nonPRIME101

Report 7-B

# Searching for Harm in Storefront Payday Lending

## A Critical Analysis of the CFPB's "Debt Trap" Data

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### Synopsis

By: Rick Hackett

## Introduction

This synopsis is intended to provide the general reader with a grasp of the purposes, approaches and outcomes of the nonPrime101 study. The report itself is very dense and technical. By reviewing this synopsis and the takeaway boxes included throughout, we hope that the general reader can navigate this report.

## Purpose of Study

Clarity Services, Inc., a consumer reporting agency, has a longer duration, and likely larger data set, than the Consumer Financial Protection Bureau (CFPB) had available for its two published reports on storefront payday lending, including *Payday Loans and Deposit Advance Products* (2013) and *CFPB Data Point*: *Payday Lending* (2014). Clarity can also follow borrowers crossing the street to a different lender, which the CFPB data set did not allow.

The nonPrime101 Small-Dollar Research team thought it would be useful to conduct some of the same tests of borrower use patterns as the CFPB, to see if the outcomes are different with a larger data set, and to see if a borrower's use of multiple lenders changes the outcomes.

We also wanted to study the kinds of questions that the CFPB could not, given the short duration of their sample:

- How long do storefront payday customers use the product from first loan to last loan?
- Do the measures of intensity of use studied by the CFPB change when looking at an entire life cycle in the product?
- Looking at a large number of borrowers over their entire life cycle of use, what is the worst case scenario (the longest sequence of de facto rollovers of a single loan) for each borrower?
- What is the size of the groups who use the product lightly (in short sequences) versus those who use it more heavily (in long sequences)?
- Is there a difference in the rate at which lighter users and heavier users exit the product and are replaced?
- Looking longitudinally over a long period of time, what is the count of light users versus heavy users?

In this synopsis, we briefly answer each of these questions. The report contains detailed answers.

## The CFPB's Current Proposal

This study is prompted by the CFPB's two studies of storefront payday lending, *Payday Loans and Deposit Advance Products*, a white paper published in 2013 (further referenced as white paper), and *CFPB Data Point: Payday Lending* published in 2014 (further referenced as *Data Point*). Those studies form the basis for a pre-rule outline of a regulatory intervention, published pursuant to the Small Business Regulatory Enforcement Fairness Act (SBREFA). The outline was published in March 2015, as part of a required process to discuss the impact of the proposal with small business representatives, before issuing a draft rule. The draft rule is expected in March 2016.

The CFPB has outlined a plan to regulate small-dollar lending that would put the storefront payday industry out of business. The CFPB and industry sources have predicted the rules will cause a 60-70 percent reduction in storefront payday loan volume. No business can survive with that big of a loss in revenue.

The CFPB's basis for the proposal is that existing payday lending is unfair and abusive. These are legal terms that depend on a finding that borrowers are harmed by the product. The CFPB has stated that harm occurs in short-term, small-dollar products because the borrower cannot afford to both make the payment of principal and fees and meet other obligations and cost of living. According to the CFPB, this results in borrowers frequently renewing their loans (for another fee) or repeatedly paying off and immediately re-borrowing a loan. As the reasoning goes, if the re-borrowing occurs in the same pay period that the loan was last paid off, then the re-borrowing is economically the same as a renewal or roll-over. It's borrowing the same money. The CFPB calls a series of loans that have this relationship a loan sequence, and declares there is harm where the cost of loan fees in the sequence "eclipses the loan amount." According to its proposal, the CFPB is willing to allow a sequence of three loans to occur, without compliance with the proposed rule's underwriting requirements. Three fees are not too much to pay. On the other hand, at the going rate of $15 per $100 per pay period, a sequence of seven loans would clearly meet the CFPB's definition of harm, because seven loans cost 105 percent of the principal.

What do we mean by loan sequence in the nonPrime101 study? Since the CFPB theory is that re-borrowing before a new paycheck is received is basically an extension of a single loan, we linked together as sequences all loans taken out in the same pay period that a prior loan was paid off. If a bi-weekly payroll borrower pays off a loan on a payday, any loan taken out before two weeks later is in the sequence. We used the exact pay period of each borrower to make this analysis, whether weekly, bi-weekly or monthly. We label all loan data as Loan 1 (a single loan), Loan 2 (a sequence of loans with the same lender), or Loan 3 (a sequence of loans with more than one lender).



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## The CFPB's Data Supporting Its Proposal vs. Clarity Services' Data in This Study

Clarity has five years of data from 20 percent of the storefront market. Clarity can see the same borrower dealing with multiple lenders. In this study, we use a subset of 72.5 million loans and 4.1 million borrowers over four years. We can also look back six months before the study period to detect recent borrowing. The CFPB studied 15 million loans over one year. Because storefront borrowers are highly likely to stay with the same lender, or use at most two, Clarity can see borrowers enter and leave the market over a market life cycle, which is usually much more than 12 months.

### 1. Measuring Life Cycles

What do we mean by life cycle? nonPrime101 defines life cycle as the number of days between the first loan and the last loan in the data set. It does not indicate the intensity of loan use during that period. We look at intensities when looking at number of loan sequences per borrower and length of loan sequences per borrower.

We found that a group or cohort consisting of all borrowers who had loans in January 2010 (about one million people) had average life cycles of almost exactly two years. We computed this using a measuring period through December 31, 2013 (four years). We used a number of other, longer test periods to measure truncation effect, or the error that can occur when a large number of life cycles actually take more than four years. We found that, although our data is affected by some truncation effect, we have captured full life cycles for at least 85 percent of borrowers. On the other hand, about 10 percent of the borrowers are in the product for more than four years. Based on this testing, we conclude that our data provides an accurate picture of how different groups of users (lighter and heavier) use the product. We also conclude that a one year test is not likely to be accurate for comparing the relative size of lighter and heavier user groups (something the CFPB did not try to measure).

## 2. Measuring Sequences per Borrower

In *Data Point*, the CFPB compared multiple ways to build the test population (sampling methods) to test the number of borrowers with one sequence, two sequences, etc. All other things being equal, fewer sequences suggest less "harm." We applied the same analytical methods as the CFPB to our larger data sample to see if we got results similar to *Data Point*.

When we used the CFPB's relatively short time window to measure use patterns (11 months), we got results very similar to the CFPB studies. However, when we looked at entire market life cycles of storefront payday use, we got different results. Our takeaway is that the white paper suffered from sampling bias and both of the CFPB studies covered too short a time period to get a full picture of borrower use patterns.

We also made use of our unique ability to measure sequences involving multiple lenders (Loan 3). We do not find that borrowing intensity is greatly increased when we add in the effect of using multiple lenders. It does not increase the number of sequences per borrower.

## 3. Number of Loans per Sequence

The counting of number of loans per sequence goes to the heart of the CFPB's theory of "harm," long sequences in which the fees "eclipse the loan amount." We computed the average and the median sequence duration using samples drawn using all of the CFPB methodologies, as well as our additional ability to test borrowers with no loans 90 days and six months prior to the cohort month (January 2010).

We had several significant findings. First, for all but one of the samples, the median sequence duration was two loans in a row. For the method used in the white paper, it was three loans in a row. nonPrime101 defines median as the point at which half of the sample is higher and half lower. In other words, half of all loan sequences are within what the CFPB would define as safe in its recent outline of a regulatory proposal for small-dollar loans. In contrast, the average (or mean) sequence duration was between four and five for our various samplings of new borrowers (those without loans immediately before the cohort month) and between six and seven for the method used in the white paper. The latter sample suggests average borrower experience that approaches the CFPB's "fees that eclipse the loan amount." All other samples do not.

The significant difference between the median borrower experience (two or three loans in a row) and average (mean) experience suggests that a minority of sequences tend toward extreme length, dragging the average up into the realm of where the CFPB believes "harm" exists.

We again applied our unique ability to see if borrowers cross the road to another lender to extend their sequences. They do, but not very much. The difference in mean sequence duration between Loan 2 and Loan 3 ranges from nil to insignificant.

## 4. What's the Worst Case per Borrower?

We then measured the worst case (the longest loan sequence) for each borrower. If a borrower can go up to four years in the product without a sequence of loans in which the "fees eclipse the loan amount," there is a good argument that borrower is not "harmed" (at least not by the price of the extended loan).

The median worst case for all newer borrowers in January 2010, measured over four years, was five loans in a row. For all borrowers in that cohort, it was nine loans in a row. These statistics reflect the fact that, in any given month, 75-80 percent of borrowers are in an extended borrowing experience. They are heavier users. The mean worst case confirms this. For newer borrowers, the mean worst case is around nine loans in a row. For all borrowers in January 2010, it is close to 16 loans in a row.

We looked at outliers to see what sequence durations are dragging the mean (average) so high. At the 90th percentile (the top 10 percent), we find the duration of a single-lender sequence is 26 loans and a multi-lender sequence at 42 loans in a row. These are the worst of worst cases and suggest that there is room for regulatory intervention that has little to do with single-digit sequences.

## 5. How Many Borrowers Show Evidence of "Harm?"

Figure 5 in the report is perhaps the most important graphic. It shows that, for all sampling methods other than the white paper (all methods of looking at borrowers other than one that oversamples heavy users) 60 percent of borrowers never have a worst case greater than six loans in a row. Remember that the average sequence for those borrowers is less than five and the median is two (over four years).

The foregoing view is all based on what is called a static pool, or a group of consumers that is selected once and then followed over a period of time. We saw that how one selects the pool makes a huge difference in finding an inference of "harm". We therefore set out to find out the relative size (over time) of the pools of continuous heavier users and less frequent, lighter users. We did so because the legal issue of whether or not the product is so unfair as to justify banning it will be greatly influenced by the balance of ending "harm" to heavy users versus denying access (another "harm") to lighter users.

## 6. How Many Borrowers Show Evidence of "Harm" in a Longitudinal Pool?

We set out to approximate the relative size of the populations of heavier, continuous users versus less frequent, lighter users, over a four-year period. To do so, we first computed the attrition rate for the continuing users found in our January 2010 cohort. They leave the product very slowly. Fifty percent are still in the product a year later. After that, 80 percent are found a year later and a similar percentage a year later. Twenty-five percent are still in the product at the end of four years.

In contrast, when we sampled the population every December from 2010 to 2013, we found almost complete replacement each year of the group of lighter users with new, lighter users. This is consistent with our finding that lighter users have shorter life cycles.

Using the attrition rates for heavier users that we found in the sample, we then modeled a constant population of 1,000 borrowers over four years, assuming that lighter users would be replaced every year (as our data suggested). This admittedly rough approximation showed that, even though 80 percent of borrowers in any month are heavier users, over a period of time only 60 percent of borrowers are in a group that is likely to have a worst case of more than six loans in a row, and 40 percent are unlikely to experience "harm" of paying more than they borrowed.

We are currently building a statistically valid, random sample, longitudinal pool over four years and will shortly report a precise calculation of this count of possibly "harmed" versus never "harmed" borrowers.

For this final report in the series, Report 7-C, we have constructed a random sample that starts with 1,000 borrowers. The count of "active" borrowers is maintained at 1,000 each month, with a new randomly sampled borrower being added whenever a borrower from the initial group drops out for good (completes the life cycle in the product). This replacement borrower is a new borrower who appears for the first time in the data set in the month when the replaced borrower exits the data set. Using the constant-sized sample, we can precisely measure and report about the groups who use the product at various levels of intensity over a long period of time. This statistically valid approach will test the longitudinal analysis described above with precision, and will also allow us to answer other questions, such as:

- For borrowers who have a "worst case" longest sequence that exceeds the CFPB threshold of "harm," what do the other sequences for those borrowers look like? For example, is it likely that a borrower with one sequence of 10 loans also has a number of shorter sequences that do not imply "harm?" We will also return to Cohort 3 (full sample) and Cohort 4 (new borrowers) to examine the same question in our larger data set.
- What is the length of time between sequences for borrowers at varying levels of intensity of use? For example, do lighter users of the product come and go periodically (with long periods out of debt until they experience new financial shocks), or do they use the product a few times in a short period and then never reappear?
- What role do defaults play in intensity of product use? Is there a correlation between sequence duration and defaults?

We hope to publish Report 7-C in less than a month. We hope to publish Report 7-C in less than a month.

## 7. Policy Takeaways

Our discussion of how many borrowers are "harmed" versus how many are not assumes the validity of the argument that "harm" occurs when a borrower pays more in fees than the principal that is borrowed. Existing research on use cases for payday loans challenges that assumption. We discuss in the report the studies of consumer uses for payday. Current research suggests that many borrowers use the product either to cover an emergency expense or to cover a mismatch between timing of income and due dates (after grace periods) of expenses. Fully a third of the use cases fall in this category, and the vast majority of the costs being covered in those use cases are for transportation, housing or utilities. A close fourth is medical care.

A consumer whose alternative to even a very expensive payday loan is to go without housing, transportation, utilities or medical care has a very high opportunity cost when not taking the loan. That cost can be loss of a job, loss of housing, loss of heat or loss of health. We argue that if only a third of the 60 percent of cases where cost may exceed principal are nevertheless economically justified by the opportunity cost of not using the product, then a majority of consumers are not "harmed" by the product.

We therefore suggest that an intervention that is certain to eliminate the storefront industry may not make legal or economic sense. We argue that the CFPB should allow the product to continue in an amortizing installment form, where permitted by state law. We argue further that a sequence of up to six loans should be allowed where only the payday form is allowed under state law, with borrowers guaranteed an amortizing installment exit plan if they hit the six loan trigger.

We also argue that our data shows borrowers who have not had a loan for 30 days are much less likely to get into a long loan sequence. Moreover, there is not a big difference in behavior between a 30-day and a six month cooling off period in the data. Thus, a model permitted loan should only require a 30-day cooling off period before the borrower could once again access the product.

We have modeled the effect of our proposal on the loan volumes in our data set. If one assumes a legal maximum sequence length of six loans, followed by a 30-day cooling off period, current industry loan volume would drop by 20.67 percent. Adding a four-pay-cycle period for installment repayment of principal after a six loan sequence would produce a cumulative loan volume reduction of 36.36 percent (some of which would be offset by interest revenue during the installment repayment). While a one-third reduction in loan volume and associated revenue would be a severe income shock to the industry, it is much more likely to be survivable (through store and lender consolidation) than the 70 percent volume reduction proposed by the bureau.

In summary, payday use data shows mean (average) usage statistics that are heavily influenced by a minority of borrowers who engage in very long loan sequences. They represent a "long tail" on the usage statistics. We think the CFPB should cut off the "tail" and not the entire product, because cutting off the product is likely to harm more consumers than it helps.

# Table of Contents



List of Tables, Figure and Appendices

# Table of Contents Continued

List of Tables, Figure and Appendices Continued



Appendix A: Percentage of Borrowers by Number of Sequences



Appendix B: Number of Loans per Sequence



Appendix C: Maximum Sequence Duration per Borrower



Appendix D: Distribution of Maximum Sequence Duration per Borrower (Four Years)

# Searching for Harm in Storefront Payday Lending

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By: Rick Hackett

February 12, 2016

### How To Read This Report:

Sections I through IV of this report present complex statistical analyses in tables, figures and text. We have added takeaway panels (like this one) in each of those sections that summarize key questions and findings. The reader can glean the gist of each section from those panels, but should carefully review the full text to understand limitations on the statements in the takeaways.

