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

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

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ABSTRACT

Subprime Consumer Credit Demand: Evidence from a Lender's Pricing Experiment*

Using a unique panel data set from a UK credit card company, we analyze the interest rate sensitivity of subprime credit card borrowers. In addition to all individual transactions and loan terms, we also have access to details of a randomized interest rate experiment conducted by the lender on the existing (inframarginal) loans. Access to such information by academic researchers is rare. The data and the experimental design provide us with a clean identification of heterogenous interest rate sensitivities across borrower types within the subprime population. We find that subprime credit card borrowers generally do not reduce their demand for credit when subject to increases in interest rates. However, we estimate a number of interesting responses that suggest that subprime borrowers are not a homogenous group. The paper also contributes to the literature by demonstrating the importance of isolating exogenous variation in interest rates. We show that estimating a standard credit demand equation with the non-experimental variation in the data leads to severely biased estimates. This is true even when conditioning on a rich set of controls and individual fixed effects.

JEL Classification: D11, D12 and D14
Keywords: subprime credit; randomized trials; liquidity constraints

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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 propose some firm-specific practices as instruments for 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].

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 lender that exclusively serves the subprime market in the United Kingdom[2]. Our paper

[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.

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

makes several novel contributions to the literature. First, besides all the individual transactions and credit terms we observe in the data, we were also granted access to a *randomized* price experiment implemented by the lender in July 2006. The experiment was performed on the *existing* clients as part of the lender's risk pricing practice so that it is *not* a solicitation based market experiment. Such unique data and the experimental setting allow us to identify the causal effect of borrowing cost on credit demand for the inframarginal loans. Second, the nature of the experimental design also allows us to investigate the *heterogeneity* in interest rate responses along a number of interesting domains.

Third, our study concerns a developed economy (the UK) with a highly sophisticated credit market. Lenders in countries like the UK have access to advanced risk pricing technologies. Thus our analysis provides novel evidence on the functioning of subprime lending markets in developed economies, on the prevalence of credit constraints and on the role of credit card debt in consumption smoothing among subprime consumers in such economies. This evidence is critical to the development of public policy with respect to household finance in the UK and other developed economies[3]. Our final contribution is methodological. Access to the lender's experiment provides us with full information on the way in which interest rates changed, i.e., gives us the proper counterfactual. Therefore, we can assess the degree of bias caused by the endogeneity of interest rates in estimating credit demand elasticities.

Besides the availability of experimental variation in borrowing rates, the distinct characteristics of subprime borrowers such as low income and impaired credit history make this population a particularly interesting one when it comes to estimate the sensitivity to borrowing rates. For one thing, individuals in our data set are very likely to be net borrowers[4]. A borrower who

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from here on.

[3]There is now a sizeable academic literature on estimating credit elasticities in developing countries. Using a field experiement, Karlan and Zinman (2008) and using between-branch variation, Dehejia et al (2012) provide evidence on the size of credit demand elasticities in South Africa and Bangladesh respectively. Karlan and Zinman (2008) estimates modest interest rate sensitivity of the demand for new term loans in South Africa, with demand apparently more sensitive to loan maturity (that is, minimum payments). Dehejia et al (2012) estimate subtantial interest rate sensitivity among the poor.

[4]Technically speaking, a net borrower is a consumer whose indifference curve is located at the steeper portion of the intertemporal budget line. For such an individual income and substitution effects generated by a change

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[5]. 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[6].

Before carrying out our empirical analysis, we develop a simple dynamic model of consumption. This model guides our analysis (for example, it motivates our choice of an appropriate outcome variable) and our interpretation of the results. Moreover, it is important to be sure that the experiment we study has the statistical power to reject economically interesting hypotheses. To that end we calibrate the model with plausible parameters values taken from the prior literature on intertemporal consumption and saving choices. This calibration tells us what magnitude of response we might expect from the experimental treatments (which are interest rate changes.) We then compare these to the statistical "minimum detectable effects" that we calculate given our experimental design (the sample size and the allocation of individuals to treatment and control groups.) This exercise confirms that, particularly for individuals who tend to utilize their credit limits fully, the experiment has sufficient statistical power to detect the responses suggested by standard theory and at plausible parameter values.

We find that subprime credit card borrowers do not generally reduce their demand for credit when subject to an increase in interest rates. However, those who tend to utilize their cards

in interests rates go in the same direction, i.e., the sign of the theoretical prediction is unambiguous.

[5]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.

[6]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).

fully but either have higher reported income or other borrowing opportunities (other credit cards) make some adjustments on the retail purchase side. We find about £60 to £80 reduction in retail purchases over the three months following the interest rate increases for these types of borrowers. This reduction in retail purchases does not go together with an increase in repayment towards the outstanding credit card balance. Hence, we find no evidence of an overall reduction in credit card debt for them. The only subgroup of borrowers for whom we find a statistically significant response are high risk, high utilization borrowers who tend to use their cards for cash advances (a very costly form of borrowing). When faced with an interest increase of 3 percentage points they tend to reduce their borrowing by about £100 over three months.

An important and policy relevant finding of our paper is that increasing borrowing costs lead subprime credit card borrowers to accumulate additional debt over time. This follows from their insensitivity to interest rate increases. We find that treated borrowers accumulate, on average, £16 of extra debt (relative to the control group) over one quarter. What makes this finding even more important is that estimating a standard credit demand equation with the non-experimental variation in the data does not uncover this effect. The effect is not revealed by a non-experimental analysis even when we condition on a rich set of control variables and on individual fixed effects. While the experimental estimates tell us that treated individuals accumulated an extra £16 of debt in one quarter, a researcher that does not know the experimental structure of the data would estimate a decline in debt as much as £32.

