CAHIER D'ÉTUDES
WORKING PAPER

N° 60

SUBPRIME CONSUMER CREDIT DEMAND: EVIDENCE FROM A LENDER'S PRICING EXPERIMENT

Sule ALAN, Ruxandra DUMITRESCU and Gyongyi LORANTH

FEBRUARY 2011

Conference on Household Finance and Consumption
Luxembourg 25-26 October 2010

BANQUE CENTRALE DU LUXEMBOURG
EUROSYSTÈME

Conference on "Household Finance and Consumption"

This paper was presented at the conference on "Household Finance and Consumption", which was co-organised by the Banque centrale du Luxembourg and the ECB, and was held on 25-26 October 2010 in Luxembourg. The organising committee consisted of Michael Ehrmann (ECB), Michalis Haliassos (CFS and Goethe University), Thomas Mathä (Banque centrale du Luxembourg), Peter Tufano (Harvard Business School), and Caroline Willeke (ECB). The conference programme, including papers, can be found at http://www.ecb.europa.eu/events/conferences/html/joint_ecb_lux.en.html. The views expressed in this paper are those of the authors and do not necessarily reflect those of the Banque centrale du Luxembourg, the ECB or the Eurosystem. Financial support from the Fonds National de la Recherche Luxembourg (FNR) is gratefully acknowledged.

Subprime Consumer Credit Demand: Evidence from a Lender's Pricing Experiment[1]

Sule Alan[2]
Faculty of Economics and CFAP
University of Cambridge
UK

Ruxandra Dumitrescu
Faculty of Economics and CFAP
University of Cambridge
UK

Gyongyi Loranth
Faculty of Business, Economics and Statistics
University of Vienna and CEPR

January 2011

[1]We are grateful to the lender for generously providing us with the data. We especially thank the lender's database managers who volunteered a great deal of assistance. We thank Orazio Attanasio, James Banks, Martin Browning, Thomas Crossley, Mark Jenkins, Soren Leth-Petersen, Valerie Lechene, Hamish Low, Nick Souleles, Philip Vermeulen and participants of the Cambridge-Wharton conference, BCL-ECB conference and seminar participants at Cambridge University, Koc University, University of Copenhagen, the Institute for Fiscal Studies (IFS) and Bundesbank-ECB for comments and suggestions. This research is funded by the Cambridge Center for Financial Analysis and Policy (CFAP) and the University of Vienna.

[2]Corresponding author: sa479@cam.ac.uk, phone: +44 1223 335 283.

Abstract

We test the interest rate sensitivity of subprime credit card borrowers using a unique panel data set from a UK credit card company. What is novel about our contribution is that we were given details of a randomized interest rate experiment conducted by the lender between October 2006 and January 2007. We find that individuals who tend to utilize their credit limits fully do not reduce their demand for credit when subject to increases in interest rates as high as 3 percentage points. This finding is naturally interpreted as evidence of binding liquidity constraints. We also demonstrate the importance of truly exogenous variation in interest rates when estimating credit demand elasticities. We show that estimating a standard credit demand equation with nonexperimental variation leads to seriously biased estimates even when conditioning on a rich set of controls and individual fixed effects. In particular, this procedure results in a large and statistically significant 3-month elasticity of credit card debt with respect to interest rates even though the experimental estimate of the same elasticity is neither economically nor statistically different from zero.

Keywords: subprime credit; randomized trials; liquidity constraints.

JEL Classification: D11, D12, D14

1 Introduction

Borrowing rates affect firms' and households' demand for credit. Quantifying such effects, i.e., estimating credit demand elasticities, has become an increasingly important academic endeavour. At the micro level, lenders are interested in gauging these elasticities as an input to their optimal loan pricing strategies. At the macro level, knowledge of these elasticities is essential for the conduct of monetary policy. Moreover, they can be informative regarding whether households are credit constrained. This is of course important given policy concern with the finances of poor and vulnerable households.

In estimating the sensitivity of credit demand to borrowing rates, the major difficulty faced by researchers is that genuinely exogenous variation in borrowing rates is rarely observed. For example, the observed cross sectional variation in interest rates is likely to be endogenous to borrowing and repayment behavior through unobservable characteristics of the borrowers. Researchers try to overcome this problem by using quasi-experimental designs. Attanasio et al (2008) estimate interest rate elasticities of car loan demand exploiting the tax reform of 1986 in the US. Alessie et al (2005) analyze the same issue using a similar design. Gross and Souleles (2002) use the US Credit Bureau data and exploit some firm-specific practices to instrument borrowing rates. Adams et al (2007) use data on a US private subprime auto loan company. The general conclusion drawn from the studies is that there seems to be no sensitivity to borrowing rates among low income households. However, such households display some sensitivity to loan features related to liquidity, such as down payment requirements, credit limits and loan maturities. This finding is interpreted as the presence of binding liquidity constraints[1]. All the studies mentioned above rely on identifying assumptions and may be subject to criticism.

Similar to the literature cited above, we estimate the sensitivity of credit demand to interest rates. For this, we use a unique panel data set on detailed credit card transactions from a private

[1] The exception is the Gross and Souleles (2002) study where the authors find evidence of significant elasticity of credit card debt with respect to interest rates.

credit card company that exclusively serves the subprime market in the United Kingdom[2]. What is novel about our study is that our lender varied interest rates applied to revolving debt via randomized experiments. One of these experiments, designed in July 2006 and implemented in October 2006, has been made available to us. Therefore we have the opportunity to estimate the effect of truly exogenous changes in borrowing rates on the demand for credit card debt. To our knowledge, this paper is the first to estimate interest rate sensitivity of credit demand in a developed market using a randomized interest rate experiment.

Karlan and Zinman (2007) is the only previous paper we are aware of to use experimental variation in interest rates to estimate the interest rate sensitivity of credit demand. Karlan and Zinman show that the demand for new term loans in South Africa exhibits modest interest rate sensitivity, with demand apparently more sensitive to loan maturity (that is, minimum payments). Our analysis differs from Karlan and Zinman in a number of key respects. First, we study the interest rate sensitivity of credit card debt, rather than new term loans. Second, we study a developed economy (the UK) with a highly sophisticated credit market. Lenders in countries like the UK have access to advanced risk pricing technologies. For this reason, the estimates of credit elasticities and evidence of market failure in a developing country is not directly relevant for highly developed countries like the UK, even in a subprime market. Thus our analysis provides novel evidence on the functioning of subprime lending markets in developed economies, on the prevalence of credit constraints in such economies, and on the role of credit card debt in consumption smoothing among subprime consumers in such economies. This evidence is critical to the conduct of monetary policy and the development of public policy with respect to household finance in the UK and other similarly developed economies.

Besides the availability of experimental variation in borrowing rates, the distinct characteristics of subprime borrowers such as low income and impaired credit history make our sample

[2]For confidentiality reasons, we do not disclose the name of the company. We will refer to it as the "lender" from here on.

an interesting ground to estimate the sensitivity to borrowing rates. For one thing, we can comfortably assume that individuals in our data set are likely to be net borrowers. A borrower who is currently not liquidity constrained is expected to lower his consumption via either lowering his purchases on credit or making more payments toward his balance when faced with an increase in interest rates[3]. On the other hand, a borrower who is constrained by his credit limit is not likely to change his consumption following a (small) interest rate change. Hence, if we define monthly new credit card borrowing as monthly purchases on credit minus the payments made toward the outstanding balance (this is the monthly addition to the existing credit card debt that accrues interest), we expect to see no change in new credit card borrowing for such individuals[4].