## Introduction

The Clarity Small-Dollar Markets Research team[1] has access to very large data sets of online small-dollar applicants and borrowers. Clarity has augmented its research capability by obtaining access to a set of loan records relating to 110 million loans made by five large storefronts between 2009 and 2014, constituting about 20 percent of the storefront payday market.[2] Through unique customer IDs, we can see customer activity across multiple lenders in this market. This ability to follow individual customer activity across a long period of time and multiple lenders allows us to answer a unique set of questions.

This paper uses a subsample of storefront loans and borrowers covering all of 2010 through 2013. The data set is described in Table 1.

### Table 1

#### Description of Storefront Data Set 2010-2013

| Borrowers Individual Market | Number of Borrowers | Number of Loans | Number of Lenders | Mean Number Loans per Borrower |
|---|---|---|---|---|
| Storefront Borrowers | 4,124,936 | 72,545,690 | 5 | 17.6 |

1. Rick Hackett is a Special Policy Consultant to nonPrime101, and was formerly the Assistant Director for Installment and Liquidity Lending Markets at the CFPB. This report is based on statistical analysis by Amir Fekrazad, a PhD candidate at the University of Texas - Austin, and Heather Lamoureux, Research Associate for nonPrime101.

2. The data consists of 100 percent of the loans originated by the lenders, thereby eliminating lender-induced sampling bias.

We also have the ability to look back into the first six months of 2009 to identify activity by the same borrowers and lenders for purposes of sampling modulation based on prior activity.

We follow borrower loan use activity in order to answer the questions:

- How long does the borrower stay in the market (the market life cycle)?
- What are the patterns of use of payday loans over a market life cycle?
    - What are the different methods used to analyze this question?
    - How does sampling bias in the different methods affect the results?
    - Do we find long serial loan sequences that suggest "harm" for a majority of borrowers? As defined by the CFPB? Defined by other criteria?
- How do nonPrime101 results compare to the CFPB publication results?
- Assuming that the CFPB rules effectively shut down the storefront[3] single-payment lenders, how many borrowers who currently use the product without "CFPB harm" will be denied access to useful credit?

To answer these questions, we segmented the data set shown in Table 1 in several different ways. We conducted use pattern analyses of the following segments:

- All users in the data set.
- "Restricted States" an analysis of all users in a restricted sample from Metropolitan Statistical Areas (MSAs) where state law does not restrict the use of multiple lenders and loans.[4]
- Cohort 3: All users in January 2010, followed as a static pool for the balance of the four-year sample period
- Cohort 4: Users in Cohort 3 that did not have a loan outstanding in the prior six months.
- Cohort 5: Users in Cohort 3 that did not have a loan outstanding in the prior three months.
- Cohort 6: Users in Cohort 3 that did not have a loan outstanding in the prior month.

Our analyses focus primarily on Cohorts 3 through 6 (described above) because static pool analysis was the primary method used by the CFPB in their published regulatory analyses. We include the "all users" data and the "restricted states" data in this report, both to confirm our initial observations (below) on the effect of such an approach and to permit the reader to assess the effect of reduced state regulation on use patterns.

3. This paper is based solely on data from the storefront payday industry. We have observed in other papers that the online industry does not evidence the same intensity of use (or customer loyalty) that we find in storefront payday. See our Report 7-A, Table 2. Thus, a rule that limits the number of seriatim customer interactions may not have a similar effect on online lenders.

4. This sample is fully described in our Report 7-A. In this report, we do not dwell on the results for the "restricted states" sample, as it was the primary focus of Report 7-A. Because we ran the statistical analysis of this sample for each of the approaches used in this paper, however, we present the results. The results for this sample may be useful in detecting the effect of state regulation of multiple lender and multiple loan use in decreasing the negative behavior measured in our various tests, by comparing to the results for the "all users" sample.

# Policy Context

This report was written in the context of the CFPB's intention, announced in its March 2015 *Outline of Proposals Under Consideration and Alternatives Considered* (SBREFA Outline), to intervene in the market for small-dollar loans by adopting rules to regulate small-dollar lenders. We have previously analyzed the effect of the proposals in the SBREFA Outline on the lenders in our sample: a 70 percent reduction in payday loan volume that would very likely shutter the businesses involved.[5] The intervention proposed in the SBREFA Outline is based on bureau research reported in the white paper *Payday Loans and Deposit Advance Products* (April 2013) and the *CFPB Data Point: Payday Lending* published in March 2014.The author of this report participated in the drafting of the white paper and is familiar with the research data used for *Data Point* (the same data set as the white paper). The CFPB's storefront payday data set is robust, covering a significant portion of the market. Unfortunately, it is truncated, lasting at most 12 months for each lender, and it is fragmented with no ability to see a particular borrower interact with more than one lender (it was de-identified without using consistent borrower IDs).

The CFPB has also acknowledged the limitations of the analytical methods used in the white paper. The CFPB analyzed those limitations in *Data Point*. The white paper sampled all borrowers who appeared in the first month of the sample. The later study attempts to judge the sensitivity of the white paper methodology to sampling bias, namely over-sampling heavy users of the product. In *Data Point*, the bureau ran two parallel statistical analyses of the behavior of (a) all borrowers in the data set (regardless of when their first loan occurred) and (b) new borrowers appearing for the first time in the second month of the sample. In this latter test, the bureau analyzed 11-month use patterns.

The bureau's data limitations create possibly significant weaknesses. First, the difference in borrower use patterns between truly new borrowers that we can observe in a 4.5-year data set and borrowers who are new only in the sense that they had no loan in the prior month (the CFPB new borrower) is significant. Second, a 12-month snapshot could never be thought to present a picture of the full market life cycle of a user of the product. The CFPB did not claim it did. When we look at loan use over a full market life cycle, we detect much more heterogeneity in loan usage patterns than the bureau did, especially when we remove the sampling bias of the white paper. Finally, the bureau's data did not allow a longitudinal view of that heterogeneity in assessing the counts of borrowers with different experiences. All the bureau could safely say is that a significant minority of borrowers in their 12 months used too many loans in long sequences. The bureau cannot safely say that over the life of a consumer's use of the product, typical patterns show X percentage of borrowers evidence use that may be "harmful." In this report, we attempt to conduct such an analysis.

5. https://www.nonPrime101.com/blog/clarity-analysis-confirms-cfpb-simulation/

Why all this focus on statistical use patterns? The bureau's legal tools are focused on practices that the bureau claims are "unfair, deceptive or abusive.[6]" Legally, an act or practice is unfair if it causes substantial injury to consumers, the injury is not reasonably avoidable by consumers, and the injury is not outweighed by countervailing benefits to consumers or competition. An act or practice can be abusive in four different ways, but the most likely rubric the bureau will apply finds an activity to be abusive if it "takes unreasonable advantage of the consumer's inability to protect his or her interests in selecting or using a consumer financial product or service." Whether using the term "injury" in the unfairness analysis or deciding whether unreasonable advantage is taken of the consumer, an essential first finding is that consumers are materially harmed. The bureau has claimed there is a significant source of harm in the white paper and the SBREFA Outline by finding consumers take out unaffordable payday loans, cannot make the lump sum payment while meeting other obligations, and then either renew or immediately re-borrow the loan. The result is a long sequence of serial loans "with successive finance charges eventually eclipsing the original loan amount, before they are able to retire their debt.[7]" The bureau has also noted potential harms in the form of explicit default and in the form of financial shock. The bureau asserts these can be found in the inability to pay other obligations and maintain a positive bank balance when the lender uses recourse to the consumer's deposit account to collect the loan - what consumer advocates have dubbed "phantom defaults."[8]

When the bureau speaks of loan sequences in which the "finance charges eventually eclipse the original loan amount," they offer one numeric rule of thumb we can use in examining statistics for evidence of what the bureau defines as "harm." The median fee for a storefront payday loan is $15 per $100 per pay period. Thus, a sequence of seven of more loans will involve finance charges that "eclipse the loan amount." The bureau has also offered up its metric for a sequence that borders on dangerous but is short enough to be tolerated without harm, in the SBREFA Outline. That proposal would permit a sequence of three loans to be made without onerous underwriting, so long as a 60-day cooling off period follows that sequence.[9] Thus, we should look for two signals in the data that follows: how many borrowers have sequences of three or fewer loans, and how many have sequences of seven or more loans? Of course, we can conduct this analysis without necessarily agreeing that the bureau definition of "harm" meets either the legal definition or a correct policy definition, and any use of the word "harm" in this paper should be read to mean the CFPB's definition of harm.[10]

6. SBREFA Outline; Dodd-Frank Act Section 1031.

7. Outline, p. 9. (emphasis supplied).

8. http://www.responsiblelending.org/payday-lending/research-analysis/payday-mayday-visible-and.html. We plan to analyze phantom defaults using bank account data pulled by online lenders in a future Report.

9. Notably, the bureau infers harm if three loans are made within 60 days of each other in the SBREFA Outline. Our statistics report on sequences consisting of loans made within one full pay period of each other. This is the definition of sequence used in both of the bureau's published statistical reports, and has inherent economic logic - from a cash flow perspective, the consumer is re-borrowing the same dollars he repaid in the past few days. The bureau has not suggested an empirical or theoretical logic for its 60-day tracking proposal, except to claim that 60 days is enough time for the shock of a prior loan to dissipate. We believe the logic of the white paper - tacking together loans that occur in the same pay period that a prior loan was paid off - makes more economic sense and is a valid measure by which to treat legally separate loans as economically the same loan. Our computation of sequence duration is more granular that the bureau's white paper, however, because we link together loans based on each consumer's actual pay frequency, rather than using the 14-day linking method used by the bureau's white paper.

10. In order to avoid losing track of the principle we will use the term "CFPB harm" where required for clarity.

What the bureau does not say and legally cannot say is: these loans are just too expensive.[11] The bureau is prohibited by law from setting usury limits. Thus, the bureau must look for consumer harms in the results of use of the loans.[12] The bureau's publications have, so far, focused on harmful results. As discussed above, legally the bureau must also balance that harm against benefits that may exist for consumers who use the product without "harm," at least in applying the "unfairness" law. Because the bureau alleges harm in the multiple, extended, sequential uses of the product, this report focuses on the description of use patterns, with a particular focus on whether our larger, longer, cross-lender data set suggests any different results from those published to date by the bureau. We also focus on some questions not asked in the bureau publications, but which can be answered from supply-side data, and must be answered under the unfairness legal analysis: What is the distribution of the number of consumers who may be harmed versus those who very likely are not?

The questions we ask in this report, and the policy significance we attribute to the answers, are inherently limited by the kinds of answers that can be found in supply-side (administrative) statistics. That is true of both the bureau data and Clarity's data. In this report, we address the issues raised by the bureau, namely the harms that allegedly can be inferred from the relationship of fees paid to the amount borrowed, in a short period of time. The data we analyze here, and all of the data available to the bureau, cannot answer equally important demand-side questions, such as: What is the opportunity cost to the borrower who cannot access this product (such as loss of housing or transportation) if the product is banned? That cost may dwarf the price of even a long sequence of loan renewals. For a consumer who can eventually repay a loan, it may make economic sense to pay even twice the amount borrowed in fees to avoid losing a home or a vehicle used to get to work. Similarly, if the demand for credit continues to exist after the product is banned, what access alternatives will the borrowers have to avoid that opportunity cost and what will those alternatives cost?[13] The data presented here should be viewed through the lens of the limitations of supply-side data.

Finally, this report calls out limitations in the supply-side analysis presented to date by the bureau, but is not intended to supply the only interpretation of the data. For that reason, we have presented, in the Appendix, all of our data results, so that those who seek answers to other questions, or wish to critique our answers to the questions we present, can form their own interpretations of what can be seen from a much larger and robust sample than was used in policy making to date.

11. Under the Dodd-Frank Act, the bureau is prohibited from setting usury ceilings. See section 1027(o).

12. The bureau could also use its power to attack an alleged deceptive manner in which loans are marketed or originated, and some bureau writings have suggested that marketing payday loans as a short-term solution is misleading. Marketing practices are, however, usually specific to individual lenders and more likely to attract individual enforcement focus than a generic rulemaking approach.

13. One known alternative is bank checking account overdrafts, which are more expensive than payday loans. The bureau has already analyzed the migration back and forth between overdraft and payday-like bank deposit advance products in the white paper. The overdraft alternative to payday likely will become less available to heavy users of overdraft as a result of the bureau's promised, separate regulation of bank overdraft.

# I. Refining Our View of Payday Use Statistics, Market Life Cycles, Truncation Effects and Sampling Bias

## A. Prior Report

### Takeaways - Section I-A

Looking at all borrowers in the four-year sample:

- About 14 percent use one loan
- About 20 percent begin and end payday use in 100 days or less, BUT
- Almost 10 percent are in the product for the entire four-year period, albeit not continuously

This suggests our four-year data set shows the lion's share of consumer life cycles in the product.

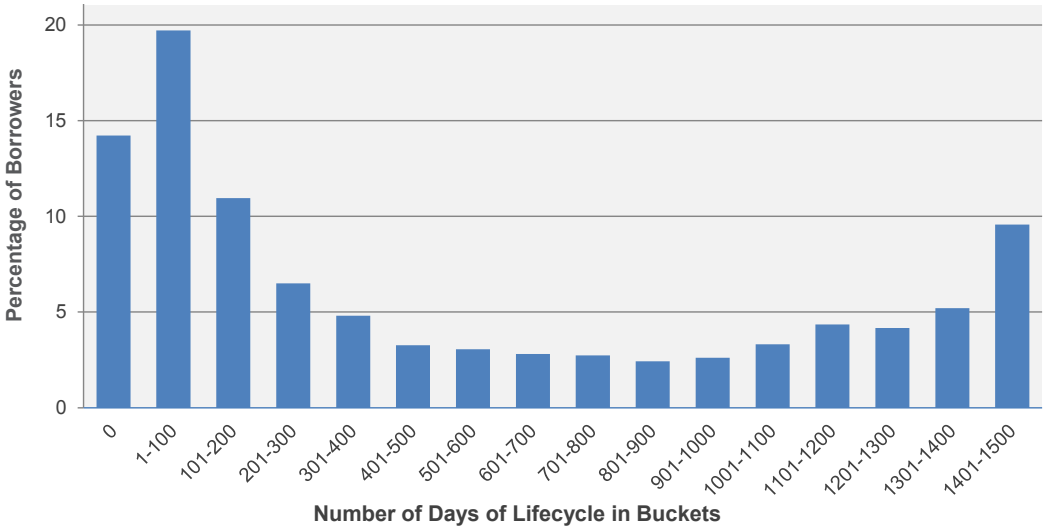
In our recently published Report 7-A, we asked the question whether the four-year snapshot we are using (2010-2013) is representative of a significant number of complete market life cycles of borrowers. That is, if borrowers typically use payday loans fairly regularly for 10 years, then a four-year snapshot could represent the beginning, middle or end (but not all) of a customer experience in these markets. If the cycle is two years, then a four-year snapshot will capture typical lifetime behavior for a significant portion of the sample. An analysis of the cohort of borrowers who appear in January of the first year (out of four years) will capture the market life cycle for most of the borrowers in that group.