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 guide our empirical analysis. Data and the experimental design are explained in detail in section 4. We present and discuss the experimental estimates in Section 5 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. These cards were responsible for 22.4% of the total consumer transactions, which stood at £540 billion in 2007 (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)[7]. 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 highest observed rate around 70% [8]. A typical nonstandard borrower usually starts with a very small credit limit like £150 and the credit limit generally remains around £500[9].

The term "subprime" refers to a subsection of the nonstandard market in the UK. This

[7] 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.

[8] 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.

[9] 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.

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)[10]. 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 to the individuals in our data set. The model serves two main purposes. First, it motivates our choice of measure of credit card borrowing and facilitates the interpretation of our empirical results. Second, we calibrate the model with common parameter values in order to work out expected responses to the interest rate changes in the experiment we study. We compare these predicted responses to the statistical power of the experiment in order to confirm that our experiment and data have power against economically plausible hypothesis.

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 problem in the usual way:

[10] 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.

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

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 transactions made on credit between periods 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 $t+1$[11]. This is not true if NT includes cash advances, in which case the interest charges resume as soon as the cash advance was made. Define 'net new borrowing' NNB_{t+1} as new monthly transactions 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 transactions), κ is the fraction used to calculate required minimum payment, and θ is a known amount to be paid if $\kappa B_t < \theta$.

[11] 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

$$\kappa = 3\% \text{ of monthly balance}$$
$$\theta = £5$$

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 the internal rather than the 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. Note however that the payment variable can be decomposed as

$$P_{t+1} = Max[\kappa B_t, \theta] + DP_{t+1} \tag{5}$$

where κB_t is the required minimum payment that is determined by the statement balance (and therefore interest rate sensitive), and DP_{t+1},which is the 'discretionary payment' made over and above the minimum payment required. The 'discretionary net new borrowing' $DNNB_{t+1} = NT_{t,t+1} - DP_{t+1}$ is our variable of interest, because it represents the behavioral changes. It is purged of the purely mechanical increase in required minimum payments that would be associated with an increase in debt. Note that with a binding minimum payment, discretionary payments (DP) are zero and discretionary net new borrowing is simply equal to the net new transactions (NT)[12].

An individual who is a net borrower is expected to lower his net new borrowing when faced

[12] It is true that this simple model does not differentiate between cash advances and purchases. However, the empirical strategy, as we will explain later, will accommodate for the reality of cash advances. The empirical analysis is based on a comparison of discretionary net new borrowing (DNNB) between treatments and controls. Hence, it does not matter how the outstanding debt was accumulated in the first place. The question is that whether the borrower reacts to the rate increase by lowering DNNB (either by lowering purchases and cash advances or by increasing the discretionary payments).

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 transactions on credit, $NT_{t,t+1}$, or by increasing his monthly payments, P_{t+1} (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

$$C_{t,t+1} = Y_t + NT_{t,t+1} - P_{t+1} \tag{6}$$

where Y_t is monthly income (likely to be stochastic). We expect however, no sensitivity of $DNNB_{t+1}$ to increases in interest rates for the individuals who are constrained by their credit limits.

Note that the model does not assume that agents cannot save. However, the model does assume that there is a single liquid asset in which agents can go long or short (but obviously not in both, and likely at different interest rates). Given this set up, agents will not borrow and save at the same time. As the agents we study all have debt, we assume that the relevant part of the budget constraint is the "borrowing" part. The model is written in terms of debt (Equation 2), but could equally have been written in terms of assets (which would be negative for our agents). In reality individuals hold more than one asset. Our agents may hold long positions in illiquid assets such as housing, but the illiquidity of these assets implies that they will have little effect on short responses to interest rate changes.

In what follows we implement our empirical strategy using $DNNB$ as our main outcome variable. We will also use NT and DP.

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. The interest rate changes were communicated to the individuals in treatment groups in September 2006. In January 2007, another experiment was launched 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, that is October, November and December 2006.

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 that 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 Data Monitor UK Plastic Cards 2008 Report). 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 did not change credit limits.

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 balance appearing on the June 2006 statement minus the payments made by the due date applied to the balance[13]. 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 of£552. This is quite a large figure given a monthly interest rate of about 2.5% [14]. 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, and 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[15]. Individuals were allocated into cells according to their utilization rates and behavioral scores as at July 2006. After the allocation, the randomization was performed within cells. Such designs are very well known in the statistical, medical and experimental economics literatures. Simple randomization to treatment and controls is rarely employed in real randomized control trials for a number of reasons. For example, block designs reduce the variance of the experimental estimates (see e.g., List et al. , (2011), or Duflo et al., (2008)). This design implies that within cells, there is no selection problem, and conditional on cell, interest rate changes are completely exogenous.

Table 2 presents the cell design, the type of treatment received and the sample sizes of each cell[16]. For example, cell 1 contains individuals who had high utilization rates and low behavior scores (high default risk) in July 2006. In this cell, 337 individuals received a 3 percentage

[13] 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.

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

[15] 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.

[16] 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.

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. It is worth mentioning 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.

It is worth clarifying two aspects of this design before moving to the implementation. First, within each block the allocation to treatment is not one half of the sample. It simply means that the random probability of treatment is not one half. In fact, the experimental literature shows that a 50-50 allocation of subjects to treatment and control groups is typically not statistically optimal (see, e.g, List et al., (2011)). It is only optimal if the effect of the treatment is homogeneous. Second, the treatment is different for different cells (some with increases of 3 percentage points, some with increases of 1 percentage point, one with an interest rate cut.) This means that each block can be treated as a separate experiment.

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 outstanding, interest rate, credit limit, income, age, behavior score, utilization rate and statement balance. We also performed distribution equality tests using the 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. Alternatively, if the treatment caused voluntary closures our treatment effect estimates may be biased toward finding sensitivity. With respect to the latter we checked carefully and find that no account was closed within the sample period. For the former, recall 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.

4.3 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[17].

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

$$U(C) = \frac{C^{1-\gamma}}{1-\gamma} \tag{7}$$

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).