Our lender carried out the experiment using a block design where a randomly selected sample of accounts were assigned to 9 cells based on their utilization rates (monthly balance divided by credit limit) and an internally developed behavior score that summarizes individuals' risk characteristics (following the practice of the lender, we will refer to these blocks as 'cells'). This design is motivated by the lender's risk pricing practice but it serves our purposes well as the ex-ante conditioning on observables yields more precisely estimated average treatment effects[5]. After allocating the experiment sample into 9 cells in July 2006, the lender randomly selected a number of individuals in each cell and allocated them into control groups. The treatment groups (remaining individuals) in each cell received interest rate increases of 3 or 1 percentage points in October 2006 depending on the cell they were in July 2006. The lender lowered the interest rate of one cell's treatment group by 3 percentage points. We are able to observe the individuals for 3 months after the interest rate changes, October, November and December 2006. Since

[3]The prediction may be different for interest rate reductions. A prudent borrower may not be responsive to a (small) decrease in interest rates as he takes into account that liqudity constraints, even though not binding currently, may bind in the future.

[4]Note that the latter prediction refers to the strongest definition of liquidity constraints where there is an actual quantity limit to borrowing. One can also extend the notion of liquidity constraint to individuals who face increasing borrowing cost with quantity demanded as in Pissarides (1978).

[5]See Duflo et al (2006).

we were not involved in the design or the implementation of this experiment, we first employ several tests to establish whether our lender performed randomization properly. After multiple tests, we conclude that our lender carried out randomization properly.

First, we estimate the average treatment effects of interest rate changes on new credit card borrowing for each cell. We then estimate average treatment effects by month. We also estimate average treatment effects by conditioning on a number of borrower characteristics that are, by design, independent of the treatment, such as age, income and whether the individual has any other credit card. We find that individuals who utilize their credit limits fully are insensitive to purely exogenous increases in interest rates as high as 3 percentage points. We interpret this finding as evidence of binding liquidity constraints among this group.

The nonexperimental literature cited above has strived to find good instruments for borrowing rates. In this paper, we also illustrate the importance of this endeavour. More specifically, we show the importance of exploiting only the exogenous variation in interest rates when estimating borrowing demand equations. Overall interest rate variation in our sample includes pre-treatment cross sectional variation within cells and cross cell variation, which are undoubtedly endogenous, as well as exogenous within cell variation generated by the experiment. Since we have full information on the way in which interest rates changed in October 2006, we can assess the degree of bias caused by the endogeneity. We find a statistically significant debt reduction of £24 (implying -1.05 elasticity) over the period of 3 months in the case of a 1 percentage point increase in interest rates when we ignore the lender's experiment. When we estimate the demand equation using the experimental variation in interest rates, we find that the same elasticity is neither economically nor statistically different from zero. The estimated sensitivity to interest rates using the nonexperimental data is quite misleading and hides the fact that subprime credit card borrowers *do not* decummulate debt in the face of increasing borrowing costs.

The rest of the paper is organized as follows. We provide a brief overview of the UK credit card market in the next section. In Section 3, we present a simple life cycle model to motivate the choice of our outcome variable. Data and the experimental design are explained in detail in section 4. We assess the lender's experiment to pin down the expected statistical and economic effects in Section 5. We present and discuss the experimental and nonexperimental estimates in section 6. Section 7 concludes.

2 Subprime Credit Card Market in the UK

Credit cards have steadily grown in importance as a payment device in all industrialized countries. As of 2007, it is estimated that approximately 70 million credit cards were in issue in the UK (see Data Monitor Report (2008)). Moreover, borrowing on credit cards (revolving credit card debt from one month to the next, therefore incurring interest charges) grew rapidly over the last few decades in the UK, attracting much attention from consumer protection groups, regulatory bodies and, of course, the media. In 2007 total credit card debt stood at around £65 billion, representing approximately 30% of consumer credit in the UK.

Consumers who are not considered suitable for unsecured credit by the mainstream issuers comprise the UK "nonstandard" credit card market. By definition, individuals deemed to be nonstandard borrowers are more difficult to evaluate in terms of default risk. This can be due to volatile income (many self-employed), low income (unemployed), the lack of credit history in the UK, or impaired credit history due to past defaults or mortgage arrears. Approximately 7 million individuals in the UK fall into this category, and they are in possession of approximately 6 million nonstandard credit cards as of 2007 (8.6% of total credit cards in issue)[6]. The average member of the nonstandard population has 0.85 cards whereas the average number of cards held by the prime segment is 1.5. The most distinctive feature of a nonstandard credit card is the high interest charged for the revolving debt. The rate is typically around 30-40%, with the

[6]Reasons to fall into the non-standard catagory are: absence of a bank account, unemployment, being an income support claimant, CCJs record, mortgage arrears and repossesions record, bankruptcy record and being a self employed with less than three years' proof of income.

highest observed rate around 70% [7]. A typical nonstandard borrower usually starts with a very small credit limit like £150 and the credit limit generally remains around £500[8].

The term "subprime" refers to a subsection of the nonstandard market in the UK. This subsection usually comprises individuals with adverse credit histories i.e., individuals with an even higher risk of default than the typical nonstandard individual. Therefore, issuers who target this segment exclusively (such as our lender) invest heavily in advanced risk based pricing practices to combat the adverse effect of delinquencies and bankruptcies. Our lender serves the "subprime" segment and targets self employed individuals with low income and individuals who are affected by County Court Judgements (CCJs)[9]. The presence of CCJs, in general, is the most common reason to fall into the subprime category. As of 2007, the number of credit cards held by individuals with a CCJ was approximately 2.9 million. The second most common reason is being self-employed (1.3 million cards).

3 Theoretical Framework

In this section we lay out a simple dynamic model of consumption tailored for individuals in our data set. The exposition is intended to determine a meaningful choice variable for credit card borrowing. This variable is then used as an outcome variable to estimate interest rate sensitivity.

Assume that the generic individual is a lifetime utility maximizer with a time separable utility function. Assume further that the only tool available to him to implement his desired consumption profile is credit card borrowing. His problem can be written as a two period

[7] A policy of interest rate ceilings for credit has not been adopted in the UK. Such policies, although debated, are considered conterproductive as they may drive vulnarable consumers such as those with low income and/or limited credit history into illegal credit markets.

[8] To provide a comparison, the interest rate applied to a typical mainstream card is around 15-18% with a credit limit of £2000 and above.

[9] County Court Judgement refers to an adverse ruling of the County Court against a person who has not satisfied debt payments with their creditors. An adverse ruling remains on the individual's record for six years from the date of judgement. CCJs are the attribute most comonly associated with subprime individuals in the UK. Unfortunately we do not have information on whether an individual has a CCJ or not in our data set.

problem in the usual way:

$$MaxU(C_t) + \beta E_t[V_{t+1}(D_{t+1})] \tag{1}$$

where C_t is consumption in period t, β is a subjective discount factor ($\beta = 1/(1+\delta)$ where δ is the rate of time preference), D_t is the credit card debt (equivalent to negative assets) for period t. $E_t[V_{t+1}(D_{t+1})]$ is the expected future value of debt. Assuming monthly periods, the state variable debt evolves as follows

$$D_{t+1} = (1 + r_{t+1})D_t + NT_{t,t+1} - P_{t+1} \tag{2}$$

where r_{t+1} is the interest rate applied to debt revolved from month t, $NT_{t,t+1}$ is new purchases made on credit between period t and $t+1$, P_{t+1} is the payment made for the balance of period $t+1$. Notice that $NT_{t,t+1}$ is interest exempt between period t and $t+1$[10]. Define 'net new borrowing' NNB_{t+1} as new monthly purchases minus the payment made toward the total outstanding balance:

$$NNB_{t+1} = NT_{t,t+1} - P_{t+1} \tag{3}$$

If $NT_{t,t+1} - P_{t+1} > 0$, the difference accrues interest charges until paid.

For most credit card products, monthly payment P_t is subject to

$$P_t \geqslant Max[\kappa B_t, \theta] \tag{4}$$

where B_t is the statement balance (interest accrued debt plus new purchases), κ is the fraction used to calculate required minimum payment, and θ is a known amount to be paid if $\kappa B_t < \theta$.

[10]In fact, it is interest exempt until the payment due date which falls between $t+1$ and $t+2$.

