals between December 15 and January 15 and also did not include the months of June, July, and August. In this manner, we are able to see if our results are robust to times during the year that are less likely to include vacation travel. The coefficient estimate from column (3) indicates that the effect remains statistically significant and, if anything, is slightly larger than our baseline estimate.

An important question regarding experience-based choice relates to the magnitude of the experience. Specifically, we investigate whether or not more significant experiences have a larger marginal effect on future decisions and behaviors than do more minor experiences. In columns (4) and (5) of Table 3, we attempt to address this issue. In column (4), we reduce the sample to sequences for which the first late fee in the sequence was \$1–\$3 (usually caused by returning one movie past the deadline by one day). Column (5), on the other hand, reduces the sample to

[1] The method that we use for estimating the dynamic binary-choice model matches on α, and therefore does not estimate α and γ jointly. Thus, to provide a approximate marginal effect for ease of interpretation, we assume an alpha of -1.8, which yields a base late-fee rate of 0.14 (the average late-fee rate in our sample) when the previous visit did not include a late fee.

Table 3 Fixed-Effects Estimates of State Dependence Based on Semiparametric Conditional Logit Models

	Dependent variable: Paid fee in period (t)				
	(1)	(2)	(3)	(4)	(5)
Implied marginal effect	−0.013	−0.006	−0.016	−0.009	−0.015
Paid fee ($t-1$)	−0.1067		−0.1352	−0.0775	−0.1313
	(0.0237)**		(0.0424)**	(0.0416)*	(0.0499)**
Paid fee ($t-2$)		−0.0510			
		(0.0464)			
Seasonally restricted sample			X		
First of two paid fees \$1–\$3				X	
First of two paid fees > \$3					X
Total no. observations	45,900	9,888	13,998	16,614	9,216
Total no. chains of six	7,650	1,648	2,333	2,769	1,536

Notes. Columns (1)–(5) provide maximum likelihood estimates of state dependence using the conditional log-likelihood functions given in Equations (3) and (5). Equation (3) represents first-order state dependence, and Equation (5) represents second-order state dependence. Standard errors are computed using a bootstrap routine with 1,000 repetitions of full samples with replacement. Column (3) uses the subset of sequences that did not involve video rentals between December 15 and January 15 or the months of June to September. Column (4) uses the subset of sequences that have exactly two late fees and where the first late fee paid is between \$1 and \$3. Column (5) uses the subset of sequences that have exactly two late fees and where the first late fee paid is greater than \$3. The implied marginal effect for each log-odds estimate is provided in each column.

*Significant at 10%; **significant at 1%.

sequences for which the first late fee in the sequence was greater than \$3 (usually caused by returning one movie past the deadline by more than one day or returning multiple movies late).

We further restrict the samples in columns (4) and (5) to be sequences of types (1)–(14) in Table 2. These are sequences of six observations for which there were two late fees. The reason for this restriction is that sequences in other sufficiency classes that test for first-order state dependence (e.g., 111000 versus 110100) may not depend on the late-fee amount in the first period. The sequences with exactly two late fees, however, all rely on the amount of the first late fee in the sequence and its effect on deterring subsequent late-fee behavior.

The average and median late fees paid in our data conditional on the paid late fee being greater than \$3 are \$8.24 and \$6, respectively. Thus, the fines incurred by individuals whose data are used in column (5) are oftentimes several times larger than those of individuals whose data are used in column (4). The results indicate that the experience premium of late fees greater than \$3 is almost twice as large ($\gamma = -0.1313$) as the experience premium of late fees between \$1 and \$3 ($\gamma = -0.0775$).