Measuring life cycles does not say anything about the intensity of product use. A borrower with one loan in January 2010 and one loan in December 2013 will have the same total duration of use as a borrower who takes out a loan every pay cycle. If the CFPB is correct in suggesting that borrowers use loans in clumps or sequences, however, a longer duration of use may suggest greater overall intensity of use. More important, if our four-year snapshot is significantly longer than the vast majority of consumer lifetime-in-product, then we can use a study of four-year cohort experiences to tease out a comprehensive picture of the diversity of ways that consumers use the product and construct a reliable answer to the question: What portion of consumers use the product in a harmful way (as defined by the CFPB) and what portion do not?

Looking at the storefront life cycle in our four-year sample, Figure 1 is what we previously reported about the distribution of life cycles.

## Figure 1

### Figure 1: Distribution of Storefront Customer Life Cycle Durations (Full Sample)[14]




## Table 2

### Life Cycle Duration Summary Statistics

| Calculation | Value |
|---|---|
| N | 4,124,936 |
| Mean | 447.72 |
| Median | 235 |
| Standard Deviation | 481.33 |

Although 50 percent of borrowers in this sample are in and out of the storefront market in 300 days or less, there is a long and increasing tail on the distribution with nearly 10 percent of borrowers spanning the entire duration of the sample. For that reason, we chose to test the sensitivity of our storefront analysis to truncation effect, as detailed in the next section.

14. Distribution represents all borrowers in the data set, regardless of when they began borrowing. The measurement is the number of days from the first loan to the last loan in the data set, without regard to intervening periods when there were no loans outstanding.

# I. Refining Our View of Payday Use Statistics, Market Life Cycles, Truncation Effects and Sampling Bias

## B. Truncation Sensitivity and Sampling Bias in Life Cycle (Time in Product Market) [15]

### Takeaways - Section I-B

We fine tuned our vision into product life cycles by:

- Analyzing cohorts, namely all the borrowers in a given month, and
- Comparing results for each cohort over different measuring time periods (this is called looking for truncation effect)

We found:

- Lengthening the measuring period lengthens the average life cycle
- As our time periods get longer, the truncation effect is smaller
- Our four-year measuring period is more likely to detect variety in behavior than the CFPB's 12 months

As discussed in the Policy Context section above, we wanted to know whether our sample might be fairly viewed as a representation of a consumer's overall experience in the product, especially given the very short time period sampled by the CFPB. Testing for truncation effects is critical to answering that question.

Truncation effects occur when pattern summary statistics are drawn from a sample that is shorter than the typical duration of a pattern under study. In this report, we summarize patterns of sequential loan use and other consumer behaviors that might be accurately represented in the summary statistics. But if consumers typically take more than four years to go through a cycle of use of loan products, then our pattern statistics will not be representative. We will capture some borrowers at the beginning of their use and some at the end. Similarly, the summary statistics above look at all borrower activity in the sampling period, but not all borrowers are equal. Some borrowers use loans heavily and some only once. By using all borrowers and loans appearing in the sampling period, or by using cohorts of all borrowers at the beginning of the sample, we may oversample heavy users - especially if we find that heavy users have a life cycle longer than our sampling period but light users are in and out.

We tested for truncation effect by selecting groups (cohorts) of borrowers who took out loans in July and October of 2009 and January of 2010. We then computed life cycle data for those cohorts looking forward until the end of 2012, and then until the end of 2013. The results are shown in Table 3.

15. Although our data only includes about 20 percent of the market in loan volume, we think it can fairly represent a particular consumer's time in the market. As discussed at length in Report 7-A, storefront payday consumers are highly loyal; they have a very strong tendency to stay with a single lender over four years. Even those who cross the street are highly likely to limit use to two lenders.

Table 3 illustrates that, regardless of the length of time we follow a cohort, lengthening the time period of a sample lengthens the average (mean) number of days in a market life cycle. This increase is at a decreasing rate as the time periods lengthen. That is, the difference in life cycle duration between the three and four-year samples is greater than the difference between three and a half and four and a half year samples. This suggests that the truncation effect is tapering off.

The longer duration cohorts have a mean number of days in the market life cycles that is greater than the median number of days. These measures correspond with a skew of each cohort's distribution to the right, with the mean greater than the median. Shown as a graph, we would see a long tail to the right containing a significant minority of borrowers with life cycles substantially longer than the median. In other words, most borrowers have a short market life cycle, relative to the mean, but a significant minority has very long market life cycles. This tendency is also reflected in the relationship of mean and median in the shorter duration studies of Cohorts 2_i and 3_i. These cohorts were intentionally truncated at three and a quarter and three years' duration, and the long tail of a minority of long life cycles is cut off. The medians are a small amount greater than the mean.

## Table 3

### Comparative Life Cycle Computations for Single-Month Cohorts (Truncation Effect)

| Date Range | Jul 2009-Dec 2013 | Jul 2009-Dec 2012 | Oct 2009-Dec 2013 | Oct 2009-Dec 2012 | Jan 2010-Dec 2013 | Jan 2010-Dec 2012 | Jan 2010-Dec 2013* |
|---|---|---|---|---|---|---|---|
| Cohort | Cohort 1 | Cohort 1_i | Cohort 2 | Cohort 2_i | Cohort 3 | Cohort 3_i | Cohort 4 |
| Cohort | July 2009 | July 2009 | October 2009 | October 2009 | January 2010 | January 2010 | January 2010 |
| Mean Life Cycle (Days) | 798.82 | 668.73 | 774.25 | 637.95 | 753.80 | 608.95 | 541.49 |
| Standard Deviation | 618.38 | 484.53 | 587.23 | 451.57 | 553.04 | 415.84 | 537.57 |
| N (Borrowers) | 1,062,327 | 1,062,327 | 1,097,348 | 1,097,348 | 1,013,070 | 1,013,070 | 91,644 |
| Median | 695 | 635 | 701 | 642 | 728 | 652 | 317 |
| Years | 4.5 | 3.5 | 4.25 | 3.25 | 4 | 3 | 4 |

* Cohort 4 = Cohort 3, but excludes borrowers with loans from July - December 2009

Table 3 tells us that looking at a four-year sample of payday borrower activity in our sample is likely to be directionally representative of overall consumer behavior in the market. Because storefront payday borrowers are very likely to stay with the same lender, regardless of intensity of use, the data is less likely to be skewed by the limited number of lenders in the set.[16]

16. See Report 7-A

Our sample is very likely more representative than the CFPB studies, all of which are based on one-year or shorter samples. We see mean life cycles between 799 and 609 days, depending on whether our sample duration is four and a half years or three years. Thus, the CFPB analysis based on 365 (or fewer) days is at best a partial snapshot of consumer behavior. Our snapshot of 1,460 days is more likely to be representative, especially when we analyze cohorts that start in the first month.

Table 3 also contains a first look at the effect of sampling bias, which is discussed extensively in the bureau's March 2014 *Data Point* on payday loans. Cohort 4 measures the activity of borrowers who appear in January 2010, but excludes those who had a loan in the prior six months. As discussed below, sampling only new borrowers significantly reduces the average time between the first and the last loan, from 754 to 541 days. As illustrated in Table 4, this is a reduction from using the product, perhaps sporadically, from a maximum of 52 percent of the available days to a maximum of 37 percent of the available days - all because of how the sample was selected.

## Table 4
### Single-Month Cohort Life Cycles Expressed as Percentage of Available Days

| | Cohort 1 | Cohort 1_i | Cohort 2 | Cohort 2_i | Cohort 3 | Cohort 3_i | Cohort 4 |
|---|---|---|---|---|---|---|---|
| **Number Of Possible Days Cohort (Borrower) May Be in Loan Cycle** | 1642.5 | 1277.5 | 1551.25 | 1186.25 | 1460 | 1095 | 1460 |
| **Mean Number of Days in Life Cycle** | 798.82 | 668.73 | 774.25 | 637.95 | 753.80 | 608.95 | 541.49 |
| **Percentage of Mean Number of Days Out of Possible in Life Cycle** | 48.6% | 52.3% | 49.9% | 53.8% | 51.6% | 55.6% | 37.1% |
| **Median Number of Days in Life Cycle** | 695 | 635 | 701 | 642 | 728 | 652 | 317 |
| **Percentage of Median Number of Days Out of Possible in Life Cycle** | 42.3% | 49.7% | 45.2% | 54.1% | 49.9% | 59.5% | 21.7% |
| **Years** | **4.5** | **3.5** | **4.25** | **3.25** | **4** | **3** | **4** |

The results on sampling bias suggest some important facts. First, by comparing the counts of the number of borrowers in each one-month cohort (Table 3), we can see that in any given month the cohort of all storefront borrowers who take out or renew a loan are predominantly heavy users. Cohort 3 (all borrowers in January) is 10 times the size of Cohort 4 (truly new borrowers in January). Second, new users of the product are more likely to get in and out of the product quickly than are seasoned users. Finally, any study that samples all users in a given month in order to measure typical use patterns will be skewed by a very significant sampling bias toward heavier, longer term users.[17]

17. The first CFPB study on payday, white paper on *Payday Loans and Deposit Advance Products* (April 2013) suffered from this bias. The CFPB attempted to correct for this defect by measuring 11-month use by those who did not have a loan in the first month of their sample in *Data Point* published in March 2014.

Because of the very significant difference in borrower use patterns found between Cohort 3 and Cohort 4, we also tested alternative definitions of "new" borrowers, by excluding from Cohort 3 those who (a) had a loan in the last three months of 2009 (creating Cohort 5) and (b) those who had a loan in the last month of 2009 (creating Cohort 6). The results are shown in Table 5. Table 5 illustrates that the size of Cohort 6 is roughly twice the size of Cohort 4. The definition of "new" is thus very sensitive to how far back one looks in time to detect prior loan activity.

## Takeaways - Section I-B -Table 4

We tested the importance of cohort sampling method:

- Our original tests used all borrowers in a month, which corresponds to the CFPB's white paper method
- We then tested only those borrowers who had not used the product in the six prior months, three prior months, and one prior month

We found:

- Sample selection based on recent use makes a huge difference in time-in-product
- The only sample selection screen used by the CFPB (no use in one month prior to sample) is not adequate to tease out very real differences in behavior of truly new borrowers

## Table 5

### Comparative Cohorts of January 2010 Borrowers by Prior Loan Activity

| Date Range | Jul 2009 - Dec 2013* | Oct 2009 - Dec 2013* | Dec 2009 - Dec 2013* |
|---|---|---|---|
| Cohort | Cohort 4 | Cohort 5 | Cohort 6 |
| Cohort | Jan-10 | Jan-10 | Jan-10 |
| Mean Life Cycle (Days) | 541.49 | 572.25 | 636.57 |
| Standard Deviation | 537.57 | 542.08 | 551.01 |
| N (Borrowers) | 91,644 | 116,418 | 190,335 |
| Median | 317 | 367 | 495 |
| Years | 4.0 | 4.0 | 4.0 |
| | * Jan cohort excludes any borrower who appeared in July 2009 to Dec 2009 | * Jan cohort excludes any borrower who appeared in Oct 2009 to Dec 2009 | * Jan cohort excludes any borrower who appeared in Dec 2009 |

The definition of "new" also significantly affects use patterns. The mean and median market life cycle of new borrowers in Cohort 6 (no loan in last 30 days) are closer to the values for Cohort 3 (all users) than they are to Cohort 4 (no loan the previous six months). There are a significant number of borrowers in Cohort 3 who have not had a loan for 30 days but are still longer-term users of the product.

Our findings on the typical life cycle of a payday borrower also suggest that even a six -month exclusion may not solve for absolutely new borrowers. We observe mean life cycles are closer to two years, so a very long data set would be needed to find truly new customers of storefront payday - one that would allow a two-year look-back period to exclude experienced borrowers. Nevertheless, we think the four-year sample (with an additional six month look back) provides a much more accurate view than the bureau's 12 or 11 months, for both new and seasoned borrowers.

First, our findings suggest that discussion of four-year use patterns of newest users who have no loans in the last six months of 2009 are likely to be very reliable illustrations of the lifetime use by payday borrowers in that group. As seen above, the newest borrowers have a median life cycle of less than a year and a mean life cycle of less than one and a half years. As discussed in section III-A below, we observe that new borrowers are highly likely to exit the product in one year after they begin borrowing. We can follow the minority who do not exit for a full four years.

Second, we compute that 82 percent of all borrowers (new and not new) in the January 2010 cohort have exited the product six months before the end of the four-year sample period and 85 percent have exited 90 days before the end of the period. Given our observations below on the likelihood that going 90 days without using the product will rule out most heavy users of the product (who are the most likely to return), this tells us that we likely have a full life cycle in our four-year sample for at least 85 percent of the borrowers in our sample.

We conclude that a four-year history of behavior in using the product is likely to be illustrative of a full market life cycle for the borrowers we follow, and that observing such a long period will likely be additive to the existing short-term studies of the CFPB.

## Conclusions - Section I-B

We concluded:

- The significance of sampling bias means we should compare all possible methods of cohort selection to test for loan sequence frequency and duration (intensity of use)
- Our four-year sample provides a more robust view of borrower experience than the CFPB's 12 month snapshot

# II. Use Patterns and Intensity Measurements

In the prior section of this report, we concluded that our four-year sample is likely to provide a good representation of the product life cycle of borrowers. In Section II, we compare results from our data to measures of loan use intensity published by the CFPB. We compare first the CFPB 11-month study of loan sequences per borrower versus a study of the same period from our data set. We then look for differences produced when we expand our view to a full four-year sample, to determine whether a review of a full life cycle produces different results from a short snapshot. We conclude there are material differences. We then apply a full four-year review to other measures of use intensity: duration of loan sequences (number of loans per sequence) and maximum sequence duration (or worst case) per borrower.

## Definitions - Section II -A

Important definitions to remember in reading tables.

**Full Sample:** All users in our sample

**Cohort 3:** All users in January 2010 (corresponds with the CFPB's white paper method)

**Cohort 4:** Same as Cohort 3 but excluding anyone with a loan in prior six months

**Cohort 5:** Same as Cohort 3 but excluding anyone with a loan in prior three months

**Cohort 6:** Same as Cohort 3 but excluding anyone with a loan in prior one month (corresponds with the CPFB's *Data Point* new borrowers)

**Subsample 1:** All users in geographic proxy for lightly regulated states (no restriction on multiple lenders/loans). This data illustrates the effect on national samples of states that do not impose these use restrictions.

## A. Existing Regulatory Studies and Methodology / Application to Larger Data Set

As noted above, the first CFPB study of payday use patterns, the white paper, used a sampling method similar to our Cohort 3 following all of the borrowers in the first month of a sample for the duration of the sample. In response to criticism from the industry, the second CFPB study (*Data Point*) analyzed the same data to report, among other things, the sensitivity of results to sampling methodology. *Data Point* presented results derived by following all borrowers in the second month of the data set, excluding any borrower who had a loan in the first month. The CFPB called these "new" borrowers. Since the CFPB had only 12 months of data, the results showed use over an 11-month period. This sampling method corresponds to our Cohort 6.

The CFPB also reported results obtained by measuring the usage activity by all borrowers, regardless of when they began using the product in the sample, a method we also report on below.[18] In subsection I-A, we ask whether our larger data set produces similar results to those found by the CFPB, and whether looking at longer periods of time materially changes the outcomes found by the CFPB.