For example, the value of κ for our lender is

$$\begin{aligned} \kappa &= 3\% \text{ of monthly balance} \\ \theta &= £5 \end{aligned}$$

Note that having a credit card means that the individual is pre-approved for a loan subject to a given credit limit. Therefore, our analysis should be based on internal rather than external margin. Another important feature of credit card debt is that changes in interest rates apply to all existing debt. Hence, when faced with an increase in interest rate, individual's debt and required minimum payment automatically increase due to the additional interest charges. For these reasons, for a given month t, the actual choice variable for the individual is the net new borrowing NNB_t[11]. An individual who is a net borrower is expected to lower his net new borrowing when faced by an increase in the borrowing rate if he were not severely liquidity constrained in the first place. He will do so by either reducing his new monthly purchases on credit, NT, or by increasing his monthly payments, P (or both). It is clear that either action means a reduction in consumption for the borrower. For such an individual monthly consumption can be described as

$$C_{t,t+1} = Y_t + NT_{t,t+1} - P_{t+1} \tag{5}$$

where Y_t is monthly income (likely to be stochastic). We expect however, no sensitivity of NNB_t to increases in interest rates for the individuals who are constrained by their credit limits. In what follows we implement our empirical strategy using NNB_t as our outcome variable.

[11]Note that the payment variable is $P_{t+1} = \kappa B_t + DP_{t+1}$ where κB_t is the required minimum payment that is determined by the statement balance and therefore interest rate sensitive, DP_{t+1} can be called the'discretionary payment' made over and above the minimum payment required. In the case that the payment constraint in equation (4) binds, $NT_{t,t+1} - DP_{t+1}$ is the correct choice variable. For what follows however, we will use P_{t+1} instead of DP_{t+1} to construct NNB_{t+1} as even a 3 percentage point increase in interest rates creates economically too small an increase in the minimum payment requirement. Morever, DP_{t+1}(our construction) is a noisier variable than P_{t+1} (given us by the lender).

4 Data and Experimental Design

Our data set is provided to us by a private credit card issuer which operates in the subprime segment of the UK market. It specifically targets self employed individuals with low income and individuals who are affected by County Court Judgements (CCJs). For confidentiality reasons, the limited number of nonstandard credit card issuers in the UK prevents us from giving the exact market share of our lender. Nevertheless, we can say that it is one of the major players in the subprime market. It has several credit card products all with conditions typically observed in subprime markets such as high interest rates and low credit limits. The data set comprises all individual transactions including purchases, payments and interest charges, as well as minimum payment requirements. We also have income, age and marital status reported by individuals at the application stage. Unfortunately, we do not have information about individuals' other credit commitments such as mortgages and other consumer loans.

Since 2006, the lender has routinely performed randomized interest rate experiments on subsamples of their clients. The company further informed us that they only raised (or lowered) interest rates on individual accounts via controlled experiments, not in any other fashion. Each experiment lasted around 3-6 months and the lender initiated another experiment immediately following the previous one. Interest rate changes were permanent until the next change took effect. The proportion of individuals allocated to control groups became increasingly smaller with each new experiment. All interest rate experiments were designed based on ex-ante determined blocks which we will explain in greater detail below.

The lender agreed to provide us with one of the experiments that was designed in July 2006 and implemented in October 2006, involving 18,900 individuals. In January 2007, another experiment was implemented with 27,000 individuals and some of the individuals in our experiment were included in the next experiment. Therefore, the effect of interest rate changes can be cleanly measured only over the three months following the implementation of the experiment.

The experimental sample was not chosen from the lender's full clientele base. Accounts that are flagged for reasons such as default, several months of delinquency or inactivity are excluded before the selection of the sample. Furthermore, the lender excluded individuals who have been with the lender for less than seven months at the time of the design (July 2006). For the experiment we have, all of this resulted in the exclusion of approximately 40% of the accounts. Table 1 presents the characteristics of the individuals in our sample. Values are calculated for the month in which individuals were assigned to treatment and control groups (July 2006).

The average individual in our sample is 41 years of age. Median income reported at the application stage is £15,000. Given the median individual income for the UK is about £19,000, individuals in our sample represent the lower end of the income distribution. Approximately 60% of the individuals report that they are employed, and approximately 35% of them report that they own their residence. Some useful information we will use later is that about 40% of the individuals in our sample do not own any credit card other than the one issued by our lender.

The average monthly utilization rate, defined as outstanding monthly balance divided by the credit limit, is about 73% with the median value of 90%. The average utilization rate for all UK credit card borrowers is approximately 34% (see BIS (2010)). Two other statistics highlighting the differences between our average borrower versus the average UK borrower are the interest rates and credit limits. The mean (median) interest rate is 31.8% pa (32.9% pa) (note that this is the situation as at July 2006, thus before the implementation of the experiment). These interest rates are significantly higher than the rates on typical UK credit cards (approximately 15-18% pa).

The mean (median) credit limit is £1,080 (£950), much lower than the average UK credit card limit of £5,129 in 2007. During the sample period, the lender changed credit limits for both treatment and control (independent of the treatment status as we established through our statistical analysis). Between July 2006 and December 2006 approximately 52% of our sample received no credit limit change, 38% received a change once, and approximately 10% received a change twice. It is worth noting that the lender very rarely lowered the credit limit; only 2.3% of all credit limit changes (involving only 198 individuals) within the sample period were negative.

As Table 1 shows, the average monthly purchase value is about £77 with the median value of £0. It is worth drawing attention to the size of revolving debt in the table. This figure is calculated as the individual's balance appeared on the June 2006 statement minus the payments made by the due date applied to that balance[12]. Therefore it is the actual revolving debt that the interest charge is applied to. The mean revolving debt as at July 2006 is approximately £650 with the median value being £552. This is quite a large figure considering a monthly interest rate of about 2.5% [13]. It is clear that a significant portion of the individuals in our data set use their card for borrowing purposes. To be precise, approximately 81% of the individuals in our sample revolved debt every month between the period of July 2006 and December 2006.

Perhaps the most intriguing feature of our data is that the lender changed its clients' interest rates only through randomized trials since 2006, not in any other fashion. They carried out the randomization as a block design where a sample of individuals were assigned to cells defined by the interaction of utilization rates and internally developed behavior scores that summarize individuals' risk characteristics[14]. Individuals were allocated into cells according to their utilization rates and behavior scores as at July 2006. After the allocation, the randomization was performed within cells. Table 2 presents the cell design, the type of treatment received and the sample sizes of each cell[15]. For example, cell 1 contains individuals who had high utilization

[12] Our data set contains information based on the statement cycle as well as based on calendar month. Therefore we are able to calculate the debt variable accurately.

[13] Monthly interest charged on £650 of revolving debt that is subject to 30% interest rate would be approximately £16.

[14] Internally developed credit scoring systems are general practice for credit card issuer. We do not know the exact features of our lender's scoring system but we were informed that it is a continously updated multivariate probit type algorithm.

[15] After the actual randomization, the lender added a small number of extra individuals to cells 1,2 and 3 to be treated and this made the treated group different from the control. Fortunately, we have the specific identifier for these individuals and we exclude them from our analysis.

rates and low behavior scores (high default risk) in July 2006. In this cell, 337 individuals received a 3 percentage point increase in interest rates while 413 individuals were in the control group. Similarly, cell 9 contains individuals who had low utilization rates and high behavior score (low default risk) in July 2006. In this cell, 499 individuals received a 3 percentage points reduction in interest rates while 1424 individuals were in the control group. For cell 6, the lender did not allocate any individual to a control group, making the cell unavailable for our purposes. Our private conversations with the lender suggest that selection ratios are based on profitability concerns rather than statistical power concerns.