One alternative explanation for these results is that some renters may not be fully aware of the rental store policies, and are therefore learning new factual information when they first receive a late fee. To test this alternative explanation, we conducted separate analyses on populations with different rental histories. Specifically, we conducted analyses on customers who had previously rented at least 10, 20, and 40 times, respectively. We report results from these analyses in Table 4. We estimate the level of first-order negative state dependence in the data. Our results indicate that experience-based behavior is just as strong (if not stronger) for customers with long histories than it is for customers with short histories. As an even more conservative test of our primary thesis, we estimated the effects of personal experience for renters who had previously paid at least 2, 4, or 10 late fees (Table 4, columns (4)–(6)). Notably, we find the same first-order effects for experience with a fine for customers who had paid a fine in the past. Thus, we find that experience with a fine influenced both seasoned and naïve renters alike.

5. Discussion and Conclusion

Personal experience changes behavior. Using a unique field setting and longitudinal data, we show that the personal experience of paying a late fee decreases the likelihood that customers will incur a late fee during their next rental period. Larger fines lead to greater behavioral effects than smaller fines, and the influence of experience with a fine decays quickly over time. Surprisingly, personal experience affected the behavior of seasoned and novice renters alike. This was true even for customers who had previously paid fines. This provides powerful evidence in support of our thesis: the influence of personal experience extends beyond the factual information it conveys.

A number of scholars have developed models of learning and decision making in a repeat choice context. In the instance-based learning model, Gonzalez et al. (2003) describe how individuals shift from

Table 4 Estimating the Effects of Experience on First-Order State Dependence

	Dependent variable: paid fee in period (t)							
	Number of previous visits			Number of previous late fees				
	>10	>20	>40	>2	>5	>10	First half	Second half
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Implied marginal effect	−0.0180	−0.0140	−0.0250	−0.0130	−0.0210	−0.0190	−0.0170	−0.0130
Paid fee ($t-1$)	−0.1540	−0.1238	−0.2227	−0.1127	−0.1803	−0.1674	−0.1493	−0.1118
	(0.0281)***	(0.0327)***	(0.0445)***	(0.0284)***	(0.0333)***	(0.0411)***	(0.0398)***	(0.0386)***
Log likelihood	−13,451	−9,859	−5,456	−13,620	−9,736	−6,010	−7,131	−7,157
Total no. observations	33,042	24,300	13,446	33,690	24,078	14,784	17,580	17,580
Total no. chains of six	5,507	4,050	2,241	5,615	4,013	2,464	2,930	2,930

Notes. Columns (1)–(8) provide maximum likelihood estimates of state dependence using the conditional log-likelihood functions given in Equation (9) in the text. Standard errors are computed using a bootstrap routine with 1,000 repetitions of full samples with replacement. Columns (1)–(3) restrict the sample by not creating sequences of six observations for each individual until the first 10, 20, and 40 visits to the video store have been deleted, respectively. Columns (4)–(6) restrict the sample by not creating sequences of six observations until the individual has paid 2, 5, and 10 late fees, respectively. Column (7) restricts the sample by only including the first half of sequences for any individual. Column (8) restricts the sample by only including the second half of sequences for any individual. In the event of an odd number of sequences for a given individual, the last sequence is deleted. The implied marginal effect for each log-odds estimate is provided in each column.

***Significant at 0.1%.

heuristic to retrieval-based decision making as they gain experience within a domain. In the experience-weighted attraction (EWA) learning model, Camerer and Ho (1999) capture the combined influence of experience and beliefs about what could happen (or could have happened) in guiding decisions. Several experimental studies have explored learning in repeat choice contexts (Roth and Erev 1995, Erev and Roth 1998, Gonzalez et al. 2003, Camerer and Ho 1999). Some of these findings are very consistent with our results. For example, experimental tests of the EWA model suggest that information gained through experience impacts decision making more than information gained from other informational sources (Camerer and Ho 1999; see also Ho et al. 2008). Overall, however, there is a surprising lack of field research that has studied learning in repeat choice contexts. As Gonzalez et al. (2003, p. 595) admit, there is a "lack of real world validation" for repeat choice models such as instance-based learning. As a result, we know little about issues such as how learning decays over time periods longer than the few hours that characterize experimental lab sessions.