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

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:

$$\ln C_t = -\frac{1}{\gamma}\ln\beta - \frac{1}{\gamma}\ln(1 + r_{t+1}) - \frac{1}{\gamma}\ln E_t[\lambda_{t+1}] \tag{9}$$

Differentiating the above equation with respect to $\ln(1 + r_{t+1})$, we obtain:

$$d\ln C_t = -\frac{1}{\gamma}\left[1 + \frac{\partial \ln E_t[\lambda_{t+1}]}{\partial \ln(1 + r_{t+1})}\right] d\ln(1 + r_{t+1}) \tag{10}$$

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

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

lower the future consumption (increase the future marginal utility of consumption, implying a negative income effect) so that

$$\frac{\partial \ln E_t[\lambda_{t+1}]}{\partial \ln(1+r_{t+1})} > 0 \tag{11}$$

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

$$|\Delta \ln C_t| > \left| \frac{1}{\gamma} \Delta \ln(1+r_{t+1}) \right| \tag{12}$$

As a simple illustration, if, based on the micro evidence[18], 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 $(1+r_{t+1})$ is expected to reduce current consumption of unconstrained borrowers by *at least* £10.5[19]. 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.

5 Results

5.1 Experimental Estimates

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 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

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

[19] 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 effect is realized at the time when interest rate change is communicated. Therefore, expected economic effect after the implementation is only the substitution effect.

is no longer the proper choice variable for a given month.

Guided by the standard intertemporal theory of consumption outlined in Section 3, the following equation forms the base for our estimation:

$$DNNB_{TR,i} = \alpha + \beta TR_i + \varepsilon_i \tag{13}$$

where $DNNB_{TR,i}$ denotes the discretionary net new borrowing of individual i in the group of treated. TR_i is the treatment dummy which takes the value of 1 if individual i is in the treatment group and 0 if the individual is in the control 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 gives us an unbiased estimate of the *average* effect of treatment on the treated (AETT). The coefficient α is the mean value of the discretionary net new borrowing of the control group. Of course, one can also add other control variables to the above regression. However, care should be taken in order for the internal validity results to hold. Later on, we will devise control variables that are pre-determined relative to the treatment in order to explore the heterogeneity in treatment effects.

Going back to equation (13), for individuals who are currently liquidity constrained, we expect that the estimated average treatment effect β (differences in the means between the treated and the control) 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 $DNNB_i$ in this framework) when faced with an increase in interest rates. This is because 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 is 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 the interpretation of the experimental results is that the type of treatment differs (slightly) across cells. For cell 1, for example, the estimate of equation (13) measures the 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 an overall average treatment effect for the entire sample. However, we will be able to do this when we pool all cells and impose linearity in the next subsection. Note also that the percentage increase in the interest rate for an individual in a given cell will depend on her pre-treatment interest rate. For example, in cell 1 where the treatment group received a 3 percentage point increase, an individual with 30% pre-treatment rate will face a 10% increase in his borrowing rate.

In order to estimate the average treatment effect, the natural starting point is to compare the means across individuals by running the following regression for each cell:

$$DNNB_i = \alpha + \beta TR_i + \varepsilon_i. \tag{14}$$

where $DNNB_i$ denotes *total* discretionary net new borrowing made in the 3 months following the implementation of the experiment, $DNNB_i = \sum_{t=1}^{3} DNNB_{it}$. Then, to see if there is any time-dependent (delayed) response to the interest rate changes, we estimate the average treatment effect by months by running the following regression:

$$DNNB_{it} = \alpha + \beta_1 TR_i + \beta_2 November + \beta_3 December + \beta_4 November * TR_i + \beta_5 December * TR_i + \epsilon_{it} \tag{15}$$

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 (autocorrelated errors) and the possibility of heterogenous treatment effects (heteroskedasticity). Table 6 presents the average treatment effects and minimum detectable effects for each cell. The first column of the table gives the mean value of the discretionary 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 $DNNB$ for the control group (α) in the high utilization cells ranges from £25 to £50; and it is much higher in the mid utilization and low utilization cells. This tells us that much more new borrowing takes place in the low utilization cells compared to the high utilization cells. The question is whether there is any difference between the treated and the control within cells. As it can be seen in the second column of the table, we do not detect any statistically significant difference in the discretionary net new borrowing between the control and treated in any cell. How can we interpret this result?

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 the total $DNNB$ due to the treatment of a 3 percentage point interest rate increase was greater than £51.8, 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 4.3. 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 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. The estimated mean difference is higher for cell 1 (-£25.2) 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 (£0.14 and -£4.7 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)[20].

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. 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 due to small cell sizes. 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 £24. The minimum detectable effect for this cell is £68.9 implying that it is very unlikely for us to detect a true effect. We do not feel confident about the results obtained from cell-by-cell estimation for the low utilization cells. Next, we investigate the sensitivity of borrowing demand to interests rate changes by pooling across all cells, and thereby substantially increase the precision of our estimates.

[20] We also repeated this estimation for variable D_t, debt carried forward from one month to the next and found no effect.

5.2 Pooled Experimental Results

An alternative utilization of the experiment would be to pool across all cells and estimate a linear credit demand equation in the following form:

$$\Delta DNNB_{i,t} = \sum_{j=0}^{K} \beta_j \Delta r_{t-j,i} + \alpha' CellDummies + \pi' Interactions + \varepsilon_{i,t} \tag{16}$$

or, to estimate interest rate elasticities of credit card debt, $D_{i,t}$ or $D_{i,t}/L_{i,t}$ (to control for the supply effect):

$$\begin{aligned} \Delta D_{i,t} &= \sum_{j=0}^{K} \beta_j \Delta r_{t-j,i} + \alpha' CellDummies + \pi' Interactions + \varepsilon_{i,t}, \qquad (17) \\ \Delta \left(\frac{D_{i,t}}{CL_{i,t}} \right) &= \sum_{j=0}^{K} \beta_j \Delta r_{t-j,i} + \alpha' CellDummies + \pi' Interactions + \varepsilon_{i,t} \qquad (18) \end{aligned}$$

where the change in interest rates $\Delta r_t = r_t - r_{t-1}$. The lags are included to account for a delayed response to interest rate changes. Note that the differencing takes out the 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 ($j = 0, 1, 2$).