As can be seen from the cell design, the treatment is not homogenous across cells; cells with low behavior scores (cells 1, 2 and 3) received a 3 percentage point increase in interest rates whereas cells 4, 5, 7 and 8 received a 1 percentage point increase. Note also that the cross sectional distribution of interest rates prior to the implementation did not differ across cells. It is clear from this design that we cannot estimate the overall average treatment effect for the entire sample. For example, since a 3 percentage point decrease in interest rate was given only to individuals with high behavior scores and low utilization rates (cell 9), we cannot generalize the effect of a 3 percentage point decrease in interest rates to the experimental sample. Similarly, the estimated effect of the 3 percentage point increase can be generalized only to individuals with low behavior scores.

4.1 Implementation

Unlike many studies that used randomized field experiments (mainly in development economics), we were not involved in the design or implementation of the experiment our analysis is based on. Although randomized experiments are now standard practice amongst credit card companies and they have every incentive to implement them correctly, we need to make sure that the randomization was carried out properly to ensure the internal validity of our results.

We perform several tests including a series of mean equality and distribution equality tests on a range of variables including our outcome variables. These tests were carried out for the month of July 2006 (the date of the design) and repeated for August 2006 and September 2006 (the last 2 months before the implementation). Table 3 presents the p-values obtained from mean equality tests and Table 4 presents the likelihood ratio statistics (χ^2) from the probit regression of the treatment dummy on several variables such as debt, interest rates, credit limit, income, age, behavior score, utilization rate and statement balance. We also performed distribution equality tests using Kolmogorov-Simirnov and K-Wallis tests for the variables in Table 3 (results are available upon request) and could not detect any statistically significant difference between the treated and the controls. We are in the end, convinced that the randomization was carried out properly.

4.2 Other Threats to Internal Validity

Even though the randomization was carried out properly there may be other threats to the internal validity of our experimental estimates. Sample attrition, for example, would be of particular concern if it were caused by the treatment. This could happen if the treatment (interest rate increase) initiated delinquency and eventually default, making the remaining treatment sample no longer comparable to the control sample. If the treatment caused some accounts to be charged off, our treatment effect estimates may be biased toward finding insensitivity to interest rates. Recall however that we can follow outcomes of the experiment only for three months. It is unlikely that we would see any default in such a short period as it usually takes several months of delinquency for the lender to charge the delinquent account off.

However, we can explore whether the treatment induced *intention to default* by looking into the number of delinquent months following the treatment. The idea here is that if the treatment induces default, we may observe it as delinquency (missed monthly payments) starting from the implementation date. For this, we investigate whether there is any statistically significant difference between the treated and control in terms of falling into a delinquency cycle after the treatment. More specifically, we test the equality of number of delinquent months between the treated and the control groups from September 2006 to December 2006, inclusive. Table 5 presents these results (p-values for equality tests). We do not reject the hypothesis of equality and conclude that the treatment did not induce intention to default within the sample period.

Another problem common in randomized experiments is noncompliance, that is, the possibility that units allocated to the treatment group are not treated. This situation could arise in our case if, for example, some individuals that are allocated to a treatment group objected to the interest rate increase and the lender consequently reversed the change. Fortunately, we do not face this problem in our sample; all accounts that are allocated into treatment groups did receive the change in interest rates.

5 Assessing the Experimental Design

In this section we assess how informative the experimental design is in answering the questions we pose. In particular, we would like to know first, how much of an effect we can detect statistically and second, how much of an economic effect we can expect given the theoretical model outlined in section 3. For the former we resort to the concept of "minimum detectable effect". In our case it is the minimum true difference (in £) between the control and the treated that can be statistically detectable with 80% confidence at a 5% significance level[16].

In order to calculate expected economic effect of a change in interest rates, we first assume a functional form for the utility function in the intertemporal model we outlined in section 3. Following the large body of theoretical and empirical literature, we take the constant relative risk aversion (CRRA) utility function:

$$U(C) = \frac{C^{1-\gamma}}{1-\gamma} \tag{6}$$

[16] See List et al (2010) and Duflo et al (2006) for excellent reviews.

where γ is the coefficient of relative risk aversion reciprocal of which is the elasticity of intertemporal substitution. Given the CRRA utility function, consider the first order condition arising from the maximization of equation (1).

$$C_t^{-\gamma} = \beta(1+r)E_t[\lambda_{t+1}] \tag{7}$$

where $E_t[\lambda_{t+1}]$ is the expected marginal utility of consumption at t. Here we assume that the interest rate is pre-determined (consistent with our lender's practice of announcing the interest rate changes in advance). Taking the logarithm of both sides and re-arranging, we obtain:

$$\ln C_t = -\frac{1}{\gamma}\ln\beta - \frac{1}{\gamma}\ln(1+r) - \frac{1}{\gamma}\ln E_t[\lambda_{t+1}] \tag{8}$$

Differentiating the above equation with respect to $\ln(1+r)$, we obtain:

$$d\ln C_t = -\frac{1}{\gamma}\left[1 + \frac{\partial \ln E_t[\lambda_{t+1}]}{\partial \ln(1+r)}\right] d\ln(1+r) \tag{9}$$

This equation simply states that a change in interest rates will have a substitution effect (first term) and an income effect (second term). For a borrower, an increase in interest rates will lower the future consumption (increase the future marginal utility of consumption, implying a negative income effect) so that

$$\frac{\partial \ln E_t[\lambda_{t+1}]}{\partial \ln(1+r)} > 0 \tag{10}$$

Therefore the substitution effect will constitute the lower bound for the reduction in consumption so that

$$|\Delta \ln C| > \left|\frac{1}{\gamma}\Delta\ln(1+r)\right| \tag{11}$$

As a simple illustration, if, based on the micro evidence[17], we take $\frac{1}{\gamma} = 0.75$ and monthly income/consumption of £1400 (given the reported mean individual income in Table 1), the above inequality implies that a 1 percent increase in $(1 + r)$ is expected to reduce current consumption of unconstrained borrowers by *at least* £10.5[18]. Together with the calculated minimum detectable effects, such expected economic effects will be useful in order to interpret our experimental results in the following section.

6 Results

6.1 Experimental Estimates

The main objective of the paper is to infer the interest rate sensitivity of monthly credit card borrowing. Individuals who would like to borrow more but have limited access to credit are expected to be insensitive to the cost of borrowing. The sensitivity of demand for borrowing to interest rates can be easily determined at the extensive margin; as interest rates go up, loan take up is expected to go down for unconstrained individuals. However, testing this sensitivity using credit card debt requires a different treatment. As explained in section 3, once incurred, revolved credit card debt itself is no longer the proper choice variable for a given month.

Guided by the standard intertemporal theory of consumption outlined in Section 3, we estimate the following equation for each cell:

$$NNB = \alpha + \beta T + \varepsilon \tag{12}$$

where NNB denotes the net new borrowing and T is the treatment dummy which takes the value of 1 if the individual is in the treatment group and 0 if the individual is in the control

[17] See Attanasio et al (1999), Alan (2006) and Alan and Browning (2010).

[18] Remember that individuals in our sample are assumed to be net borrowers, or simply "hand to mouth" consumers with no savings. It is also important to note that theoretically, the income income effect is realized at the time when interest rate change is communicated. Therefore, expected economic effect after the implementation is only the substitution effect.

group. The fact that the randomization was carried out properly assures us that there would be no observable or unobservable difference between the treatment and the control group other than receiving the treatment. Then the coefficient β in the above regression will give us an unbiased estimate of the average effect of treatment on the treated (AETT). For individuals who are currently liquidity constrained, we expect that the estimated average treatment effect is not statistically different from zero. This should be true for small interest rate increases. In our data set, those individuals are likely to be the ones with high utilization rates (cells 1, 4 and 7).

Borrowers with low utilization rates are expected to lower their consumption (equivalent to lowering NNB_t in this framework) when faced with an increase in interest rates since they do have available borrowing opportunities but they choose not to fully utilize them (they could borrow more on this card). Cells 2,3,5,8 and 9 fit this description. The mean utilization rate was approximately 50% for cells 2,5 and 8, and around 10% for cells 3 and 9 in July 2006.