In *Data Point*, the bureau examined the sensitivity to sampling methodology of its measurement of the distribution of borrowers with various numbers of sequences in an 11-month period. Figure 9 in *Data Point* shows the results for new borrowers and contrasts it with the data in Figure A1 of *Data Point* for "all users" and the white paper sample. In Appendix A, below, we present complete distributions derived using all three of the bureau methods applied to (a) 11-month samples beginning in January 2010 and (b) four-year samples beginning in January 2010. The first sequence count we report is for sequences with a single lender (the method used by the bureau), what we call Loan 2. We also report the results of the same analysis using Loan 3 (sequences consisting of loans made by more than one lender). In both cases, we define sequence in a manner similar to the bureau. The bureau defined a loan as "in sequence" if it was made within 14 days after the last loan was paid off. We define a loan as in sequence if it is made within the same pay period that a prior loan was paid off. Thus, if a bi-weekly payroll borrower pays off a loan on payday on a Friday, we count as in sequence any loan made within the next 14 days. If a borrower is paid weekly, in our analysis we would count as in sequence any loan made within seven days of the last loan payoff. Thus, our approach is slightly more accurate as a test for loans that are taken out before the net effect of the last loan payoff is muted by a new cash infusion.

In Table 6, we compare key values found in our Appendix A and in the CFPB's *Data Point*. Data in the same color is computed using the same sampling method.

## Takeaways - Section II-A - Table 6

We tested all of the CFPB's sampling methods (Full sample, Cohort 3 and Cohort 6) as applied to our data for the short time period observed by the CFPB. We measured the percentage of borrowers using one loan sequence, two loan sequences, etc. The more sequences a borrower used, the more intense use is implied.

We found:

- For the short period used by the CFPB, there is not a great difference between our results and the results found by the CFPB
- The small difference in results between the different sampling methods, when looking at a 12-month sample, supports the CFPB's conclusion that the bias in their white paper sampling was not that significant
- The borrowers in our sample actually have slightly greater sequence frequency than the CFPB's sample
- Truly new borrowers (not in data for six months prior to sampling) actually appear to be more intense users, in a *one-year* test

18. This is the method we used in computing summary statistics relating to various geographic subsamples in Report 7-A.

## Table 6

**Comparison of Sample Distributions (11-Month Cohorts) Number of Sequences per Borrower**
Clarity vs. CFPB

| By Percentage of Borrowers | 1 Sequence | 2 Sequences | 3 Sequences | 4 Sequences | 5+ Sequences |
|---|---|---|---|---|---|
| CFPB Data Point New Borrower | 48 | 26 | 15 | 7 | 4 |
| Clarity Loan 2 Cohort 6 ≈ CFPB-Defined New Borrower | 38 | 28 | 16 | 9 | 9 |
| Clarity Loan 3 Cohort 6 ≈ CFPB-Defined New Borrower (Multi-Lender) | 41 | 28 | 16 | 8 | 7 |
| CFPB Data Point "All Users" | 60 | 23 | 10 | 4 | 2 |
| Clarity Full Sample ≈ CFPB All Users | 55 | 25 | 11 | 5 | 4 |
| CFPB White Paper | 50 | 28 | 13 | 6 | 4 |
| Clarity Loan 2 Cohort 3 ≈ CFPB White Paper | 47 | 28 | 13 | 6 | 6 |
| Clarity Loan 3 Cohort 3 ≈ CFPB White Paper (Multi-Lender) | 52 | 26 | 12 | 5 | 5 |
| Clarity Loan 2 Cohort 4 - 6 Months Truly New | 44 | 28 | 15 | 7 | 6 |
| Clarity Loan 3 Cohort 4 - 6 Months Truly New Multi-Lender | 48 | 26 | 13 | 6 | 7 |

In reviewing Table 6, the reader should consider that results showing more borrowers with only one, or at most two, loan sequences reflect more borrowers with less intense use (assuming sequences are of equal duration). In other words, the higher the percentage of single-sequence users, the less "harm" the sampling methodology suggests is occurring. We discuss the distribution of sequence durations in Section II-B, below.

Table 6 prompts a number of preliminary observations.

As noted by the CFPB, measuring all users who appear over any time period in a data sample will show the lowest intensity of use, because it includes many borrowers are just ending or just beginning a period of intense use. It reflects the greatest truncation effects on sampling.

In this case, an "all users" sample shows the lowest frequency of multiple sequence use. This result can be seen in the high percentage of borrowers using only a single loan sequence over 11 months and the low percentage using five or more sequences in that period.

The white paper method, corresponding to Clarity's Cohort 3, produces a slightly lower percentage of borrowers with only one sequence in 11 months, with more borrowers with multiple sequences.

The CFPB new borrower method, corresponding to Clarity's Cohort 6, further skews toward higher usage, but only slightly.

Counter-intuitively, a six-month exclusion period in Cohort 4 produces the most intense usage patterns, if only slightly greater, for an 11-month usage statistic.

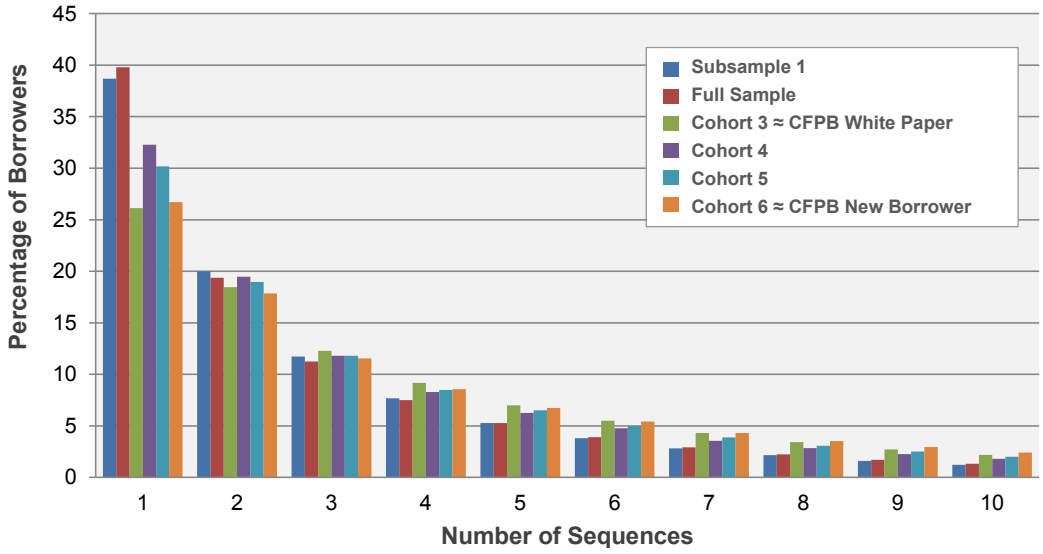
The most significant conclusion from this table is that *Data Point* appears directionally correct: when looking at 11-month usage patterns, differences in sampling methods have only minor effects on the distribution of intensity of use (measured by the number of sequences used). In addition, looking at a much larger number of loans than the CFPB, with more varied sampling methods, we can basically confirm the CFPB's results for an 11-month snapshot. Indeed, our larger sample actually shows a slightly higher overall intensity of use than the CFPB found, in each of the various sampling methods. In other words, our borrower population shows slightly more intense loan usage (on this measure) than the borrowers in the CFPB study.

Finally, we can add to the CFPB's snapshot the observation (that the CFPB could not make) that detecting sequences involving multiple lenders (Loan 3) only slightly changes the results, notably to slightly increase the percentage of borrowers with fewer sequences, although these sequences may be longer (as discussed below). This result makes sense when one considers our finding in Report 7-A that more than 25 percent of borrowers who switch lenders do so during a multi-lender sequence. That is, the first loan with the second lender is taken out in the same pay period when the last loan was repaid to the prior lender. Our methodology for Loan 2 would count such an occurrence as two sequences, but the Loan 3 methodology would count such an occurrence as a single, multi-lender sequence. Thus, it makes sense that we see slightly more borrowers with only one sequence in Loan 3 than in Loan 2.

Our finding that the results for 11-month samples in *Data Point* appear comparable to results from our larger sample does not mean, however, that *Data Point* captured all important trends in measuring sequences per borrower. When we extend our analysis of the same sample groups for a four-year period, we find significant differences between the CFPB's samples and the testing that our longer sample range permits. In Figure 2, we present the same sampling methods discussed above, but applied above over the four-year duration of our sample.

## Figure 2

### Percentage of Borrowers by Number of Sequences in Four Years (Loan 2)




The corresponding data is in Table 7.

## Table 7

### Percentage of Borrowers by Number of Sequences in Four Years (Loan 2)

| Number of Sequences per Borrower | Subsample 1 | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 5 | Cohort 6 ≈ CFPB "New Borrower" |
|---|---|---|---|---|---|---|
| 1 | 38.67 | 39.80 | 26.12 | 32.29 | 30.18 | 26.72 |
| 2 | 19.98 | 19.37 | 18.45 | 19.47 | 18.96 | 17.85 |
| 3 | 11.72 | 11.25 | 12.28 | 11.79 | 11.80 | 11.55 |
| 4 | 7.67 | 7.49 | 9.18 | 8.29 | 8.49 | 8.57 |
| 5 | 5.27 | 5.27 | 6.99 | 6.25 | 6.50 | 6.75 |
| 6 | 3.81 | 3.90 | 5.50 | 4.76 | 5.00 | 5.41 |
| 7 | 2.82 | 2.91 | 4.31 | 3.54 | 3.89 | 4.31 |
| 8 | 2.15 | 2.22 | 3.41 | 2.83 | 3.06 | 3.53 |
| 9 | 1.60 | 1.70 | 2.72 | 2.25 | 2.50 | 2.95 |
| 10 | 1.23 | 1.32 | 2.17 | 1.80 | 2.01 | 2.41 |

## Takeaways - Section II-A

- When we looked at the same samples over a full four-year time period, we found a much more significant bias in the white paper method. The direction of the changes and their magnitude increased. The white paper method misjudged the directional differences between sampling all borrowers and sampling new borrowers.

We believe this four-year sample is more representative of a typical consumer's experience in the payday product than the bureau's 11-month snapshot. As discussed above, the mean life cycle for a Cohort 3 (white paper sample) borrower is 754 days. For Cohort 4 (six-month exclusion - truly new borrower), it is 541 days. The view through the CFPB's 11-month window (330 days) therefore is greatly affected by truncation effect, and the four-year window much less so.

Looking at our four-year sample in Table 7, we see that the difference in use intensity (number of sequences) for Cohort 3 (white paper) borrowers versus Cohort 6 (30-day new borrowers) is three times the difference observed over 11 months (Table 6). More importantly, the difference is in the opposite direction from the 11-month table. New users show **less** intense use, not greater. Turning to truly new borrowers that can only be observed in our data, we see the CFPB reporting 50 percent of white paper borrowers having only one sequence, where only 44 percent of Clarity Cohort 4 truly new borrowers have a single sequence. In Table 7 (four years), only 26 percent of the white paper borrowers limit their use to only a single sequence over four years, but 32 percent of truly new borrowers have that limited intensity. In other words, an 11-month sample shows less intense use by white paper borrowers (by six percent), but a four-year sample shows the opposite effect, by eight percent.

We think that the change in the direction of difference between Cohort 3 borrowers and newest borrowers is explained in later sections of this paper that measure the length of sequences. We report below that newest borrowers tend to have short sequences, whereas Cohort 3 borrowers include a significant number of consumers who are in the product almost continuously. We believe newer borrowers have a higher number of short sequences, when looking at a short, 11-month sample. The count of sequences appears higher than for seasoned borrowers, because of the short measuring period. The Cohort 3 sample selection heavily favors borrowers who use the product continuously and that bias changes the direction of the difference between newer borrowers (who are actually lighter users) and seasoned borrowers, who predominate in the Cohort 3 sample. The bureau's 11-month sample simply isn't long enough to detect a pattern difference between new and seasoned borrowers.

This is not simply a feature of statistical interest. The white paper presents numerous other statistical results that are based on a sampling approach which, combined with the very short duration of the sample, may skew the picture of payday use toward heavier users consuming the product in a manner which is likely to be defined by the CFPB as harmful.

Analyzing the question whether the product is "harmful" to enough consumers to justify denying access for all consumers would greatly benefit from an analysis of a longer sample. We hope to provide that analysis below.

To summarize, the bureau's analysis of an 11-month sample in *Data Point* was directionally accurate, but it was skewed by not having a long enough sample period. *Data Point* concluded that the sampling methods used in the white paper do not produce significantly different results from other methods, thereby validating the conclusions in the white paper. When we look at a longer data sample, however, we see that truncation effect significantly limits the ability of the *Data Point* analyses to detect the skew in the sampling methods used in the white paper. When we more fully align the sample duration with the life cycle of a payday borrower, we see that sampling methods in the white paper skew the outcomes in that report. We detect this first in the metric of number of sequences used. While the overall difference between 11 months and four-year samples is not huge (14 percent), we thought it significant enough to test other more important metrics we now turn to: length of sequences (number of loans in a sequence) and longest sequence per borrowers. For both, we will see that looking at truly new borrowers presents a difficult policy decision -- more difficult than the CFPB white paper would suggest.

## B. Number of Loans in a Sequence

### Takeaways - Section II-B

We then looked at length of sequence: how many loans in a row that are not separated by a paycheck.

We found:

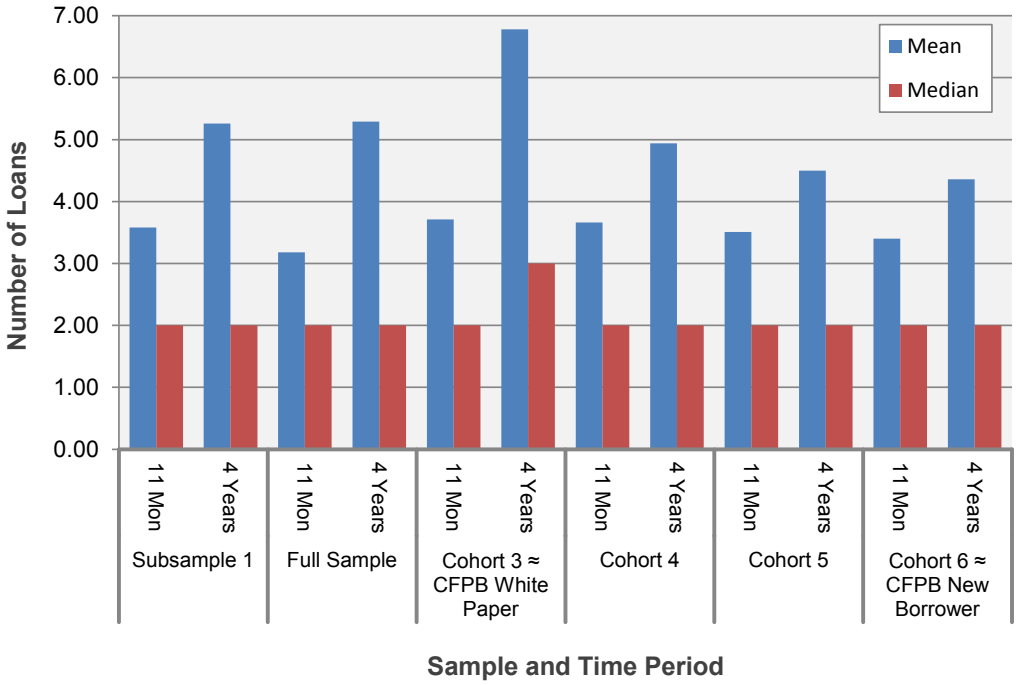
- For all sampling methods other than the white paper, 50 percent of all sequences are two loans in a row. The white paper method increases this median by 50 percent.
- The white paper method increases the average sequence length from approximately four loans to nearly seven loans (the point at which a borrower has paid as much in fees as he borrowed in the principal).
- The very large difference between the median (50/50 point in the distribution) and the mean (average) tells us that there is a minority of borrowers with very, very long loan sequences. They drag the average way up.
- When we add our unique ability to detect borrowers crossing the street to another lender to continue a loan sequence, the length of sequences does not increase materially.