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 (as at July 2006) and their interactions with interest rate changes isolates the experimental variation. With a less than fully saturated model however, estimation of the above equations is subject to a standard omitted variable bias even when we control for the utilization and the behavior score (and their lags) since all the omitted interaction terms are, by design, correlated with interest rate changes. Hence, we estimate above equations by conditioning on cell dummies and their interaction with interest rate changes to control for across cell variation in interest rates.

Table 8 presents the estimates of interest rate elasticities using the pooled data. The first column refers to the whole sample whereas the second and the third columns are based on the high utilization and low utilization cells respectively. Here, the results of the whole sample does not tell anything different than previous analysis: There seems to be no debt reduction or a decline in discretionary net new borrowing in the face of an increase in interest rates. However, some striking results appear when we condition on utilization rates. Recall that we could not establish any interest rate response in low utilization cells and we could not draw any conclusion from this finding simply because we lacked the required statistical power. However, pooled results presents a very clear picture: Even though there is still no evidence of a reduction in net new borrowing, we observe that individuals with low utilization accumulate (instead of decummulate) credit card debt when faced with interest rate increases. The last column of Table 8 show that these individuals accumulate about £16 of extra debt 3 months following a 1 percentage point increase in interest rates. This result is statistically significant.

This is a very important and policy relevant result. Our theoretical model suggest that since individuals who have not fully utilized their credit limit are unlikely to be constrained, they should lower their borrowing. In fact, they should pay off their debt faster when faced with higher interest rates. We see the exact opposite of this prediction in the table. The subprime credit card borrowers do not, on average, decummulate debt in the face of increasing borrowing costs. On the contrary, for borrowers with extra borrowing capacity, higher interest rates lead to higher debt over time. What is perhaps equally striking is that it is impossible to detect this results without the complete knowledge of the experiment. Later in Section 6, we show that the econometrics applied to these data without utilizing the experiment can give a very misleading result even if one uses fix effects estimators to circumvent the endogeneity of interest rates.

5.3 Other Outcome Variables

Our main outcome variable of the total discretionary net borrowing ($DNNB_i$) is composed of total transactions (NT_i) and total discretionary payments (DP_i). The variable NT_i can be further composed in to retail purchases (RP_i) and cash advances (CA_i). Therefore we can also estimate the average treatment effect using these components separately as our outcome variables. This analysis can potentially give us a more detailed picture of all adjustments made in response to interest rate increases. Note that the insensitivity result we obtained using $DNNB_i$ does not necessarily translate into the insensitivity of $DNNB_i$'s components. For example, it is possible to obtain a significantly negative average treatment effect for retail purchases (individuals lower their purchases on credit), while having the average treatment effect for $DNNB_i$ statistically zero[21].

Table 9 presents results for the 3-month total retail purchases (RP_i) and the total discretionary payment (DP_i) for each cell. This table confirms the previous results based on $DNNB_i$. There is no evidence that individuals lower their retail purchases on credit or increase their discretionary payments when faced with interest rate increases. Average treatment effects are statistically and economically insignificant. We carry out the same analysis for the outcome variable cash advance (CA_i) and finds no statistically significant response. We now ask the question whether the overall mean analysis we have carried out so far conceals some interesting heterogeneity in responses. The next subsection aims to answer this question.

5.4 Heterogeneity in Treatment Effects

Based on the discussion above, the apparent insensitivity to interest rates by borrowers who fully utilize their credit limits can be interpreted as evidence of binding liquidity constraints

[21] Suppose that we have two otcome variables Y_1 and Y_2. We estimate the AETT seperately as $Y_1 = a + bT + \epsilon$ and $Y_2 = c + dT + u$. Let us further assume that we found b (or d or both) is statistically significant. It is still possible that the AETT using the outcome variable $Y_1 - Y_2 = (a - c) + (b - d)T + \epsilon - u$ is statistically zero. Similarly, it is possible to estimate statistically zero b and d and statitistically significant $(b - d)$. The later is especially possible if there is some fixed effects in the first two regressions. Then the differencing may increase the precision by removing the fixed effect.

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 credit card other than the one issued by our lender may indicate a borrowing limit the individual faces. 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), although it would seem so in our sample due to the observed payment. Alternatively, these individuals can use their other cards for their retail purchases.

Unfortunately, we have no way of knowing the nature of the payments, whether it is a balance transfer to another card (with lower interest rate) or a genuine payment, made toward balances. Both actions are likely to appear as positive treatment effects when we use discretionary payments (DP) as the outcome variable. Note that balance transfers would bias our results toward finding sensitivity, and the estimated magnitude of consumption reduction should then be considered as an upper bound.

5.4.1 Conditioning on Income and the Presence of Other Credit Card(s)

In what follows, we repeat our previous analysis conditional on some control variables.[22]We begin with variables such as the borrower's income and the existence of other credit cards. We generate a high income dummy that takes the value 1 if the individual reported income is higher than £20,000. Similarly, we generate the other card dummy that takes the value 1 if the individuals reported to have other credit card(s). Table 10 presents estimated average

[22]For each regression, we performed *placebo* regressions on pre-treatment periods to assure that the treated and the control are statistically similar conditional on the variable of interest. For example, for the treatment effect regression of high income individuals, we first check that the high income individuals in the treatment and the control groups are similar (in a given cell) before the implementation of the experiment.

treatment effects conditional on holding other credit cards and having high income[23]. The interaction of the high income and the other card dummy result in too small cell sizes for cell 2, 3 and 5 therefore we exclude them for this analysis. To make the discussion easier, Cell 9 is also excluded from the table as borrowers there face a distinct treatment (a 3 percentage point decrease instead of an increase). The results based on $DNNB$ in this table are generally very similar to our previous results: We find no evidence of a reduction in $DNNB$ in any cell.