Given the reliance on laboratory research in testing models of experience-based choice, an important strength of the current research is the longitudinal nature of our data. In addition to the benefits of controlling for individual-specific effects, examining the effects of personal experience over time enables us to conduct a very conservative test of the influence of personal experience on behavior. In contrast to findings from laboratory experiments, we demonstrate that personal experience can affect behavior days or even weeks into the future. In light of the conservative nature of our tests, the effects of personal experience on behavior are quite robust.

Our findings also offer empirical support for the recency effect. Experimental research has found that individuals often choose strategies that are a best reply to their recent experience (Erev and Haruvy 2011, Schweitzer and Cachon 2000) and that individuals are particularly likely to change their behavior following a loss (Bereby-Meyer and Erev 1998). The recency effect may help to explain two key aspects of our results. First, we find that rental customers who paid a fine were particularly likely to change their behavior and return their next movie on time. Second, after returning movies on time, the effect of having paid a fine in the past decayed, and customers returned to their prior likelihood of returning a movie late. Notably, these findings are consistent with the "forgetting" parameter in the EWA model that emphasizes recent outcomes (Camerer and Ho 1999).

Our results inform a number of practical prescriptions. Across many domains, managers use fines to gain compliance. For example, some managers impose fines to curtail smoking at work and even encourage healthy behaviors outside of work by fining employees who fail to meet specific health criteria (Costello 2007). Our findings suggest that following a personal experience with a fine, employees will be particularly likely to comply with the desired behavior. In fact, policies that regularly impose small fines may be particularly effective in gaining compliance.

In other cases, managers may wish to minimize the salience of fees they charge. Many businesses, such as credit-card companies, rely on fees as an important source of income. These businesses may wish to implement policies such as automatic withdrawal or prepaid late-fee accounts to minimize customer dissatisfaction and increase customer retention. Balancing the costs and benefits of imposing fees on customers is a decision managers need to make as they balance the risk of alienating customers with the benefits of gaining compliance (Shapira 1995).

Our findings also inform prescriptions for public policy. For example, policymakers may be able to deter crime not only by adjusting punishment levels and detection rates, but also by changing the personal experience of potential criminals. Rather than giving a juvenile caught vandalizing a warning, an officer might be able to deter future crime more effectively by meting out a punishment that involves a personal experience (e.g., briefly handcuffing the offender).

When it comes to motivating individuals, personal experience offers a unique vehicle for changing behavior. Importantly, personal experience influences seasoned individuals with prior experience. Though we found that compliance effects decay over time, personal experience with a fine can motivate long-term behavior. In some cases, the influence of these changes can be profound. Just ask Reed Hastings and his competitors at Blockbuster.

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Appendix. Estimates of State Dependence Based on the Linear Probability Model

	Linear probability model				IV (LPM)
	(1)	(2)	(3)	(4)	(5)
Late fee paid ($t-1$)	0.154 (0.003)***	–0.023 (0.003)***	–0.023 (0.004)***	–0.027 (0.005)***	–0.018 (0.004)***
Fraction late ($t-1$, $t-10$)		0.693 (0.007)***			
Fraction late ($t-1$, $t-25$)			0.858 (0.009)***		
Fraction late ($t-1$, t–50)				0.926 (0.014)***	
Adj. R^2	0.024	0.108	0.129	0.129	0.018
Observations	215,216	154,337	96,037	46,253	206,263

Notes. The dependent variable in columns (1)–(4) is an indicator that equals one if the customer paid a late fee during that visit. Robust standard errors are presented in parentheses. *Fraction late* ($t-1$, $t-X$) is a variable that equals the fraction of time that the customer paid a late fee in the previous X visits. Column (5) uses the Anderson–Hsiao method with the dependent variable being the difference between the late-fee-paid indicator in period t and the late-fee-paid indicator in period $t-1$. The *late fee paid* ($t-1$) difference is instrumented with *late fee paid* ($t-2$).

***Significant at 0.1%.

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