The bureau's core hypothesis of "harm," as quoted above, is that long sequences of de facto rollovers result in draining the borrowers' pockets with little or no economic benefit to the borrower.[19] Thus, the distribution of sequence duration is a critical statistic in looking at use of the product. Since the CFPB argues a series of de facto loan rollovers are effectively a single, very expensive loan, it is important to understand the range of durations of loan sequences.

Appendix B contains detailed distribution data, using the various sampling methodologies discussed above, for sequence duration of Loan 2 and Loan 3. Figure 3 presents one portion of that data, looking at mean and median sequence duration for Loan 2 (loan sequences with a single lender).

## Figure 3

### Mean and Median Number of Loans per Sequence (Loan 2)




Observe in Figure 3 that the median number of loans in a sequence is almost always two loans, across all of the samples and time periods. As above, the CFPB does not argue that two loans in a row are harmful, yet half of all loan sequences are only two loans long. The only longer median, three loans, is in the four-year sample for the white paper methodology (Cohort 3), and it is still within what the CFPB has deemed safe in the SBREFA Outline.

Medians are important; they are the value at which half of a group are larger and half are smaller. The mean in every case in these samples (and in almost every other analysis we have conducted of this data set) is larger than the median, meaning there are very large values for a minority of whatever we are measuring: sequences per borrower, loans per sequence, and other values. Storefront payday is characterized by a minority of borrowers doing things that skew the average toward the CFPB harmful end of the spectrum.

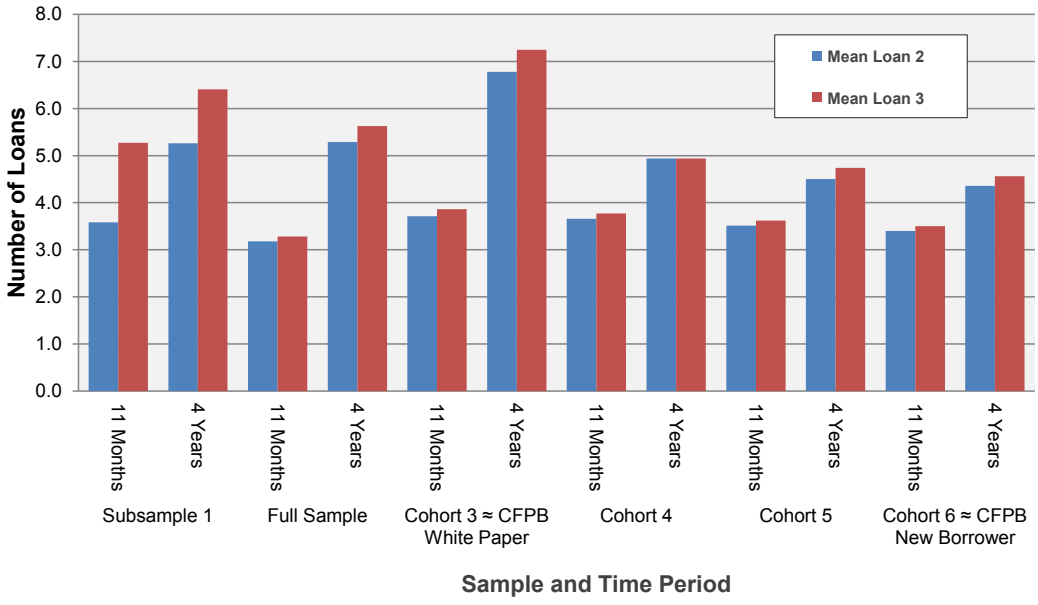
19. We comment on the CFPB's presumption of harm in the Policy Context section above.

In this case, we see that sequence duration for white paper (Cohort 3) borrowers evidences a mean (average) number of loans per sequence that is very close to seven (6.78). This is the point where the CFPB finds economic irrationality (paying more than one borrowed in fees, without reducing principal).[20] In contrast, the mean for new borrowers, in Cohort 4, is not quite five loans per sequence (4.94), a point where it is harder for the CFPB to infer economic irrationality. The standard deviation for both groups is larger than the mean, suggesting there is a minority-within-the-minority with very expensive outcomes. We see in Appendix B that the 90th percentile (the outcome for the 10 percent of longest sequences) is 17 loans in a sequence in the white paper group and 11 loans in a sequence for new borrowers (Cohort 4). The outliers in both groups show behavior that the CFPB could suggest reflects economic irrationality.

Appendix B also presents values for this analysis for sequences that consist of loans from more than one lender (Loan 3) - the perspective the bureau could not provide because of the way that data was preserved. We see in Table B-2 that adding the effects of crossing the street to another lender does not change median sequence duration at all compared to median sequence duration for a single lender (Table B-1) across all four cohorts.[21] Figure 3-A presents the change in means from Loan 2 to Loan 3 graphically.

## Figure 3-A

### Mean Number of Loans per Sequence (Loan 2 and Loan 3)




As seen in Figure 3-A, mean sequence durations increase slightly for white paper (Cohort 3) borrowers, from 6.78 loans to 7.25 loans, and very little for new (Cohort 4) borrowers, from 4.69 to 4.94 loans. Thus, the bureau should not infer (as it suggested in footnote 15 of the white paper) that cross-lender migration (which tends to increase sequence duration) might show greater average usage of long sequences than the bureau's single-lender analyses. That effect is not enough to offset the skew created by looking at a short duration sample that is biased to heavy users.

20. The median fee for storefront payday is $15 per $100 per loan. In a seven loan sequence (original loan plus six de facto renewals), a borrower pays $105 for borrowing $100.

21. We note that the median sequence duration does increase in the restricted states sample, described above, in which we sampled all borrowers in selected MSAs chosen because state law did not restrict the use of multiple lenders simultaneously. Our Loan 3 analysis captures the effect of using multiple lenders simultaneously or in close chronological proximity, and the frequency of that behavior is reflected in the 11-month median in Table B-2.

## C. Distributions of "Worst" (Longest) Sequence per Borrower

### Takeaways - Section II -C

What's the worst case scenario for each borrower? For each borrower, over four years what is the longest loan sequence?

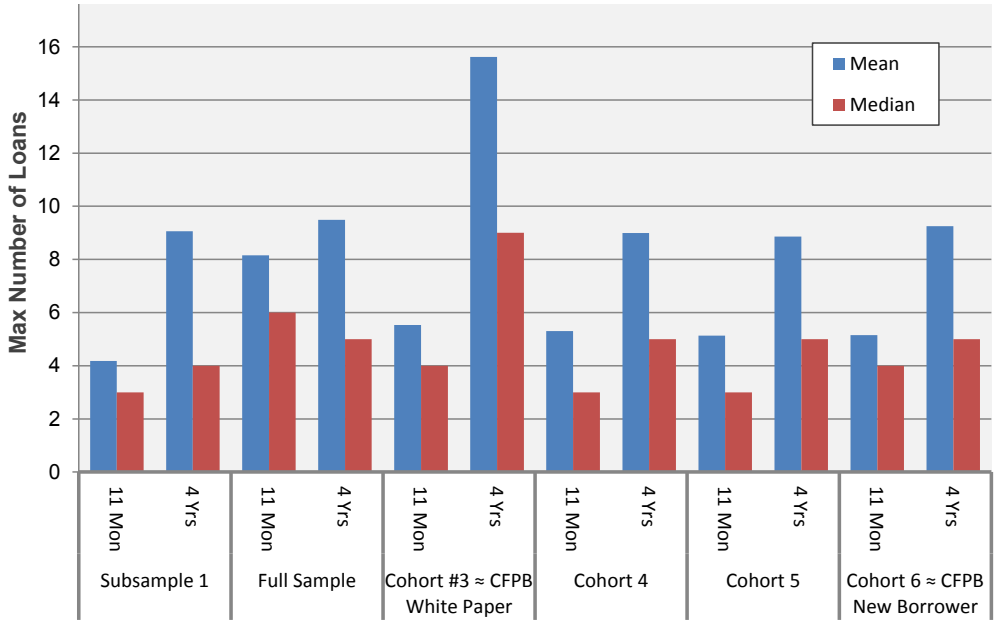
We found:

- For all sampling methods other than the white paper, half of all borrowers never have a sequence longer than five loans, but for the white paper method, that figure is nine loans.
- The average "worst case" for white paper borrowers is nearly 16 loans in a row, but for all other samples others it is less than nine.
- When we add our unique ability to detect borrowers "crossing the street" to another lender to continue a loan sequence, the longest median sequence per borrower does not increase materially.

Another way to analyze the extent to which payday borrowers evidence CFPB harm is to ask: What was the longest sequence per borrower in our various cohorts? What is the worst case experience for each borrower? Appendix C contains the complete distributions for this question. The means and medians for Loan 2 are presented in Figure 4.

## Figure 4

### Maximum Sequence Duration per Borrower (Loan 2)




As Figure 4 illustrates, this way of looking at the data shows even more stark contrasts between the median "worst" sequence length and the mean "worst" (or longest) sequence length. The median of the white paper group is at nine loans and is at five loans for new borrowers, significantly higher than the three loans versus two loans we saw in the distribution of all sequences, as we saw in Figure 3. The mean longest sequence/borrower in the white paper group (Cohort 3) is 15.62 loans, a clear suggestion of CFPB harm. For the newest borrowers shown in Cohort 4, it is almost exactly nine loans, another strong suggestion that the longest sequence for the newer borrowers is, on average, still far too long, and likely CFPB harmful.[22]

This metric, the measurment of the longest sequence per borrower, also demonstrates the relative importance of being able to detect multi-lender sequences, our Loan 3. As shown in Appendix C, the median longest sequence for newer borrowers (Cohorts 4-6) remains at five loans. In contrast, the median longest sequence for the white paper (Cohort 3) borrowers moves from nine to 10 loans in Loan 3. The severity of outlier behavior increases markedly for white paper (Cohort 3), with values for the 90th percentile jumping from 26 to 42 loans in a longest sequence, as between Loan 2 and Loan 3. Notably, the change in values from Loan 2 to Loan 3 for outliers (90th percentile) of newer borrowers (Cohorts 4-6), increases by at most two loans per longest sequence. This is yet another example of the qualitative difference of the behavior of newer borrowers, as compared to the white paper sample. Figure 4-A compares the mean values of longest sequence per borrower in single-lender (Loan 2) versus multi-lender (Loan 3) sequences.

22. Based on the definition of harm by the CFPB in their studies.

## Figure 4-A

### Mean of Longest Sequence Duration per Borrower (Loan 2 and Loan 3)

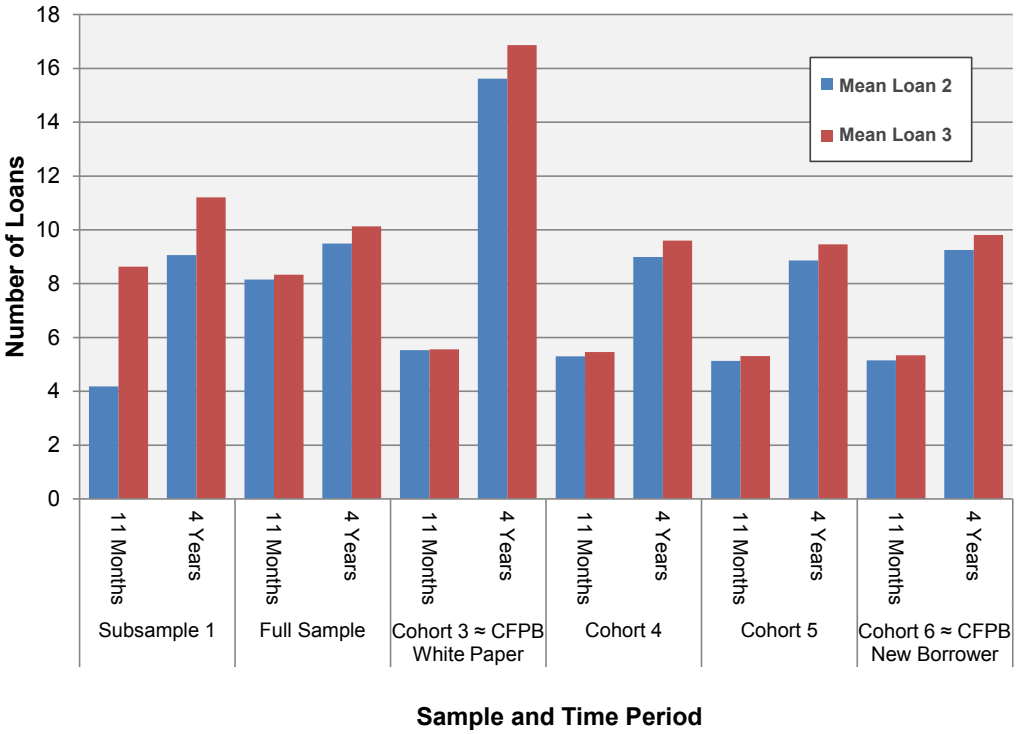



Figure 4-A shows us that being able to detect cross-lender activity will increase mean values only marginally, while outlier differences (discussed above) are significant.

As in prior analyses, the median values for longest sequence do not clearly evidence CFPB harm to consumers (except in the white paper sample), but the mean values are troubling. This tells us that, for many of our test samples, half of all borrowers do not have a worst case sequence, in their entire borrowing history, that clearly meets the test of CFPB harm. If half of borrowers are not CFPB harmed, that raises the policy question: can the CFPB justify shutting off access to the product for *all borrowers*, in order to avoid the harm to the *half* who fall in this category? In the next subsection, we turn to another metric for measuring the *number* of borrowers who might evidence CFPB harm.

## D. Percentage of Borrowers by Longest Sequence

### Takeaways - Section II -D

The average "worst case" per borrower is long, so we looked at the distribution: What percentage of borrowers have a "worst case" of three loans, four loans, etc. ?