The picture changes markedly however when we take a closer look at the components of $DNNB$. As shown in Table 11, low income individuals who reported no other credit card do not reduce their retail purchases; the average treatment effects are both economically and statistically insignificant. However, as shown in the last two columns of the same table, high utilization individuals who reported higher income (cells 1, 4 and 7) lower their retail purchases significantly when faced with an interest rate increases. The last column of the table indicates that the treated higher income individuals who have other credit cards in cell 4 spent about £64 less than the control (with the same characteristics) over the three-month period. The amount is about £68 for cell 7. A significant reduction in purchases is also estimated for high income individuals who reported no other card in cell 7 (by about £60.5).

Interestingly, as shown in the column 3, high utilization individuals in the highest risk group (cell 1) also lower their purchases significantly (by about £79 over 3 months). Note that these individuals reported income higher than 20,000 and no other credit card. We do not observe a similar response by individuals who report higher than £20,000 income and holding other credit cards in this cell. This surprising result could be that these high risk individuals may have already borrowed up to their credit limits on their other credit cards.

Note that individuals who reportedly have multiple credit cards may be substituting among cards rather than lowering consumption (debt reduction). The results we obtain by using the

[23] We also condition on age, employment status and home ownership. Unreported results suggest no significant effect of these variables.

discretionary payment (DP) supports this argument. Table 12 shows the results for DP. Here, we do not see any evidence of debt reduction, which should appear as significantly positive average treatment effect. In fact, for cells that we estimate significant reduction in retail purchases, we also estimate similar amount of reduction in discretionary payment explaining the statistically insignificant results for $DNNB$. We also repeat this analysis for the CA (cash advance) variable and find no significant treatment effects in any cell conditional on income and having multiple credit cards (unreported). We also condition on only the income and only on the other credit cards dummy and find no significant effects for any cell except for cell 7. For this cell, we find that individuals with income higher than £20,000 lower their retail purchases by about £45 when faced with a 1 percentage point increase in interest rates.

5.4.2 Minimum Payers and Cash Advancers

Next, we explore heterogeneity in responses across other dimensions. We begin by noting that individuals in our data have different payment habits. In particular, a significant proportion of the individuals tend to pay the minimum required amount (or little over it) every month. Sensitivity results may be different for these "*corner*" individuals. We then note that a significant proportion of our sample use their credit card for cash advances CA (as well as retail purchases). Rates applied to the cash advances are different (typically higher) than those applied to the retail purchases, and were not affected by the experiment we analyze here (only the retail rates were changed). However, the behavior of individuals who opt for this high cost borrowing may be significantly different from those who stay away from it.

Table 13 presents the proportion of minimum payment payers and cash advancers prior to the implementation of the experiment. The Minpayer dummy is set to 1 if the individual consistently made payments *less* than 5% of his statement balance over three months prior to October 2006. It is set to zero otherwise. Similarly, the Cash advancer dummy is set to 1 if the individual made *at least* one cash advance in one of the months prior to the implementation and zero otherwise. It is immediately clear from the table that the majority of the individuals in the high utilization cells pay more or less the required minimum payment every month and no more. This behavior seems to be independent of credit worthiness of the borrower; Cells 1, 4 and 7 do not appear to be different. As the utilization rate goes down we see higher proportion of discretionary payers. The picture for the cash advancers is not as clear; although the lowest utilization cells contain a smaller proportion of cash advancers we generally do not see any clear relationship between utilization/risk and propensity to make cash advances.

When we repeat our analysis conditional on the Minpayer dummy we find no significant average treatment effect for any cell using any outcome variable (unreported) . This is not surprising as most individuals in our sample are minimum payment payers[24].

Table 14 presents the results conditional on the Cash advancer dummy. The surprising result is that the cash advancers in cell 1 show a significant sensitivity to the treatment of 3 percentage point increase in interest rates. The discretionary net new borrowing is lowered by about £100 by this group over the three months following the experiment. This response is economically and statistically significant. Looking at the other components of $DNNB$, that is RP and DP, we find no significant effect on either variables. Why do cash advancers in the highest risk group lower their discretionary net new borrowing? Our experiment altered interest rates on purchases, but not cash advances. Cash advancers of course use their credit card for purchases as well and so are affected by the interest rate changes (if in the treatment group). Why they appear to be more responsive to interest rate changes is a puzzle. One possible explanation is that they may be more aware of the interest costs because of heavy utilization. But this is only speculation, and an explanation for this puzzle must await further research, and in all probability, further experiments.

[24] We also re-estimated average treatment effects using the experimental variation after excluding convenience users from the sample. Convenience users are defined as people who carried forward zero debt 3 consecutive months prior to the implementation, that is from July to September 2006. We find that the results in the paper are robust to this exclusion (we did this for variables DNNB, NT and DP). Of course, this is a direct result of the fact that the experimental design balances convenience users between treatment and control groups.

Table 14 also shows a significant but rather small reduction in retail purchases by non-cash advancers in cell 7. This result is less surprising as individuals in this cell, being the most credit worthy, did make some significant retail purchase reductions (recall Table 10). Recall that in cell 7, individuals with over £20,000 reported income reduced their retail purchases by about £60 over the three months. It is then no surprise that we observe the non-cash advancers in this cell to reduce retail purchases as they are most likely to be in the higher income group.