An important point that will be relevant when it comes to interpretation of the experimental results is that the type of treatment differs (slightly) across cells. For cell 1 for example, the estimate of equation (14) measures sensitivity to a 3 percentage point increase in interest rates, whereas for cell 4, it measures the effect of a 1 percentage point increase. Therefore, we cannot provide the overall AETT for the entire sample. However, we will be able to do this when we impose linearity in the next subsection. Note also that given the existing cross sectional variation in interest rates, percentage increase in interest rates in a given cell will depend on the pre-treatment interest rates. For example, in cell 1 where the treatment group received 3 percentage point increase, an individual with 30% pre-treatment rate will have a 10% increase in his borrowing rate.

In order to estimate the average treatment effect, first, we compare the means across individuals and months by running the following regression for each cell:

$$NNB_{it} = \alpha + \beta T + \varepsilon_{it}. \tag{13}$$

Then, to see if there is any delayed response to the interest rate changes, we estimate the average treatment effect by months by running the following regression

$$NNB_{it} = \alpha + \beta_1 T + \beta_2 November + \beta_3 December + \beta_4 November * T + \beta_5 December * T + \epsilon_{it} : \tag{14}$$

Here, β_1 is the estimated average treatment effect for October, $\beta_1 + \beta_4$ is for November and $\beta_1 + \beta_5$ is for December.

Following the standard practice, we correct the standard errors to take into account the panel structure and the possibility of heterogenous treatment effects. Table 6 presents average treatment effects and minimum detectable effects by cell. The first column of the table gives the mean net new borrowing for the control groups (α), the second column presents average treatment effects (β).

Looking at the first column of the table, one notices that the mean net new borrowing for the control group in the high utilization cells is negative and around £14; it is positive and around £8-£10 in the mid utilization cells and relatively high (around £32-£38) for low utilization cells (cells 3 and 9). This tells us that not much new borrowing takes place in the high utilization cells, in fact we see a small amount of debt reduction (a negative NNB value) by these individuals. The question is whether there is any difference between the treated and the control. As seen in the second column of the table, we do not detect any statistically significant difference in net new borrowing between the control and treated in any cell.

The numbers in column 3 give the minimum true difference (in £) between the control and the treated needed to be statistically detectable with 80% confidence given a 5% significance level. For example, if in cell 1, the true change in net new borrowing due to the treatment of a 3 percentage point increase was greater than £19.5, we would be able to detect it with 80% confidence, given our choice of significance level. The last column presents the lower bound of absolute consumption change expected given the discussion in section 5. The values in this column are calculated using the mean pre-treatment interest rates (approximately 32%), mean monthly consumption of £1400 (calculated using mean annual income divided by 12) and an elasticity of intertemporal substitution of 0.75.

Consider first the high utilization cells. These are cells 1, 4 and 7 with the utilization rates very close to 100 %. Except for cell 1, these are also the highly populated cells by design, so treatment effects are estimated precisely. Individuals in these cells tend to revolve large amounts of debt from month to month and pay heavy interest charges. Among all the account holders in our sample, these are the ones who may want to borrow more but may not be able to do so, and may be credit constrained and insensitive to interest rates. The fact that we estimate no statistically significant difference between the control and the treated is no surprise. Note that for these cells, mean NNB for the control is around £14 (where a negative sign means lowering existing debt). The estimated mean difference is higher for cell 1 (-£9.7) which could be because this cell received a 3 percentage point increase in their interest rates while the other two cells received only a 1 percentage point increase. For cells 4 and 7, the average treatment effects are economically negligible (£0.07 and -£1.2 respectively). Given the small minimum detectable effects we do not have much doubt that the true sensitivity must be effectively zero for these cells. This insensitivity result for the high utilization cells carries through when we estimate the average treatment effects month by month (see Table 7).

What about the low utilization cells? Individuals in these cells are not liquidity constrained in the strong sense of the term as they can borrow more on this card. The mean net new borrowing for the control groups in these cells is all positive and decreasing with utilization rates. Individuals who were in mid utilization cells in July 2006 (cells 2 ,5 and 8) seem to acquire £7-£10 new debt every month, whereas the number is about £30-£40 for individuals who were in low utilization cells, although the means are imprecisely estimated for cells 2 and 5. For cells 2, 5 and 8 (with an average utilization rate around 50%) and cells 3 and 9 (with average utilization rate around 10%) we find statistically zero average treatment effects. Note however that the minimum detectable effects are large for these cells. Given the expected economic effects, our estimates will not be precise enough to detect the expected effect with any confidence (minimum detectable effects are larger than expected economic effects). For example, consider cell 5. The expected consumption decline due to a 1 percentage point increase in interest rates is at least £8. The minimum detectable effect for this cell is £25 implying that it is very unlikely for us to detect a true effect. While we do not feel confident about the results obtained from cell-by-cell estimation, in Subsection 6.3, we confirm these "insensitivity" results when pooling across all low utilization cells.

6.2 Heterogeneity in Treatment Effects

Based on the discussion above, the apparent insensitivity to interest rates by borrowers who utilize their credit limits fully can be interpreted as evidence of binding liquidity constraints among this group. It is generally accepted that liquidity constraints are more likely to affect the young and those with low income. In our case, having no other credit card than the one issued by our lender may also indicate the actual credit limit the individual is facing. A very high utilization individual who reported to possess no other credit card would be the likeliest candidate to be constrained in the strong sense of the term. On the other hand, individuals who reported to have other credit cards may have the flexibility to transfer their balances (subject to some switching costs) to other cards when faced with an increase in interest rates. Such a transfer clearly would not change the individual's overall debt holding (no reduction in consumption) but would seem that way in our sample due to the observed payment. Unfortunately, we have no way of knowing the nature of the payments, whether it is a balance transfer to another card or a genuine payment, made toward balances. Balance transfers would bias our results toward finding sensitivity, and the estimated magnitude of consumption reduction should then be considered as an upper bound.

Table 8 presents estimated average treatment effects conditional on having other credit cards, age and income[19]. Results in this table are generally very similar to our previous results. We see no interest rate sensitivity in any cell, whether individuals have other credit cards or not. The numbers do not show any clear pattern. A possible explanation for this may be that the other credit cards these individuals hold are likely to be equally high interest rate cards since they were willing to apply for a new high interest rate card. Anecdotal evidence also suggests that some individuals apply for a new high interest rate card when they fully utilize their other high interest rate cards.

In contrast to the findings of Attanasio et al (2008) and Alessie et al (2005), we do not find any significant difference between high and low income individuals with the exception of cell 1 where we estimate about £31 reduction in net new borrowing for individuals who reported income over £20,000 (columns 5 and 6 in Table 8). Note, however, that the distinction between high and low income is only relative in our case, as almost all our individuals have low income. In summary, we find no evidence of sensitivity to either a 1 or 3 percentage point increase (or the 3 percentage point decrease, cell 9) in our sample, even after conditioning on variables that are thought to be useful in characterizing unconstrained individuals, such as being middle aged and older, and possessing multiple credit cards. This result was largely expected for the individuals who were fully utilizing their credit cards (and among those who have no other credit cards).

6.3 Can Econometrics Replicate the Experiment?

Credit demand equations have been estimated on nonexperimental data, usually by imposing some exogeneity restrictions. Attanasio et al (2008), Alessie et al (2005) and Gross and Souleles

[19] We also condition on employment status and home ownership. Unreported results are very similar and available upon request.

(2002) are exemplary studies of this sort.[20]. These authors emphasize the potential detrimental effects of endogenous interest rates on credit demand estimates and promote instrumental variable estimation. In this section, we illustrate the importance of this choice i.e., the importance of exploiting only exogenous variation in interest rates when estimating such equations.