We found:

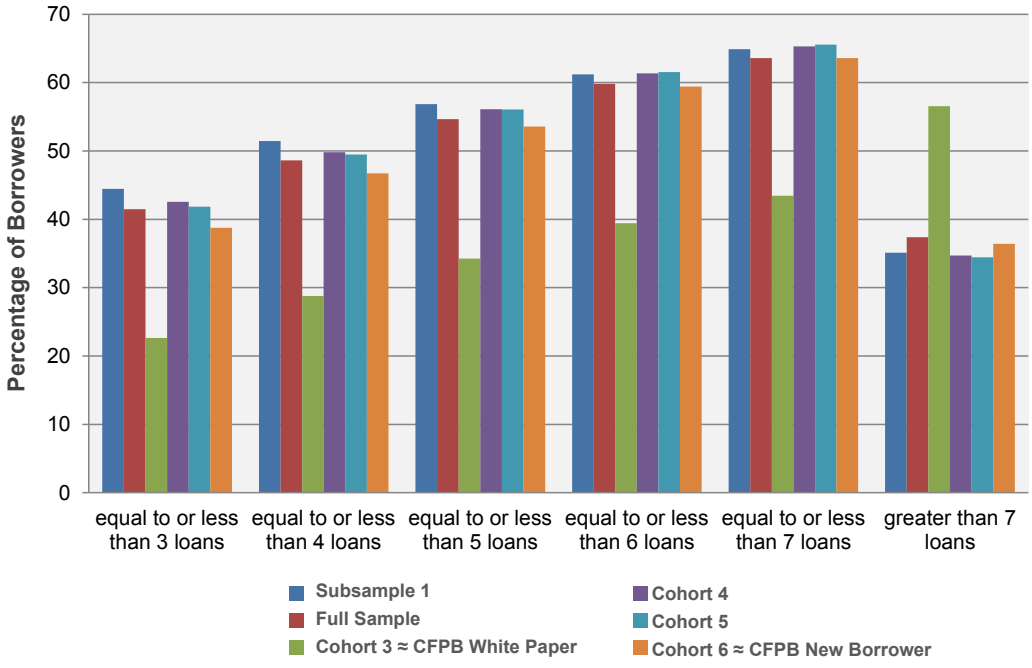
- Looking at any sample of "new" borrowers, 40 percent of borrowers never have more than three loans in a row, 50 percent have no more than four loans in a row, and 60 percent have no more than five loans in a row.

Which raises the question: How important are new borrowers in the policy analysis?

Appendix D presents the maximum number of loans in a sequence of Loan 2 and Loan 3 per borrower, expressed as percentages of borrowers with no more than three loans in a sequence, four loans in a sequence, and so on. Figure 5 presents the data for Loan 2 graphically.

### Figure 5

#### Mean of Longest Sequence Duration per Borrower (Loan 2)



Viewed (or sampled) from any perspective other than the one used in the white paper, a very significant percentage of borrowers show **no** sequential use that allows a clear inference of CFPB harm. Nearly 50 percent of Cohort 4 borrowers have fewer than four loans in the longest sequence over four years. All of the samples, other than the white paper, cross the 60 percent mark when we measure the percentage of those who never had more than six loans in a sequence, a point where it is difficult to infer CFPB harm. The sore thumb in the "greater than seven loans" bucket (the green bar) is a graphic illustration of the skewed effect of the white paper sampling approach, and also evidence of the policy conundrum discussed in the next section. That is, if more than half of all new users do not clearly show CFPB harm, should the bureau be shutting down the industry to protect a minority who behave irrationally?

As shown in Appendix D, the distribution of borrowers by maximum sequence duration does not change materially when we measure Loan 3 multi-lender sequences. In other words, being able to detect borrowers "crossing the street" does not diminish the relatively high percentage of borrowers who never have more than 3-6 loans in a row.
The foregoing perspective raises a very serious issue. If a majority of borrowers will never experience a "debt trap" under any analysis other than the white paper's over sampling of heavy users, which sampling methodology is the most relevant for policy making? We turn to this issue next.

# III. Policy Issues Revealed by the Data and More Data on Those Issues

There are two significant issues in policy analysis of the data: (a) which cohort samples, new borrowers or all borrowers in a month are more relevant for policy guidance, and (b) if the experience of new borrowers is relevant, how should policy makers strike the balance of access versus harm?

## Takeaways - Section III

We argue in this section that the experience of new borrowers (as opposed to everyone in a given month) is important because it is tomorrow's new borrowers who may need this product and will be denied access. In addition, if the "cycle of debt" exists and is broken by the new rule, then all consumers are potential new borrowers as they enter the newly reconstructed market.

On the first issue, we see that newest borrowers in Cohort 4, and even somewhat new borrowers in Cohort 6, have worst-case experiences that are very different from the group who appear to use the product almost continuously. If the bureau is legislating to a snapshot of the current state of the group of borrowers who appear to use the product continuously, it might justifiably ban the product. The bureau has a legislative mandate, however, to consider continued access for those who use the product responsibly, while mitigating harm to those who do not. That balance is contained in the legal definition of "unfairness" and in the bureau's basic Congressional charter in the Dodd-Frank Act.[23] Access is an issue not only for today's non-prime borrower but also for the borrower who enters the market tomorrow. A small-dollar rule that bans the industry will ban it for tomorrow as well as for today.

As we discovered in Report 8, roughly half of the borrowers in this product could afford to pay for a high-rate, short-term amortizing installment loan, and we can see from the data in this report that roughly half of new entrants to the single-payment product can handle the single-payment model without obvious CFPB harm. Unfortunately, as also discussed in Report 8, two thirds of the states that allow payday loans do not permit high-rate installment loans. It is in this context that the bureau must consider whether to allow continued access to payday loans for those who can use the single-payment product without CFPB harm; however, there may be no market alternative for most of them. We would argue that the importance of access for future consumers, together with the limitations imposed on installment product alternatives to payday by the state legal landscape, make it important for the bureau to consider new borrower data analysis.

23. See Section 1021(a) of the Dodd-Frank Act, 15 U.S.C. Section 5511. The CFPB is charged to implement and enforce the federal consumer financial laws "for the purpose of ensuring that all consumers have access to markets for consumer financial products and services...."

On the second issue, striking a balance of access versus harm, the relative counts of borrower samples present a regulatory conundrum. The size of the full sample in this report is over 4 million borrowers. Cohort 3 (white paper borrowers) from January 2010 numbers roughly one million. Counts of new borrowers, who are included in the various sub-samples of Cohort 3, range from just under 100,000 to under 200,000. Thus, in any given month, the significant majority of borrowers at a payday store (75-80 percent of Cohort 3) are heavier users whose patterns of use allow an inference of CFPB harm. How should the bureau allow access for the minority of new users while mitigating harm to the heavy users, and possibly incenting an evolution to an industry that focuses on serving new, less intense users?

In answering this question, the bureau needs a view of the relative size, over time, of the populations of heavy versus light users, a longitudinal view over time. Our cohorts drawn from January 2010 give us a snapshot of the numbers of heavy versus light users in one month, but that snapshot does not tell us whether an equal distribution of those unique users will remain in the sample over any given period of time (whether a longitudinal view of the unique IDs in the population will maintain a static distribution). By way of example, one can imagine a longitudinal distribution in which the same individuals are heavy users for four years, but the 20-25 percent who are light users are replaced every year by new, light users. In that scenario, over a four-year period, a roughly equal number of consumers would be characterized by heavier and lighter use, and the balance of access versus harm would strive to preserve product access for the half of the user population who are not harmed.

## A. Longitudinal Analysis of Unique Borrower Population by Usage Pattern

We tested this possible scenario in two steps. First, we tracked the unique user IDs in each of Cohorts 3-6 over four years, observing the rate at which those unique IDs appeared annually in each December of 2010-2013. Second, we analyzed the population of replacement borrowers in those December measurement months to assess the distribution of heavier and lighter users in the replacement group.

Table 8 shows the first analysis. We broke the four cohorts into unique borrower IDs (Cohort Prime as described in the table), with Cohort 3 Prime consisting only of those IDs not found in Cohorts 4, 5 and 6 (thus, purely heavier users) and Cohorts 4, 5 and 6 similarly redefined as Cohorts 4 Prime, 5 Prime and 6 Prime, to separate IDs that appear in more than one cohort.

## Takeaways - Section III-A

Because new borrowers are important, we wanted to know how common they are over time. That requires a longitudinal analysis (looking at the replacement over time of initial users with new users and testing what kind of users they become).

We found:

The group of experienced, heavy users in our initial sample persisted for a long time. Their attrition rate was very slow, but new, lighter users tended to leave the population much more quickly and be replaced with a greater percentage of lighter users than we found in the initial one-month sample.

## Table 8

### Count of January 2010 (Prime) Cohort Borrowers Appearing in Subsequent Months (December)

| Unique Borrowers | Cohort Prime | Dec-10 | Dec-11 | Dec-12 | Dec-13 |
|---|---|---|---|---|---|
| **3 Prime** | 822,735 | 419,871 | 319,653 | 247,393 | 195,726 |
| **4 Prime** | 91,644 | 26,602 | 19,281 | 14,183 | 11,072 |
| **5 Prime** | 24,744 | 8,407 | 6,331 | 4,909 | 3,836 |
| **6 Prime** | 73,917 | 28,361 | 21,905 | 16,949 | 13,495 |

Cohort 3 Prime: Loan exists January 2010 | Loan also exists December 2009
This is Cohort 3 IDs excluding IDs that are in Cohort 4, 5 and 6)

Cohort 4 Prime: Same as current Cohort 4

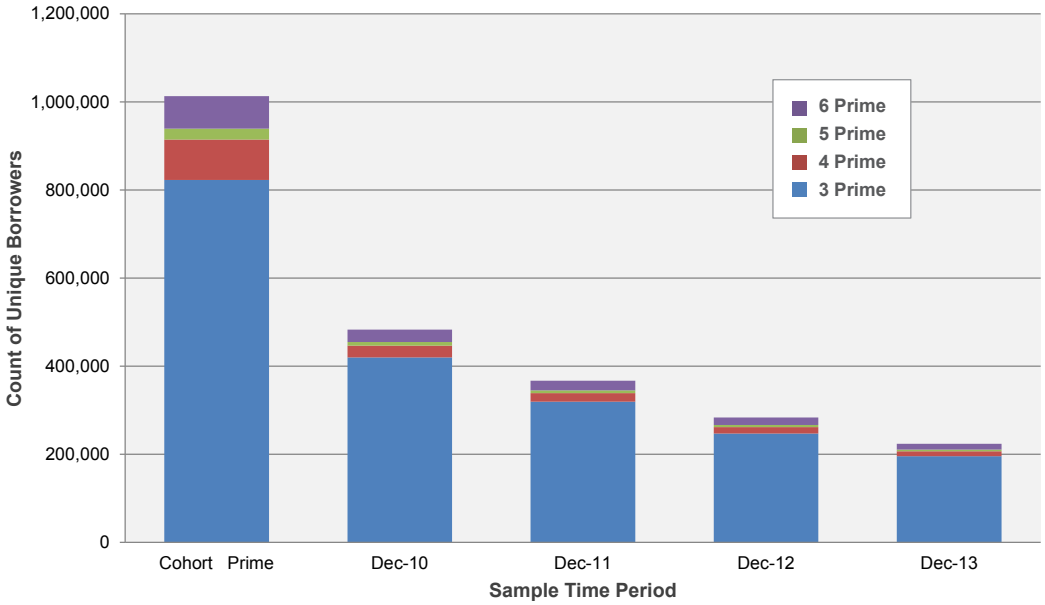
Cohort 5 Prime: Loan exists January 2010 | no loan Oct Nov Dec 2009 | loan does exist in July-Sept 2009
(Cohort 4 IDs but excluding IDs in Cohort 4).

Cohort 6 Prime: Loan exists January 2010 | no loan Dec 2009 | but loan does exist Oct-Nov 2009
(Cohort 6 IDs but excluding those in Cohorts 5 and 4)

As seen in Table 8, heavier users who make up Cohort 3 Prime are very slow to leave the product. More than half of them appear again in December 2010 and nearly 40 percent have a loan in December 2011. Roughly 25 percent are still in the product in December 2013. In contrast, more than 70 percent of Cohort 4 Prime borrowers are gone by December 2010 and only 12 percent remain in December 2013. Thus, the trends support a version of our hypothesis: the majority of heavy users in any given month are the same individuals that are observed as heavy users a year later. More accurately, the rate of attrition (and possibly replacement) of heavy users is around 50 percent in the first year and then slows to about 20 percent a year. The rate of attrition (and possibly replacement) of light users is much faster.

Figure 6 illustrates the differential attrition rates of different user prime cohorts.

## Figure 6
### Differential in Attrition Rates by Prime Cohort




We then turned to the issue of measuring the count of replacement borrowers seen in December and their usage characterization. We focused on the replacement borrowers seen in each of the December measuring months in 2010-2013 (i.e., those unique IDs that appeared in a December borrower group but were not seen in January 2010). We sampled each of the replacement borrower groups into new Cohorts 3, 4, 5 and 6 in order to represent heavier and lighter users amongst the replacement borrowers. We also further sub-sampled the December measurements by counting those in December 2011, 2012 and 2013 who were new compared not only to January 2010 IDs but also did not appear in any prior December group. This approach allows us to observe heavier versus lighter users appearing as replacements as compared to both the original January group and as compared to each previous December group.

Table 9 shows the results. This table is complex and requires explanation before observation. First, the line for Prior January 2010 Exclusions represents the count of unique borrowers in any December group who are excluded from the count of replacements because they were part of our original January 2010 Cohorts. The three December columns marked with the letter (i) show the effect on the replacement counts of excluding unique IDs that appeared in prior December counts. Finally, the computation of replacement Cohorts 3, 4, 5 and 6 apply the measures of recent loan use (e.g., no loan in prior month, no loan in prior three months, etc.) set out above, to characterize the unique *replacement* borrowers.

## Table 9
### December Borrowers by Vintage and by Recent Use

| Original Cohorts | | December Borrowers by Vintage and Recency | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Jan-10 | | | Dec-10 | Dec-11 | Dec 2011 (i) | Dec-12 | Dec 2012 (i) | Dec-13 | Dec 2013 (i) |
| 1,013,070 | | Total Ids | 982,594 | 1,044,978 | 1,044,978 | 921,136 | 921,136 | 823,926 | 823,926 |
| | | Prior Jan 2010 IDs Excluded | 447,420 | 367,170 | 367,170 | 283,434 | 283,434 | 224,129 | 224,129 |
| | | Prior Dec 2010 IDs Excluded | | | 224,555 | | 338,821 | | 346,954 |
| | | Prior Dec 2010 and 2011 IDs Excluded | | | | | | | |
| | | Prior Dec 2010, 2011 and 2012 IDs Excluded | | | | | | | |
| Jan-10 Original Cohorts | | Replacement Groups | New IDs In Dec 2010 Excluding Jan 2010 IDs | Dec-11 | New IDs in Dec-2011 Excluding Dec-2010 IDs | Dec-12 | New IDs in Dec-2012 Excluding Dec-2010 and Dec-2011 IDs | Dec-13 | New IDs in Dec-2013 Excluding Dec-2010, Dec-2011 and Dec-2012 IDs |
| Jan-10 Cohort 3: | 1,013,070 | December Cohort 3 | 535,174 | 677,808 | 453,253 | 637,702 | 298,881 | 599,797 | 252,843 |
| Jan-10 Cohort 4: | 91,644 | December Cohort 4 | 104,838 | 79,699 | 77,926 | 78,681 | 76,035 | 77,044 | 73,442 |
| Jan-10 Cohort 5: | 116,418 | December Cohort 5 | 122,589 | 103,053 | 98,857 | 100,069 | 93,724 | 97,995 | 89,890 |
| Jan-10 Cohort 6: | 190,335 | December Cohort 6 | 175,319 | 174,353 | 159,029 | 163,897 | 139,705 | 161,102 | 130,922 |

| | |
|---|---|
| December Cohort 3 | The full cohort from December of that year: excluding borrowers who have a loan in Jan 2010 |
| December Cohort 4 | The full cohort from December of that year: excluding any borrowers who had a loan in the previous 6 months (June-November inclusive) and in January 2010 |
| December Cohort 5 | The full cohort from December of that year: excluding any borrowers who had a loan in the previous 3 months (September-November inclusive) and in January 2010 |
| December Cohort 6 | The full cohort from December of that year: excluding any borrowers who had a loan in the previous 1 months (November) and in January 2010 |

## Takeaways - Section III-A

We followed the attrition of users and replacements over a four-year period, observing the initial group and their replacements, taking a count every December of continuing users and replacements. We also sorted the replacements by the frequency of their recent use (which we have seen is a good predictor of their future intensity of use).