Overall, our findings suggests that subprime borrowers are not a homogenous group. Even though there is some evidence of liquidity constraints in the strong sense of the term, these borrowers do exhibit some sensitivity to borrowing rates. However, the sensitive borrowers tend to be those with relatively higher reported income and those who have access to other borrowing opportunities (other credit cards). Therefore although a large (3 percentage point) increase in interest rates triggers a significant reduction in credit demand among a particular group (high risk, high utilization cash advancers), our results are generally suggestive of debt re-shuffling rather than outright consumption (or debt) reduction. We will confirm this in the next section when we pool all individuals and estimate credit demand equations.

6 Can Econometrics Replicate the Experiment?

Credit demand equations have been estimated on nonexperimental data, usually exploiting some quasi-experimental variation. Attanasio et al (2008), Alessie et al (2005) and Gross and Souleles (2002) are exemplary studies of this sort.[25]. These studies 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 exploiting only exogenous variation in interest rates when estimating such equations.

Interest rate experiments are common practices for specialized credit card issuers, and form

[25] 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.

part of their advance risk pricing strategies. 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 experiment, but when interest rate changes are applied to certain accounts based on some specific information of the lender that is not available to researchers.

In our data, in the absence of the cell identifier variable, we would observe interest rate changes of 1, 3 and -3 percentage points for some accounts but we would not observe the proper comparison group (individuals with no interest rate changes) for these individuals[26]. Using both the time series and cross section variation, we can estimate borrowing demand equations as in Section 5.2. However we now assume that we do not have information on the lender's pricing experiment (i.e., we do not observe the cell identifiers). Therefore we appeal to a large set of controls to mitigate the effect of endogeneity of interest rates. 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), and account age. We also estimate a fixed effects model to account for individual-specific trends in borrowing demand.

Note that these equations can also be estimated using only the cross-sectional variation in interest rates in a given month. 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 by a good instrument. We can illustrate this important point since we do have

[26] 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.

the perfect instrument: the experiment.

Table 15 presents the estimated 1 month and 3 month sensitivities for the credit card debt (D), the debt normalized by the credit limit (D/CL) and the discretionary net new borrowing ($DNNB$). 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 the credit card debt for both specifications; the estimated 1 month decline in the 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 declines 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)[27].

We tried several other controls (income, individual's age, employment status etc.) and established that the finding of significant debt reduction in 3 months is quite robust. This is also true when we normalize the debt by the credit limit. For the 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 ones depending on which controls we use. This finding is enough in itself to cast doubt on nonexperimental estimates without convincing exogenous variation in the interest rates or the interest rate changes.

[27] 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.

The last column in Table 15 presents the experimental estimates (same as Table 8 column 1 in section 5.2). We show only specification 1 as, not surprisingly, the other specification (fixed effects estimation) give 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 for the debt/new borrowing with respect to borrowing rates when we isolate cross-cell variation in the 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 16 presents the results. The nonexperimental regressions include all the control variables described earlier and account specific fixed effects. We find virtually no difference (economically, or statistically) between the estimates obtained with the 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 16). 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 (also presented in Table 8 in section 5.2.

Recall our discussion in Section 3 that when faced with an increase in interest rate, individual's debt automatically increases due to the additional interest charges unless net new borrowing declines. Since the net new borrowing is positive in the 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 the results obtained with mixed within variation (pooled regressions with experimental variation) and cross cell variation (pooled regressions without experimental variation) are materially very different (£32 decline versus £16 increase in 3 months).

7 Conclusion

We estimate the sensitivity of credit demand to interest rates. We do this with a unique data set on monthly credit card transactions from a subprime credit card company that includes a randomized interest rate experiment. We first develop a simple dynamic model of consumption; this model guides our approach to the data and our interpretation of the results. We also use a calibration of the model to quantify the theoretically expected responses to the experimental treatment. We then compare these predicted responses to the minimum detectable effects in the experiment. This demonstrates that the experimental design has sufficient statistical power to detect economically plausible responses. We find that subprime borrowers on average do not respond to changes in interest rates in the way the intertemporal theory suggests. On the contrary, we find that subprime borrowers with spare borrowing capacity further accumulates debt when faced with interest rate increases. This follows from their insensitivity to interest rate increases. In the context of current debates over consumer protection, responsible borrowing and financial literacy, this result is an important policy input. Imposing interest rate caps might be an unpalatable option for a policy maker because it could result in credit rationing. However, the results of this paper might be advanced in support of regulating credit limit increases (particularly those initiated solely by the lender).

We also use these data to illustrate the importance of isolating exogenous variation in interest rates when estimating credit demand elasticities. We show that estimating a standard credit demand equation with the nonexperimental variation in our data leads to seriously biased estimates, and that this is true even when we condition on a rich set of controls control variables and on individual fixed effects. 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. The estimated sensitivity to interest rates derived from the nonexperimental variation in the data is quite misleading: it hides the fact that subprime credit card borrowers accumulate debt instead of decumulating in the face of increasing borrowing costs. This methodological exercise has an important lesson for researchers and regulators.

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.

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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 (0–100%)	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

Behaviour score (Bscore)

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

Cells	Control's mean $DNNB$	Average Treatment Effects (β): $DNNB_i = \alpha + \beta TR_i + \varepsilon_i$ Average Treatment Effect β	Abs. Min. Det. Effect	Abs. Min. Economic Effect
Cell 1 (3pp)	28.1	-25.2	51.8	74 or 0
	(2.02)	(1.21)		
Cell 2 (3pp)	70.5	-23.9	79.7	74
	(2.94)	(.75)		
Cell 3 (3pp)	113.5	15.9	156.9	74
	(2.90)	(.25)		
Cell 4 (1pp)	25.9	.14	21.9	24 or 0
	(4.25)	(.02)		
Cell 5 (1pp)	90.7	2.8	68.9	24
	(4.05)	(.10)		
Cell 7 (1pp)	48.2	-4.7	19.9	24 or 0
	(8.70)	(.58)		
Cell 8 (1pp)	126.1	22.4	41.1	24
	(8.66)	(1.36)		
Cell 9 (-3pp)	158.7	18.7	62.8	24
	(13.0)	(.74)		