It is also important to emphasize that interest rate experiments are common practices for specialized credit card issuers, and form a part of their advance risk pricing strategies. Moreover, even in prime credit card markets some issuers are known to conduct frequent randomized experiments, and use these to guide changes in interest rates and changes in other characteristics of the accounts, such as credit limits (see Gross and Souleles 2002). Without knowing the exact experimental design (in our case, knowing the design amounts to observing the cell identifier) it is not possible to isolate the exogenous variation in interest rates. This is true even without any such experiments, but when interest rate changes are applied to certain accounts based on some lender specific information that is not available to researchers.

In our data, in the absence of the cell identifier variable, we observe interest rate changes of 1, 3 and -3 percentage points for some accounts but we do not observe the proper comparison group (individuals with no interest rate changes) for these individuals[21]. Using both time series and cross section variation, we can estimate a borrowing demand equation such as

$$\Delta NNB_{i,t} = \sum_{j=0}^{K} \beta_j \Delta r_{t-j,i} + \gamma' X_{i,t} + \alpha' MonthDummies + \varepsilon_{i,t} \tag{15}$$

or to estimate interest rate elasticities of credit card debt, D and normalize credit card debt,

[20] Attanassio et al (2008) use the 1986 tax reform act in the US, Alessie et al (2005) use a change in the usury law in Italy to instrument interest rates. Gross and Souleles(2002) use instruments exploiting exogenous timing rules of credit card companies.

[21] It is true that we observe zero interest rate changes so we know who the controls are but without cell identifiers we cannot establish proper comparison groups.

$\frac{D}{CL}$ (as in Gross and Souleles (2002)):

$$\Delta D_{i,t} = \sum_{j=0}^{K} \beta_j \Delta r_{t-j,i} + \gamma' X_{i,t} + \alpha' MonthDummies + \varepsilon_{i,t} \tag{16}$$

$$\Delta \left(\frac{D_{i,t}}{CL_{i,t}} \right) = \sum_{j=0}^{K} \beta_j \Delta r_{t-j,i} + \gamma' X_{i,t} + \alpha' MonthDummies + \varepsilon_{i,t} \tag{17}$$

where change in interest rates $\Delta r_t = r_t - r_{t-1}$. The lags are included to account for delayed response to interest rate changes. Note that the differencing takes out cross-sectional variation in interest rate levels. The remaining variation is the time variation and the cross-sectional variation in interest rate changes (which is correlated with account characteristics). The above specification can be used to estimate short-term (1 month) as well as long-term sensitivities (and elasticities). Since we observe the accounts only for 3 more months following the interest rate changes, we can estimate the sensitivity only up to 3-months ($j = 0, 1, 2$). Month dummies are included to account for cyclical spending patterns. Other variables (X) include observable characteristics of the account that may be relevant for borrowing demand. We experiment with utilization rates (lags), internal behavior score (lags), change in credit limits (lags) and account age. We also estimate fixed effects models using the above equations to account for individual-specific trends in borrowing demand.

Note that the above equations can also be estimated using only the cross-sectional variation in interest rates in a given month which is undoubtedly endogenous. Using the panel feature of our data we are able to illustrate that even the fixed effects estimators (that are designed to control for unobserved heterogeneity) can lead to biased elasticity estimates if the endogeneity in interest rate changes is not properly taken care of with a good instrument. We can illustrate this important point since we do have the perfect instrument: the experiment.

The overall interest rate variation used to estimate the above equations includes cross-cell variation, which is endogenous, as well as exogenous (experimental) within-cell variation. A fully saturated model with a full set of cell dummies (or equivalently, bscore and utilization rate (as at July 2006) dummies and their interactions) isolates the experimental variation. With a less than fully saturated model however, estimation of the above equations is subject to standard omitted variable bias even when we control for utilization and bscore (and their lags) since all the omitted interaction terms are, by design, correlated with interest rate changes. To illustrate, we first estimate the above equations without cell information. We then estimate the same equations by conditioning on cell dummies and their interaction with interest rate changes to control for across cell variation in interest rates.

Table 9 presents the estimated 1 month and 3 month sensitivities for credit card debt (D), debt normalized by credit limit (D/CL) and net new borrowing (NNB). The first two columns present results without conditioning on cells that is a nonexperimental use of the data. In the first column we present estimates without the control variables (X) and fixed effects, the second column adds those to the equations. We estimate a small but significant 1 month decline in credit card debt for both specifications; the estimated 1 month decline in debt is about £5 and £7 respectively for a 1 percentage point increase in interest rates (implying -0.22 and -0.25 elasticity calculated at the means). The estimated 3 month sensitivities are £12 (elasticity -0.51) and £24 (elasticity -1.05) respectively for specifications 1 and 2, and they are statistically significant. These figures seem very small but we should reemphasize that our sample mainly consists of very low income individuals that are not expected to be interest rate sensitive at all. Note also the statistical significance of the results. When we control for observable account characteristics and fixed effects, we still estimate a significant sensitivity of net new borrowing; a 1 percentage point increase in interest rates leads to a £14 decrease in new borrowing (in three months)[22].

We tried several other controls (income, individual's age, employment status etc.) and

[22] Unfortunately, there is no study to which we can directly compare our results in this section. Although Gross and Souleles (2002) estimate elasticities of credit card debt with respect to interest rates, their sample represents all US credit card holders. Nevertheles, they find approximately $100 decline in debt in 9 months for each percentage point increase in interest rates. This number makes our estimate of £24 decline in 3 months look quite big, especially if one considers the fact that our sample covers the low end of the income distribution in the UK.

established that the finding of significant debt reduction in 3 months is quite robust. This is also true when we normalize debt by the credit limit. For net new borrowing, the results are very sensitive to different specifications. We obtain responses ranging from statistically significant and large negative to statistically significant and large positive depending on which controls we use. This finding is enough in itself to cast doubt on nonexperimental estimates without convincing exogenous variation in interest rates or interest rate changes.

The last column in Table 9 presents the experimental estimates. We show only specification 1 as, not surprisingly, the other specification (fixed effects estimation) gave materially the same results. As it can be seen in this column, there is no sign of debt reduction or reduction in new borrowing in the case of a 1 percentage point interest rate increase. All estimates are both economically and statistically insignificant. We estimate virtually zero elasticity of debt/new borrowing with respect to borrowing rates when we isolate cross-cell variation in interest rates. This is an additional confirmation of our experimental estimates presented in Section 5.1 where we do not impose any functional form in the way we do in this section. In addition to illustrating the importance of isolating the exogenous variation in interest rates, the results obtained in this section are also useful to confirm that our "insensitivity" conclusion in Section 5.1 is not due simply to high standard errors. We obtain the same results when pooling across cells with a linear functional form which substantially increase the precision of the estimates.

Finally, we repeat this exercise for high and low utilization cells separately. Here, cells 1, 4 and 7 are classified as "high utilization" cells, the other cells as "low utilization" cells. Table 10 presents the results. Nonexperimental regressions include all the control variables described earlier and account specific fixed effects. The message that emerges from this table is striking. We find virtually no difference (economically, or statistically) between the estimates obtained with experimental and nonexperimental data for the high utilization cells (1, 4,and 7); confirming our priors about binding liquidity constraints, there appears to be no sensitivity to interest rates in these cells (compare columns 1 and 3 in Table 10). The striking contrast to this result comes from the low utilization cells (see columns 2 and 4). While the nonexperimental use of the data yields an economically and statistically large response to interest rates in the direction predicted by the intertemporal theory, the experimental results tell us a completely different story. With the nonexperimental use of the data, we estimate a £32 decline in debt (implying an elasticity of -1.72) and £11 decline in net new borrowing over 3 months in response to a 1 percentage point increase in interest rates. On the other hand, the experimental variation alone shows that these individuals in fact accumulate debt (approximately £16 over three months, implying the elasticity of 0.85) in response to a 1 percentage point increase in interest rates.