We found:

- About a quarter of the heavier users from our initial group were still using the product in the final year
- About 50 percent of the users in the last December sample were replacements with more than a year in the product
- About a quarter of the users were fully replaced every year and were lighter users when we detected them

Table 9 reveals a number of trends. First, looking at the replacement members of Cohort 3 at the bottom half of the table, we see new IDs making up a little more than half of total IDs in each December, after excluding only the January 2010 IDs. Note also that the count of January 2010 excluded IDs in December 2010 (447,420 IDs) is made up almost entirely of Cohort 3 Prime (heavy users) from January 2010 (who numbered 419,871 in Table 8). Second, and more important, we see that the percentage of replacement borrowers who are lighter users is nearly double the percentage of lighter users we observed in our January 2010 count of all borrowers. That is, Cohort 6 lighter users made up about 19 percent of all borrowers in January 2010. In contrast, Cohort 6 lighter replacement users in December 2010 make up about 33 percent of all replacement borrowers in December 2010. A similar relationship persists across each of the December measurements. Lighter users make up about twice the percentage of annual replacements as compared to their percentage of a static measurement of total original users. This is consistent with our hypothesis that heavier users persist in the borrower population for a long time, while lighter users are replaced with other lighter users over a fairly short period.[24]

The last column in the table prompts another observation. About a quarter of the IDs in December 2013 are hold-overs from January 2010. Their count (224,129) is almost entirely made up of Cohort 3 Prime heavy users from January 2010 that we tracked specifically over four years in Table 8 (195,726). Almost half of the IDs in December 2013 (346,954) are hold-overs from prior December groups who were not in the January 2010 Cohort. The last quarter of users in December 2013 is primarily lighter users who are recent additions to the population.

Finally, the application of various layers of prior-use exclusion to replacement borrowers in each December group shows that most light users are truly newer users. The count of replacement users in Cohort 6 (lighter users) each December ranges from 160,000 to 175,000. This is the count of December Cohort 6 members who were not in January 2010. When we then apply a further exclusion for any ID seen in a prior December count, the count is only minimally affected -- the vast majority of new lighter users have not been seen in any year before.

We can thus return to our effort to measure longitudinally the count of consumers who are at risk of CFPB harm in the product versus those who are not, over time. We can see that at any time the population is roughly 75 percent heavy users who have a propensity (but not a certainty) for long sequences of loans to which the CFPB would object. That group of individuals is relatively static, taking four full years to replace three quarters of their population. The other 25 percent of users (those less at risk) is almost fully refreshed every year, as demonstrated by the congruence between the count in each December of Cohort 6 (excluding only January 2010) and Cohort 6 (excluding all prior observations).

24. Note how this observation aligns with our findings, above, that borrower samples over-weighted to heavy users, like Cohort 3, have a much longer average life cycle in the product than newer, lighter users in Cohort 4.

We can apply this knowledge of the dynamics of membership in a payday borrower population over time to illustrate the relative size over a four-year period of (a) the group of persistent heavier users, versus (b) the constantly-refreshed, smaller group of lighter users. Assume a constant borrower count of 1,000 in every month. We know that the 250 lighter users we see in January of 2010 (call that Year 0) will be replaced each December over four years with almost 250 new lighter users. Thus, the count of unique lighter users we can identify over our entire period will be close to 1,250. A more complex calculation is needed to compute the count of original plus replacement heavier users over the same period. In Tables 10, 10-A and 10-B , we show a method to compute the sum of original plus replacement heavier users over our sample period, using the attrition rates we see in Cohort 3 Prime in Table 8.

First, we compute the attrition rates of Cohort 3 Prime, both as percentages and then expressed algebraically, in Tables 10 and 10-A.

## Takeaways - Section III-A

We modeled the count of people over our four-year sample who are lighter versus heavier users.

We found that about 40 percent of people using the product are not likely to ever experience harm, as defined by the CFPB, based on their use pattern.

We also argue that, based on the reasons that consumers use payday loans, a third of those "harmed" by a long loan sequence (by paying more than they borrowed) are still **helped** by the loan, because it forestalls the loss of housing or transportation. We thus conclude that banning this product could well harm as many consumers as it helps.

## Table 10

### Attrition Rate Cohort 3 Prime

| | | Rate of Attrition | |
|---|---|---|---|
| $Y_0$ | 822,735 | | |
| $Y_1$ | 419,871 | $r_1$ | 51% |
| $Y_2$ | 319,653 | $r_2$ | 76% |
| $Y_3$ | 247,393 | $r_3$ | 77% |
| $Y_4$ | 195,726 | $r_4$ | 79% |

## Table 10-A

### Algebraic Representation of Replacement Rates in Hypothetical Sample

| | | |
|---|---|---|
| **$r_1$ =0.51** | $Y_0 = 750$ | $Y_0 = 1.0 * Y_0$ |
| **$r_2$ =0.76** | $Y_1 = 750 - r_1(Y_0)$ | $Y_1 = 0.48966 * Y_0$ |
| **$r_3$ =0.77** | $Y_2 = 750 - r_1(Y_1) - r_1(r_2)*(Y_0)$ | $Y_2 = 0.36158 * Y_0$ |
| **$r_4$ =0.79** | $Y_3 = 750 - r_1(Y_2) - r_1(r_2)*(Y_1) - r_1(r_2)*(r_3)*(Y_0)$ | $Y_3 = 0.32453 * Y_0$ |
| | $Y_4 = 750 - r_1(Y_3) - r_1(r_2)*(Y_2) - r_1(r_2)(r_3)*(Y_1) - r_1(r_2)(r_3)(r_4)*(Y_0)$ | $Y_4 = 0.30549 * Y_0$ |

Then we apply these attrition rates to the original 750 heavy users and to each additional group of replacement heavy users, to compute a total of heavy users in our constant population of 1,000 borrowers spanning Year 0 to Year 4, in Table 10-B.

## Table 10-B

### Replacement Rate of Hypothetical Borrowers With Attrition Rates

| | $Y_0$ | $Y_1$ | $Y_2$ | $Y_3$ | $Y_4$ |
|---|---|---|---|---|---|
| **Rate of Attrition** | 51% | 76% | 77% | 79% | |
| $r_1$ | 750 | 383 | 291 | 226 | 178 |
| $r_2$ | | 367 | 187 | 143 | 113 |
| $r_3$ | | 750 | 271 | 138 | 105 |
| $r_4$ | | | 750 | 243 | 124 |
| | | | | 750 | 229 |
| | | | | | 750 |
| **Prior Year Carry-Over** | | 383 | 479 | 226 | 178 |
| **Replacements** | 750 | 367 | 271 | 243 | 229 |
| **Total Replacements** | 367+271+243 =1111 | | | | +750=1861 |

Table 10-B allows us to see that we would have a total of 1,861 unique heavier users versus 1,250 unique lighter users occupying our constant count of 1,000 borrowers per month over four years. Thus, roughly 40 percent of the users are consumers who have a good chance of using the product in an economically beneficial fashion. Figure 5 tells us that roughly 60 percent of these lighter users will never have a loan sequence in which fees exceed the amount borrowed.

This is admittedly a very rough measure. It divides the users into just two groups that we can fairly view as heavier or lighter users.[25] But we know that there are some outliers with longer loan sequences amongst the lighter users, and the converse is also true. There are some persistent users with shorter maximum sequence duration who drag the median back down below the level of inference of CFPB harm. That is shown by the fact that the median sequence duration for all groups is much lower than the mean. Nevertheless, this way of characterizing the people who use the product is more relevant to the legal issue of unfairness than simply pointing out that a large number of loans occur in long sequences. If the bureau is concerned about the effect of loss of access for people, it should perform an analysis of changing populations over time, much more time than 12 months. Moreover, the bureau should not forget that it cannot yet quantify the opportunity cost for users of very long sequences who, absent such an expensive loan, could lose much more than the cost of fees that exceed the principal. For example, the alternative to a long sequence could be to lose the ability to get to work or to lose housing.

Credible, third-party data does exist on this last issue (opportunity cost of lack of access). It may make sense to pay more than you borrow if the alternative to borrowing is loss of transportation or housing. Such a loss could cost more than a loan at twice the price of a loan. The Center for Financial Services Innovation (CFSI) has studied the uses of short-term loans, cross tabulating those who use the product for particular purposes with types of products used and intensity of use and financial circumstances of the users.[26] CFSI found that roughly a third of those who use short-term loans do so for emergency expenses or to bridge a timing gap between income and expense (as opposed to those whose expenses chronically exceed income). A majority of the uses for emergencies relate to car repair, medical expenses and housing. For those bridging timing gaps, 75 percent are paying for utilities and rent. Thus, one could fairly argue that a third of those who are using the product in a way that the CFPB defines as harmful, may well have an economic benefit, even though the cost of the loan may exceed the amount borrowed. We found that 60 percent of people may meet the CFPB definition of “harm.” Using CFSI data, a third of those who pay more than they borrow(CFPB harm) arguably are not harmed (thus, another 20 percent of our people). Added to the 40 percent who do not meet the CFPB’s overuse standard, we find 60 percent who logically should be permitted access to this product.

25. We are computing, and will include in a future report, a statistically precise count based on a random sample of 1,000 users every month, beginning in July 2009. We will observe their replacement over three and a half years with new users and report the usage characteristics, over four and a half years, of original users and replacement users, as well as the total count of consumers who make up a constant monthly population of 1,000 borrowers.

26. Know Your Borrower: The Four Need Cases of Small-Dollar Credit Consumers (December 2013), available at http://www.cfsinnovation.com/Document-Library/Know-Your-Borrower-The-Four-Need-Cases-of-Smal-(1).aspx. Less robust studies by Pew Research also support a finding that housing and transportation emergencies are a significant source of demand for small-dollar credit.

# IV. A Policy Alternative

How could the bureau protect the 40 percent who "overuse" the product, while retaining access for those who very likely do not?

Our research shows that there are extreme outliers in the usage patterns. In Appendix Table C-2, we see 10 percent of borrowers with multi-lender sequences lasting more than a year and a half. These are behaviors that make no sense. We suggest starting where the inference of harm is much weaker, paying less than you borrowed, or six loans in a row. The other half of the problem is that payday borrowers are simply treading water, paying only fees. Colorado has addressed this problem by forcing payday loans into an installment model, and the industry has contracted but not disappeared.[27] As explained in our Report 8, state law limiting high rates on installment loans limits an installment alternative to roughly 13 of the 34 payday states, but that limitation also suggests an approach.

We would suggest the bureau require the offering of an underwritten installment product in states where that is permitted at rates equal to (or approaching) the current charge for payday loans. It should also require such installment lenders to review recent small-dollar product use as a condition on permissibility of making a high-rate installment loan to avoid multiple simultaneous loans and sequences of high-rate installment loans. The review would detect whether the borrower is already in a covered loan, or very recently paid one off. Such a product would be underwritten to measure ability-to-repay based on residual income in the manner suggested in the SBREFA Outline, but the bureau must tailor verification requirements for underwriting inputs (such as income and rental expense) to avoid unsustainable costs of manual work.

In other states that do not allow high-rate installment loans, the bureau might also learn from the data it has and that is presented here, allowing up to six safe-harbored payday (single-payment) loans in states that prohibit high-rate installment, but only for borrowers who have no industry-wide history of payday borrowing in the last 30 days. We can see from Figure 5 that 60 percent of those subjected to such a screen will never use more than six loans in a row (i.e., will use payday without clear evidence of CFPB harm). The bureau can also help those in the 40 percent of new borrowers who end up exceeding six loans in a sequence by requiring an amortization plan[28] for the balance outstanding after six loans and require an industry-wide cooling off period for borrowers who use the amortization "off ramp."[29]

27. When Colorado changed its small-dollar product from single-payment to installment, the industry consolidated and the number of borrowers per store doubled. See http://www.pewtrusts.org/~/media/assets/2014/12/pew_co_payday_law_comparison_dec2014.pdf. See Report 8.

28. Such a plan would bear interest at whatever local law allows for an installment loan refinancing the payday balance.

29. We would also suggest that the bureau needs to reconsider its proposal to tack together all loans made within 60 days of the last loan in their definition of sequence. Having laid out all of its analysis of harm in the white paper and the *Data Point* by tacking together loans made within the same pay period after of payoff of the prior loan, and having presented no data to support its new position (in the SBREFA Outline) that the financial shock of paying off a loan lasts 60 days, the new 60-day approach creates risk by relying on foundational data studies, when those studies use analytical methods that are inconsistent with the requirements of the proposed rule.

If it accepts the idea that it should not ban a product where it cannot clearly infer harm (paying more than you borrow), then the bureau must also drop its two additional proposed restrictions on total borrowing in 12 consecutive months. As we have demonstrated in other research,[30] the two proposed 12-month restrictions each shut off more than half of the business seen today. We would argue that they make no sense if the bureau limits each borrower to a single small-dollar loan, which itself is designed to avoid harm.

We have computed the effect of this proposal on loan volume where a single-payment loan product would continue to be offered. Using the same modeling engine that predicts a 70 percent loan volume reduction under the bureau's proposal,[31] we find that a six-loan maximum followed by a 30-day cooling off period would reduce loan volume by 20.67 percent. If one adds four pay cycles for an installment payoff after the sixth loan, and then follows with a 30-day cooling off period, overall volume reduction from this proposal is 36.36 percent. Although this would be a substantial income shock to the industry, it is possible that the type of store consolidation experienced in Colorado would allow continued access to small-dollar loans from some participants in the current industry. Certainly, this option is more survivable than a 70 percent volume reduction under the bureau proposal.

Finally, the bureau can control for "phantom harm," the over-drafting of bank accounts by lenders who use account access, by limiting a lender's resubmission of bank-returned ACH or deferred check debits to consumer bank accounts (an approach already proposed in the SBREFA Outline).

For those familiar with the SBREFA Outline, all of the components of this approach have already been suggested by the bureau. The power of this argument is that the data approach in the white paper over weights the indicia of CFPB harm on which the bureau relies, and the testing for sensitivity to sampling bias in *Data Point* uses too short a data set in analyzing the skew of the white paper. Those facts suggest legal caution in imposing restrictions that make impossible a viable lending industry. This paper suggests an approach that will minimize harm to those who over-use the single-payment product, both through retaining access for relatively newer users and through off ramps for those newer users who fall into extended sequences.