Notes: Absolute t-ratios calculated with robust standard errors in parentheses. The dependent variable $DNNB_i$ is total discretionary net new borrowing over the months of October, November and December 2006. 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			
$DNNB_{i,t} = \alpha + \beta_1 TR_i + \beta_2 Nov + \beta_3 Dec + \beta_4 Nov * TR_i + \beta_5 Dec * TR_i + \varepsilon_{i,t}$			
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: Pooled Experimental Results

Treatment Effects (Pooled)	Whole Sample	High Util	Low Util
1 month sensitivity, β_0, $(D_{i,t})$	1.3	.83	3.4
	(1.9)	(2.7)	(3.4)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, $(D_{i,t})$	2.5	-4.6	15.9**
	(5.1)	(6.3)	(8.1)
1 month sensitivity, β_0, $(D_{i,t}/CL_i)$	$-.001$	$-.001$	$-.001$
	(.002)	(.003)	(.004)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, $(D_{i,t}/CL_i)$	.003	$-.002$	.012**
	(.004)	(.005)	(.005)
1 month sensitivity, β_0, $(DNNB_{i,t})$	2.7	3.8	2.6
	(2.9)	(4.0)	(5.2)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, $(DNNB_{i,t})$	-1.4	-10.8	14.4
	(5.6)	(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. Estimates obtained from the regressions of $\Delta D_{i,t}$ ($\Delta(D_{i,t}/CL_i)$ and $\Delta DNNB_{i,t}$ respectively) on the change in interest rates (and its 2 lags) by using the cell information, that is, adding cell dummies and their interactions with all other right hand side variables.

Table 9: Experimental Estimates
Average Treatment Effects for Outcome Variables RP_i and DP_i

	Control's RP	AT Effect (RP)	Control's DP	AT Effect(DP)
Cell 1(3pp)	88.0	-3.5	59.8	21.7
	(7.2)	(.14)	(4.6)	(.04)
Cell 2 (3pp)	158.8	-41.1	87.2	-17.2
	(7.0)	(1.4)	(3.8)	(.60)
Cell 3(3pp)	200.9	28.3	87.4	12.3
	(4.7)	(.41)	(3.4)	(.30)
Cell 4 (1pp)	100.4	-1.9	74.5	-2.0
	(17.2)	(.22)	(13.6)	(.24)
Cell 5 (1pp)	218.0	-.98	127.3	-3.8
	(8.6)	(.03)	(5.7)	(.15)
Cell 7(1pp)	154.3	-9.7	106.1	-5.1
	(32.2)	(1.4)	(19.4)	(.05)
Cell 8(1pp)	345.9	-5.6	219.8	-27.9
	(23.5)	(.30)	(13.8)	(1.6)
Cell 9(-3pp)	352.1	-4.0	193.5	-22.7
	(25.5)	(.14)	(16.5)	(1.0)

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

Table 10: Experimental Estimates
Outcome variable: Total Discretionary Net New Borrowing: $DNNB_i$

	Inc<20000, No card	Inc<20000, Card	Inc>20000, No card	Inc>20000, Card
Cell 1(3pp)	5.1	14.3	-51.6	-141.4
	(.16)	(.42)	(1.49)	(1.16)
Cell 4 (1pp)	6.0	15.8	-16.5	-5.5
	(.43)	(.91)	(.35)	(.22)
Cell 7(1pp)	-16.9	16.4	5.7	-45.6
	(1.2)	(1.0)	(.20)	(1.2)
Cell 8(1pp)	-1.9	35.2	62.9	-1.3
	(.06)	(1.1)	(.97)	(.02)

Notes: Absolute t-ratios calculated with robust standard errors in parentheses.
Values are in British Pounds (£). $**$: significant at 5%, $*$: significant at 10%

Table 11: Experimental Estimates
Outcome variable: Total Retail Purcases (RP_i)

	Inc<20000, No card	Inc<20000, Card	Inc>20000, No card	Inc>20000, Card
Cell 1(3pp)	8.0	8.4	**-79.2**	-36.1
	(.31)	(.28)	**(2.52)****	(.58)
Cell 4 (1pp)	-3.5	2.2	-.46	**-63.6**
	(.30)	(.14)	(.01)	**(2.25)****
Cell 7(1pp)	-.90	17.0	**-60.5**	**-67.8**
	(.07)	(1.3)	**(1.78)***	**(2.47)****
Cell 8(1pp)	-7.1	34.0	-88.9	-31.5
	(.22)	(1.1)	(1.2)	(.45)

Notes: Absolute t-ratios calculated with robust standard errors in parentheses.
Values are in British Pounds (£). $**$: significant at 5%, $*$: significant at 10%

Table 12: Experimental Estimates
Outcome variable: Total Discretionary Payment (DP_i)

	Inc<20000, No card	Inc<20000, Card	Inc>20000, No card	Inc>20000, Card
Cell 1(3pp)	2.9	-5.9	-27.6	105.4
	(.12)	(.13)	(.67)	(.77)
Cell 4 (1pp)	-9.5	-13.6	16.0	**-58.1****
	(.72)	(.81)	(.41)	**(2.10)**
Cell 7(1pp)	16.1	.61	**-66.2***	-22.2
	(1.4)	(.04)	**(1.8)**	(.63)
Cell 8(1pp)	-5.2	-1.1	**-151.8***	-30.2
	(.15)	(.03)	**(1.9)**	(.43)

Notes: Absolute t-ratios calculated with robust standard errors in parentheses.
Values are in British Pounds (£). $**$: significant at 5%, $*$: significant at 10%

Table 13: Interest Rate Sensitivity of Credit Card Debt
Proportion of Minimum Payment Payers and Cash Advancers