Recall our discussion in Section 3 that when faced with an increase in interest rate, individual's debt automatically increases due to additional interest charges unless the net new borrowing declines. Since the net new borrowing is positive in low utilization cells (see Table 6, column 1), we observe an increase in debt when faced with higher interest rates. It appears that the cross cell variation is very strong for the lower utilization group, causing significant omitted variable bias due to nonlinearities inherited in the block design. This bias is so strong that results obtained with mixed within and cross cell variation are materially very different (£32 decline versus £16 increase in 3 months). This result clearly illustrates the importance of careful research design when estimating such demand equations.

7 Conclusion

We estimate the sensitivity of credit demand to interest rates. A unique data set on monthly credit card transactions from a subprime credit card company that includes a randomized interest rate experiment gives us the opportunity to carry out this task with truly exogenous variation in interest rates. We find that individuals who utilize their credit limits fully are insensitive to exogenous increases in interest rates as high as 3 percentage points. We interpret this finding as evidence of binding liquidity constraints.

We also illustrate the importance of genuinely exogenous variation in interest rates in estimating credit demand elasticities. We show that estimating a standard credit demand equation with the nonexperimental variation leads to seriously biased estimates even when using a rich set of controls and fixed effects. In particular, this procedure results in a large and statistically significant 3-month elasticity of credit card debt with respect to interest rates even though the experimental estimate of the elasticity is neither economically nor statistically different from zero.

We argue that this is an extremely important result as the related research on interest rate sensitivity has far reaching implications for policies targeting credit markets. It could be even more important for policies that tend to focus on "vulnerable" groups such as individuals with low and volatile income, and those affected by adverse credit shocks in the past. The estimated sensitivity to interest rates using the nonexperimental data is quite misleading and hides the fact that subprime credit card borrowers do not tend to decummulate debt in the face of increasing borrowing costs. In fact, the higher interest rate leads to higher debt overtime.

Our results are obtained using data from a single lender. However, this lender is an important market player and the risk pricing practices presented here are common throughout the industry. The randomized interest rate experiments undertaken by our lender are also not uncommon, though access to the data is. Therefore we believe that the evidence we provide in this paper sheds important light on the sensitivity of credit demand to borrowing rates amongst poor households and the pervasiveness of liquidity constraints in highly sophisticated credit markets.

References

[1] Adams, W., L. Einav, and J. Levin, "Liquidity Constraints and Imperfect Information in Subprime Lending," *American Economic Review* 99 (2009), 49–84.

[2] Alan, S., "Entry Cost and Stock Market Participation over the Life Cycle," *Review of Economic Dynamics* 9 (2006), 588–611.

[3] —–, and M. Browning, "Estimating Intertemporal Allocation Parameters Using Synthetic Residual Estimation," *Review of Economic Studies* 77 (2010), 1231-1261.

[4] Alessie, R., S. Hochguertel, and G. Weber, "Consumer Credit: Evidence from Italian Micro Data," *Journal of the European Economic Association* 3 (2005), 144-78.

[5] Attanasio,O., J. Banks, C. Meghir and G. Weber, "Humps and bumps in Lifetime Consumption" *Journal of Business and Economic Statistics*, 17 (1999), 22-35.

[6] Attanasio, O. P., P. K. Goldberg, and E. Kyriazidou, "Credit Constraints in the Market for Consumer Durables: Evidence from Micro Data on Car Loans," *International Economic Review* 49 (2008), 401-36.

[7] BIS - Department for Business, Innovation & Skills, "Review of the Regulation of Credit and Store Cards. Government Response to Consultation, Economic Impact Assessment," March 2010.

[8] Duflo, E., R. Glennerster and M. Kremer, "Using Randomization in Development Economics Research: A Toolkit" (2006)

[9] Gross, D. B., and N.S. Souleles, "Do Liquidity Constraints and Interest Rates Matter for Consumer Behavior? Evidence from Credit Card Data," *Quarterly Journal of Economics* 117 (2002), 149-85.

[10] Karlan, D. S., and J. Zinman, “Credit Elasticities in Less-Developed Economies: Implications for Microfinance,” *American Economic Review* 98 (2008), 1040–68.

[11] List, A. J., S. Sadoff and M. Wagner, “So You Want to Run an Experiment, Now What? Some Simple Rules of Thumb for Optimal Experimental Design” , NBER WP: 15701, (2010)

[12] Pissarides, C., “Liquidity Considerations in the Theory of Consumption,” *Quarterly Journal of Economics* 105 (1978), 219-34.

[13] Data Monitor UK Plastic Cards 2008 Report, "Nonstandard Credit Cards in the UK", Available only via Purchase. Email: customerservice@marketresearch.com

Table 1: Decriptive Statistics, July 2006

	mean	**median**	**st. dev.**
utilization rate (%)	72.6	90.0	32.0
statement balance (£)	720.1	600.8	549.3
debt (£)	649.1	552.2	544.4
new transactions (£)	76.5	0.0	173.3
credit limit (£)	1,079.7	850.0	711.2
interest rate	31.8	32.9	3.7
income (£)	16,955	15,000	15,620
age	44.2	43	11.7
married	56%	–	–
employed	61%	–	–
self employed	13%	–	–
home owner	35%	–	–
no other card	40%	–	–

Notes: Number of observations=18,232

Table 2: Experimental Design

Utilization Rate (0 to 100%) \ Behaviour score (Bscore)	Low	Mid	High
High	CELL 1 T=3 pp #T=337 #C=413	CELL 4 T=1 pp #T=1407 #C=1742	CELL 7 T=1 pp #T=3420 #C=4112
Mid	CELL 2 T=3 pp #T=101 #C=130	CELL 5 T=1 pp #T=467 #C=135	CELL 8 T=1 pp #T=3038 #C=865
Low	CELL 3 T=3 pp #T=62 #C=80	CELL 6 T=1 pp #T=188 #C=0	CELL 9 T=-3 pp #T=499 #C=1424

Table 3: Tests for Internal Validity

Variable	Cell 1	Cell 2	Cell 3	Cell 4	Cell 5	Cell 7	Cell 8	Cell 9
Utilization Rate	.66	.93	.34	.51	.60	.52	.44	.41
Bscore	.77	.72	.36	.14	.62	.18	.37	.87
Net New Borrowing (NNB)	.22	.13	.69	.54	.21	.85	.44	.53
Revolving Debt	.54	.44	.32	.12	.59	.40	.20	.46
Interest Rates	.95	.47	.32	.44	.09	.79	.16	.72
Credit Limit	.47	.23	.41	.40	.28	.76	.25	.45
Income	.81	.66	.64	.79	.74	.40	.89	.18
Age	.60	.84	.50	.84	.74	.98	.55	.84

Notes: P-values (not adjusted for multiple testing) for the mean equality tests (equal variance imposed)

Table 4: Further Internal Validity Tests

	July 2006	August 2006	September 2006
cell 1	2.3	2.7	5.4
cell 2	8.5	9.5	3.9
cell 3	9.1	11.2	1.8
cell 4	10.3	13.3	12.2
cell 5	9.8	7.1	6.7
cell 7	4.9	2.9	5.3
cell 8	7.8	6.4	10.5
cell 9	9.8	8.7	11.1

Notes: Chi-square (χ^2_8) values are obtained from probit regressions of the treatment dummy on age, income, interest rates, balance, debt, credit limit, utilization rate and bscore (July 2006). Critical value $P(\chi^2 > 16.9) = 0.05$

Table 5: Equality of the Number of Delinquent Months, September-December 2006

	P-Values for Equality Tests
cell 1	0.72
cell 2	0.86
cell 3	0.38
cell 4	0.61
cell 5	0.50
cell 7	0.82
cell 8	0.35
cell 9	0.76

Table 6: Experimental Estimates

	Average Treatment Effects (β): $NNB_{i,t} = \alpha + \beta T + \varepsilon_{i,t}$			
Cells	Control's mean NNB	Average Treatment Effect β	Abs. Min. Det. Effect	Abs. Min. Economic Effect
Cell 1 (3pp)	-13.1	-9.7	19.5	24 or 0
	(2.84)	(1.39)		
Cell 2 (3pp)	7.6	-7.4	28.8	24
	(.96)	(.72)		
Cell 3 (3pp)	32.2	3.9	56.5	24
	(2.52)	(.19)		
Cell 4 (1pp)	-14.4	.07	8.03	8 or 0
	(7.27)	(.03)		
Cell 5 (1pp)	7.8	-.54	24.6	8
	(1.10)	(.06)		
Cell 7 (1pp)	-14.4	-1.2	7.4	8 or 0
	(7.90)	(.45)		
Cell 8 (1pp)	10.6	8.6	45.7	8
	(2.27)	(1.62)		
Cell 9 (-3pp)	38.5	6.8	53.8	24
	(9.98)	(.86)		

Notes: Absolute t-ratios calculated with clustered standard errors in parentheses. Minimum detectable effects: 80% power, 5% significance. Minimum economic effects are calculated at the pre-treatment mean interest rates (32%) for $\frac{1}{\gamma} = 0.75$ and monthly consumption of £1400. Values are in British Pounds (£).