	Minpayer		*Cash Advancer*	
	Treated	Control	Treated	Control
Cell 1 (3pp)	96.4	95.6	24.1	28.1
Cell 2 (3pp)	80.7	82.1	29.7	26.2
Cell 3 (3pp)	41.2	38.5	8.1	16.3
Cell 4 (1pp)	95.6	95.6	31.9	32.4
Cell 5 (1pp)	87.4	84.4	40.5	44.4
Cell 7 (1pp)	94.4	94.5	24.4	24.4
Cell 8 (1pp)	81.4	82.9	31.6	30.0
Cell 9 (-3pp)	49.2	46.3	16.2	16.2

Notes: Values are in percentages. The dummy variable Minpayer=1 if the individual paid less than 5% of her statement balance in all three months prior to implementation (July, August, September 2006). The dummy variable Cash Advancer=1 if the individual made a cash advance in one of the three months prior to implementation. Equality of proportions tests are carried our for all cells and no statistically significant differences between the control and the treated are detected. P-values for the tests are available upon request.

Table 14: Experimental Estimates

	Average Treatment Effects:					
	$DNNB_i$		RP_i		DP_i	
	Cash advancers	Non-cash advancers	Cash advancers	Non-cash advancers	Cash advancers	Non-cash advancers
Cell 1(3pp)	**-99.5**	.51	16.2	-6.0	115.7	-6.5
	(2.04)**	(.02)	(.18)	(.39)	(.95)	(.29)
Cell 4 (1pp)	1.73	-.51	8.2	-6.1	6.4	-5.6
	(.09)	(.06)	(.42)	(.73)	(.30)	(.65)
Cell 7(1pp)	13.9	-10.7	12.6	**-16.9**	-1.3	-6.3
	(.81)	(1.2)	(.80)	**(2.3)****	(.08)	(.72)
Cell 8(1pp)	27.1	20.7	-40.2	11.7	**-67.3**	-9.0
	(.81)	(1.1)	(1.1)	(.63)	**(1.74)**	(.47)

Notes: The absolute t-ratios calculated with robust standard errors in parentheses. The dependent variable $DNNB_i$ is total discretionary net new borrowing, RP_i is total retail purchases, DP_i is total discretionary payment over the months of October, November and December 2006. Values are in British Pounds (£).

Table 15: Interest Rate Sensitivity of Credit Card Debt

	Nonexperimental		Experimental
	Spec 1	Spec2	Spec1
1 month sensitivity, β_0, $(D_{i,t})$	-5.1^{**}	-6.8^{**}	1.3
	(1.8)	(1.7)	(1.9)
3 month sensitivity, $\sum_{i=0}^{2}\beta_i$, $(D_{i,t})$	-11.7^{**}	-24.2^{**}	2.5
	(3.8)	(4.6)	(5.1)
1 month sensitivity, β_0, $(D_{i,t}/CL_i)$	$-.005^{**}$	$-.006^{**}$	$-.001$
	(.001)	(.001)	(.002)
3 month sensitivity, $\sum_{i=0}^{2}\beta_i$, $(D_{i,t}/CL_i)$	$-.008^{**}$	$.20^{**}$	.003
	(.002)	(.004)	(.004)
1 month sensitivity, β_0, $(DNNB_{i,t})$	$-.03$	-3.8	2.7
	(2.2)	(2.7)	(2.9)
3 month sensitivity, $\sum_{i=0}^{2}\beta_i$, $(DNNB_{i,t})$	-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 $\Delta D_{i,t}$ ($\Delta(D_{i,t}/CL_i)$ and $\Delta DNNB_{i,t}$ respectively) on the change in interest rates (and its 2 lags) and monthly dummies. The second column adds lags of the utilization rate, behavioral score, change in credit limits, account age and account-specific fixed effects. Values in the last column are obtained from the regressions of $\Delta D_{i,t}$ ($\Delta(D_{i,t}/CL_i)$ and $\Delta DNNB_{i,t}$ respectively) on change in interest rates (and its 2 lags) and by using the cell information, that is, adding cell dummies and their interactions with all other right hand side variables.

Table 16: Interest Rate Sensitivity of Credit Card Debt

	Nonexperimental		Experimental	
	High Util	Low Util	High Util	Low Util
1 month sensitivity, β_0, $(D_{i,t})$	.35	-10.3^{**}	.83	3.4
	(2.2)	(2.6)	(2.7)	(3.4)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, $(D_{i,t})$	-6.3	-31.9^{**}	-4.6	15.9^{**}
	(6.1)	(6.5)	(6.3)	(8.1)
1 month sensitivity, β_0, $(D_{i,t}/CL_i)$	.00	$-.01^{**}$	$-.001$	$-.001$
	(.02)	(.002)	(.003)	(.004)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, $(D_{i,t}/CL_i)$	.001	$.02^{**}$	$-.002$	$.012^{**}$
	(.005)	(.005)	(.005)	(.005)
1 month sensitivity, β_0, $(DNNB_{i,t})$	1.4	-6.1	3.8	2.6
	(3.6)	(3.8)	(4.0)	(5.2)
3 month sensitivity, $\sum_{i=0}^{2} \beta_i$, $(DNNB_{i,t})$	-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 $\Delta D_{i,t}$ ($\Delta(D_{i,t}/CL_i)$ and $\Delta DNNB_{i,t}$ respectively) on the change in interest rates (and its 2 lags), monthly dummies, lags of utilization rate, behavioral score, change in the credit limits, account age and account-specific fixed effects. Values in the last two columns (experimental) are obtained from the regressions of $\Delta D_{i,t}$ ($\Delta(D_{i,t}/CL_i)$ and $\Delta DNNB_{i,t}$ respectively) on the change in interest rates (and its 2 lags) by using the cell information, that is, adding cell dummies and their interactions with all other right hand side variables.