Table 7: Experimental Estimates

Average Treatment Effects by Months $NNB_{i,t} = \alpha + \beta_1 T + \beta_2 Nov + \beta_3 Dec + \beta_4 Nov * T + \beta_5 Dec * T + \varepsilon_{i,t}$			
Cells	Average TE October β_1	Average TE November $\beta_1 + \beta_4$	Average TE December $\beta_1 + \beta_5$
Cell 1 (3pp)	2.6	-11.4	-20.4
	(0.8)	(0.9)	(1.6)
Cell 2 (3pp)	8.9	-34.3	3.2
	(0.7)	(1.8)	(0.1)
Cell 3 (3pp)	22.1	2.3	-12.6
	(0.6)	(0.1)	(0.3)
Cell 4 (1pp)	-3.9	7.5	-3.4
	(0.5)	(1.6)	(0.6)
Cell 5 (1pp)	9.4	-9.2	-1.8
	(0.5)	(0.5)	(0.1)
Cell 7 (1pp)	-1.5	5.0	-7.1
	(0.8)	(1.1)	(1.4)
Cell 8 (1pp)	2.6	3.0	20.2
	(0.8)	(0.3)	(1.7)
Cell 9 (-3pp)	3.2	0.5	16.8
	(0.8)	(0.0)	(1.3)

Notes: Absolute t-ratios calculated with clustered standard errors in parentheses. Values are in British Pounds (£).

Table 8: Experimental Estimates

Average Treatment Effects (β): $NNB_{i,t} = \alpha + \beta T + \varepsilon_{i,t}$						
	No Other Cards	Other Cards	Age<31	Age>39	Income<10000	Income>20000
Cell 1(3pp)	-5.2	-13.8	5.3	-11.3	13.8	-30.6*
	(0.6)	(1.3)	(0.4)	(1.2)	(1.4)	(1.8)
Cell 2 (3pp)	-21.6	0.27	3.6	-6.4	-12.6	-6.0
	(1.6)	(0.0)	(0.2)	(0.4)	(0.9)	(0.3)
Cell 3(3pp)	9.3	1.0	-9.0	-21.6	8.3	-6.5
	(0.3)	(0.1)	(0.3)	(0.9)	(0.2)	(0.2)
Cell 4 (1pp)	-0.36	-0.10	2.7	1.4	7.2	-4.6
	(0.1)	(0.0)	(0.5)	(0.3)	(1.5)	(0.7)
Cell 5 (1pp)	17.1	-11.1	-0.54	-16.1	2.8	-.52
	(1.2)	(1.0)	(0.0)	(1.2)	(0.2)	(0.0)
Cell 7(1pp)	-3.7	0.52	-6.5	2.7	-1.9	-.52
	(0.9)	(0.2)	(1.2)	(0.8)	(0.4)	(0.1)
Cell 8(1pp)	5.6	9.8	4.1	4.3	6.3	11.5
	(0.6)	(1.6)	(3.4)	(0.6)	(0.6)	(1.2)
Cell 9(-3pp)	-5.2	15.4	3.2	0.44	-3.9	4.7
	(0.5)	(1.3)	(0.2)	(0.1)	(0.3)	(0.3)

Notes: Absolute t-ratios calculated with clustered standard errors in parentheses.
Values are in British Pounds (£). $*$: significant at 10% level.

Table 9: Interest Rate Sensitivity of Credit Card Debt

	Nonexperimental		Experimental
	Spec 1	Spec2	Spec1
1 month sensitivity, β_0, (D)	-5.1^{**}	-6.8^{**}	1.3
	(1.8)	(1.7)	(1.9)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, (D)	-11.7^{**}	-24.2^{**}	2.5
	(3.8)	(4.6)	(5.1)
1 month sensitivity, β_0, (D/CL)	$-.005^{**}$	$-.006^{**}$	$-.001$
	(.001)	(.001)	(.002)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, (D/CL)	$-.008^{**}$	$.20^{**}$	.003
	(.002)	(.004)	(.004)
1 month sensitivity, β_0, (NNB)	$-.03$	-3.8	2.7
	(2.2)	(2.7)	(2.9)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, (NNB)	-3.0	-14.1^{**}	-1.4
	(3.4)	(5.2)	(5.6)

Notes: Clustered standard errors are in parentheses. **: significant at 5%, *: significant at 10%. The first 2 columns present regressions without cell information. Values in the first column (Spec 1) are obtained from the regressions of ΔD_t ($\Delta(D/CL)$ and ΔNNB respectively) on change in interest rates (and its 2 lags) and month dummies. The second column adds lags of utilization rate, bscore, change in credit limits, account age and account-specific fixed effects. Values in the last column are obtained from the regressions of ΔD_t ($\Delta(D/CL)$ and ΔNNB respectively) on change in interest rates (and its 2 lags) and month dummies (spec 1) by using the cell information, that is, adding cell dummies and their interactions with all other right hand side variables.

Table 10: Interest Rate Sensitivity of Credit Card Debt

	Nonexperimental		Experimental	
	High Util	Low Util	High Util	Low Util
1 month sensitivity, β_0, (D)	.35	-10.3^{**}	.83	3.4
	(2.2)	(2.6)	(2.7)	(3.4)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, (D)	-6.3	-31.9^{**}	-4.6	15.9^{**}
	(6.1)	(6.5)	(6.3)	(8.1)
1 month sensitivity, β_0, (D/CL)	.00	$-.01^{**}$	$-.001$	$-.001$
	(.02)	(.002)	(.003)	(.004)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, (D/CL)	.001	$.02^{**}$	$-.002$	$.012^{**}$
	(.005)	(.005)	(.005)	(.005)
1 month sensitivity, β_0, (NNB)	1.4	-6.1	3.8	2.6
	(3.6)	(3.8)	(4.0)	(5.2)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, (NNB)	-8.4	-11.4^{*}	-10.8	14.4
	(7.6)	(6.7)	(6.9)	(8.9)

Notes: Clustered standard errors are in parentheses. **: significant at 5%, *: significant at 10%. Low Util refers to cells 2,3,5,8,and 9, High util refers to cells 1,4 and 7. The first 2 columns present regressions without cell information. Values in these columns (nonexperimental) are obtained from the regressions of ΔD_t ($\Delta(D/CL)$ and ΔNNB respectively) on change in interest rates (and its 2 lags), month dummies, lags of utilization rate, bscore, change in credit limits, account age and account-specific fixed effects. Values in the last two columns (experimental) are obtained from the regressions of ΔD_t ($\Delta(D/CL)$ and ΔNNB respectively) on change in interest rates (and its 2 lags) and month dummies by using the cell information, that is, adding cell dummies and their interactions with all other right hand side variables.

BANQUE CENTRALE DU LUXEMBOURG

EUROSYSTÈME

2, boulevard Royal
L-2983 Luxembourg

Tél.: +352 4774-1
Fax: +352 4774 4910

www.bcl.lu • info@bcl.lu