30. See https://www.nonPrime101.com/blog/clarity-analysis-confirms-cfpb-simulation/

31. The modeling engine uses a subsample from the same data used in this report. The modeling engine is fully described at https://www.nonPrime101.com/blog/clarity-analysis-confirms-cfpb-simulation/

# Conclusion

We believe the white paper was constructed in good faith in an effort to tease out the possible correlation of payday use patterns and the legal boundaries of unfair, deceptive and abusive acts and practices. The same good faith effort is evident in *Data Point*. This paper focuses on the undetected limitations in the duration of the data sample used by the bureau and the questions that can be answered with a sample that is truly representative of a full life cycle of product use by a group of consumers. It may well be that the bureau has subsequently assembled a longer data set in its continuing supervision of payday lenders and will publish its own answers to the same questions in conjunction with the Notice of Proposed Rulemaking now scheduled for February 2016. If that happens, we hope that the bureau will recognize the significance of the population who use the single-payment product without clear evidence of "harm" and design an intervention that does not deprive them of access to that product.

# Appendix A

## Key:

**Subsample 1:** All users in geographic proxy for light regulated states (no restriction on multiple lenders / loans)

**Full Sample:** All users in our sample

**Cohort 3:** All users in January 2010 (corresponds with the CFPB white paper)

**Cohort 4**: Same as Cohort 3 but excluding anyone with a loan in prior six months

**Cohort 5:** Same as Cohort 3 but excluding anyone with a loan in prior three months

**Cohort 6:** Same as Cohort 3 but excluding anyone with a loan in prior one month: (corresponds with the CFPB's *Data Point* and its definition of new users)

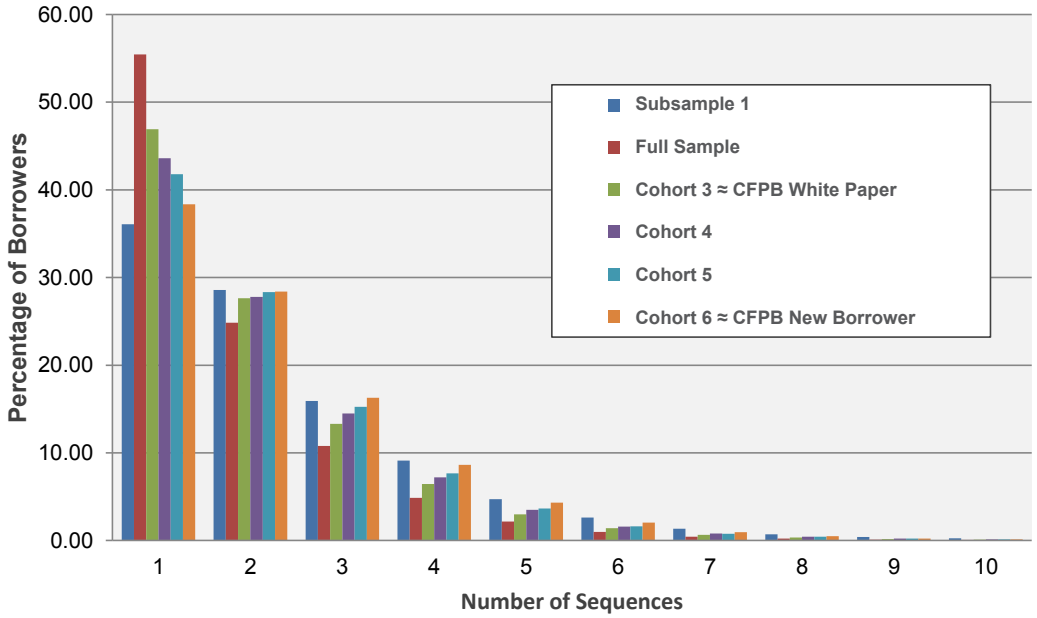
## Table A-1

### Distribution of 11-Month Samples and Percentage of Borrowers by Number of Sequences (Loan 2)

| Number of Sequences per Borrower | Subsample 1 | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 5 | Cohort 6 ≈ CFPB "New Borrower" |
|---|---|---|---|---|---|---|
| 1 | 36.07 | 55.45 | 46.92 | 43.61 | 41.79 | 38.37 |
| 2 | 28.57 | 24.85 | 27.63 | 27.78 | 28.35 | 28.41 |
| 3 | 15.92 | 10.80 | 13.31 | 14.50 | 15.25 | 16.28 |
| 4 | 9.13 | 4.88 | 6.44 | 7.20 | 7.66 | 8.62 |
| 5 | 4.71 | 2.15 | 2.97 | 3.51 | 3.65 | 4.32 |
| 6 | 2.62 | 0.97 | 1.40 | 1.59 | 1.61 | 2.04 |
| 7 | 1.35 | 0.44 | 0.65 | 0.79 | 0.78 | 0.95 |
| 8 | 0.70 | 0.22 | 0.33 | 0.44 | 0.42 | 0.49 |
| 9 | 0.41 | 0.11 | 0.16 | 0.23 | 0.21 | 0.22 |
| 10 | 0.24 | 0.06 | 0.09 | 0.13 | 0.13 | 0.13 |

## Figure A-1

### Distribution of 11-Month Samples and Percentage of Borrowers by Number of Sequences (Loan 2)



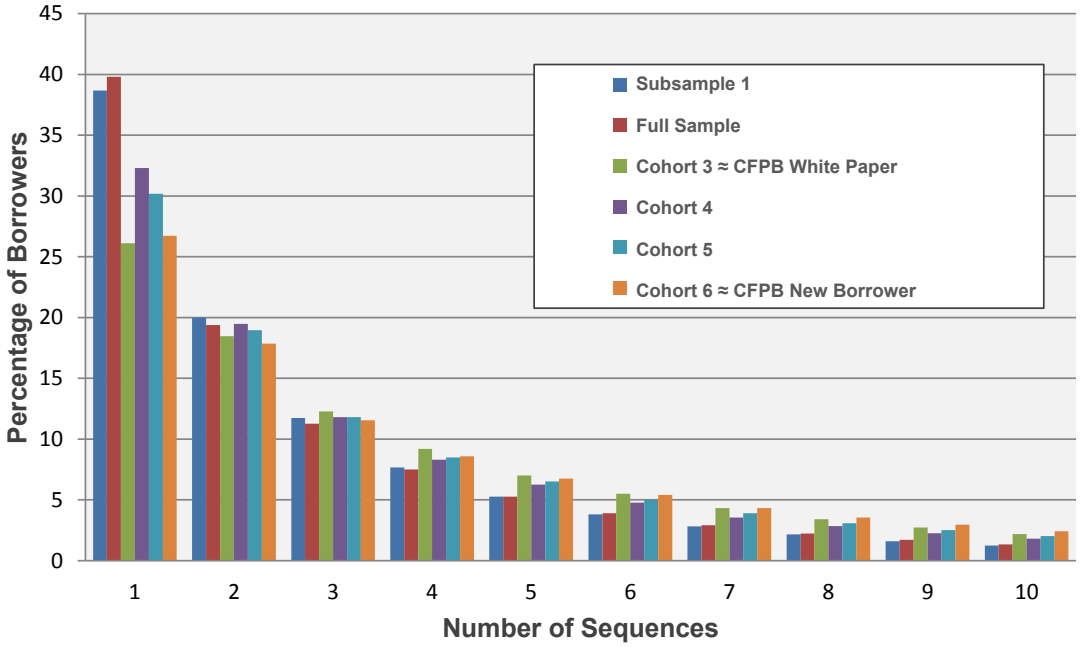
## Table A-2

### Distribution of Four-Year Samples and Percentage of Borrowers by Number of Sequences (Loan 2)

| Number of Sequences per Borrower | Subsample 1 | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 5 | Cohort 6 ≈ CFPB New Borrower |
|---|---|---|---|---|---|---|
| 1 | 38.67 | 39.80 | 26.12 | 32.29 | 30.18 | 26.72 |
| 2 | 19.98 | 19.37 | 18.45 | 19.47 | 18.96 | 17.85 |
| 3 | 11.72 | 11.25 | 12.28 | 11.79 | 11.80 | 11.55 |
| 4 | 7.67 | 7.49 | 9.18 | 8.29 | 8.49 | 8.57 |
| 5 | 5.27 | 5.27 | 6.99 | 6.25 | 6.50 | 6.75 |
| 6 | 3.81 | 3.90 | 5.50 | 4.76 | 5.00 | 5.41 |
| 7 | 2.82 | 2.91 | 4.31 | 3.54 | 3.89 | 4.31 |
| 8 | 2.15 | 2.22 | 3.41 | 2.83 | 3.06 | 3.53 |
| 9 | 1.60 | 1.70 | 2.72 | 2.25 | 2.50 | 2.95 |
| 10 | 1.23 | 1.32 | 2.17 | 1.80 | 2.01 | 2.41 |

## Figure A-2

### Distribution of Four-Year Samples and Percentage of Borrowers by Number of Sequences (Loan 2)



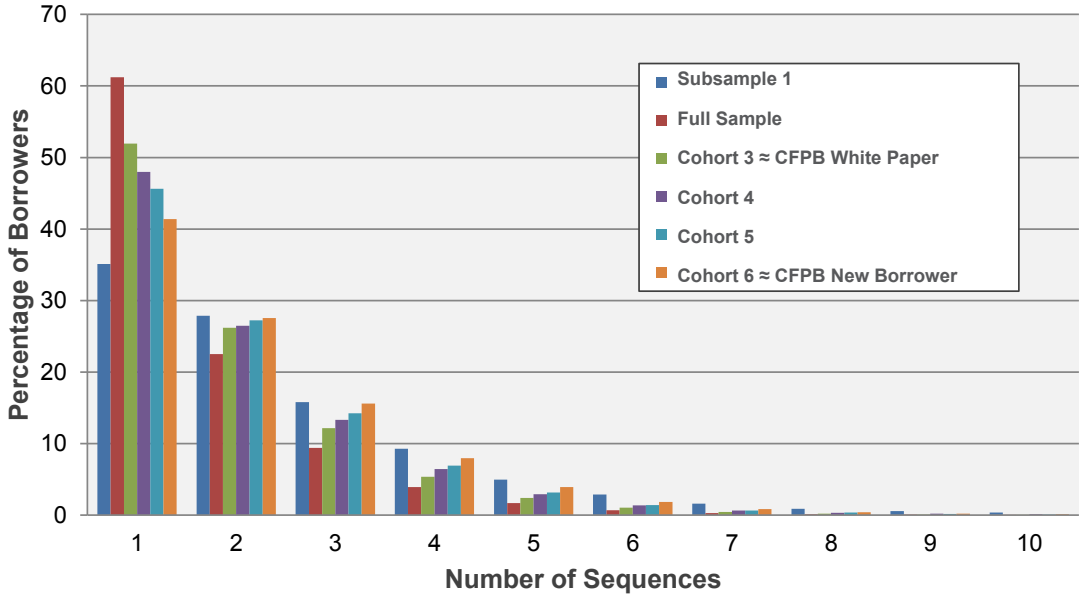
## Table A-3

### Distribution of 11-Month Samples and Percentage of Borrowers by Number of Sequences (Loan 3)

| Number of Sequences Per Borrower | Subsample 1 | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 5 | Cohort 6 ≈ CFPB "New Borrower" |
|---|---|---|---|---|---|---|
| **1** | 35.11 | 61.2 | 51.96 | 47.98 | 45.63 | 41.37 |
| **2** | 27.89 | 22.52 | 26.19 | 26.47 | 27.23 | 27.54 |
| **3** | 15.81 | 9.42 | 12.17 | 13.34 | 14.26 | 15.62 |
| **4** | 9.27 | 3.93 | 5.38 | 6.43 | 6.93 | 7.98 |
| **5** | 4.97 | 1.67 | 2.39 | 2.94 | 3.15 | 3.92 |
| **6** | 2.87 | 0.7 | 1.05 | 1.38 | 1.4 | 1.84 |
| **7** | 1.62 | 0.3 | 0.46 | 0.65 | 0.66 | 0.86 |
| **8** | 0.89 | 0.14 | 0.21 | 0.34 | 0.35 | 0.41 |
| **9** | 0.57 | 0.07 | 0.1 | 0.19 | 0.17 | 0.2 |
| **10** | 0.36 | 0.03 | 0.05 | 0.11 | 0.1 | 0.11 |

## Figure A-3

### Distribution of 11-Month Samples and Percentage of Borrowers by Number of Sequences (Loan 3)



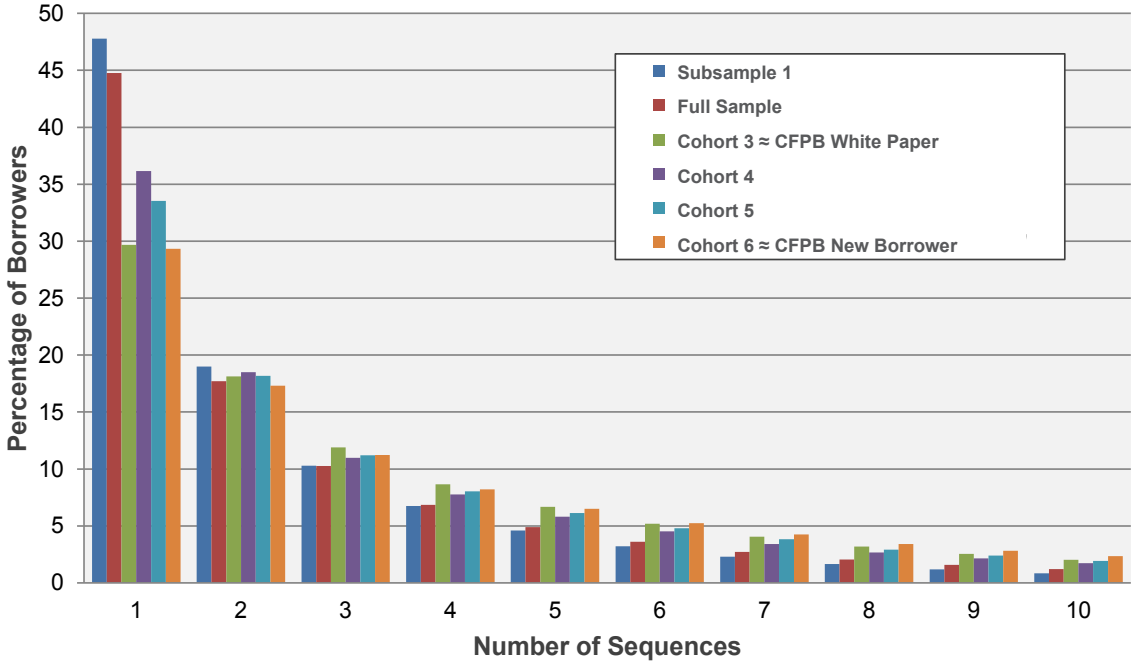
## Table A-4

### Distribution of Four-Year Samples and Percentage of Borrowers by Number of Sequences (Loan 3)

| Number of Sequences per Borrower | Subsample 1 | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 5 | Cohort 6 ≈ CFPB "New Borrower" |
|---|---|---|---|---|---|---|
| **1** | 47.78 | 44.77 | 29.67 | 36.15 | 33.54 | 29.34 |
| **2** | 18.99 | 17.72 | 18.12 | 18.51 | 18.18 | 17.31 |
| **3** | 10.3 | 10.27 | 11.89 | 10.99 | 11.2 | 11.24 |
| **4** | 6.74 | 6.85 | 8.66 | 7.77 | 8.03 | 8.22 |
| **5** | 4.59 | 4.9 | 6.67 | 5.81 | 6.14 | 6.5 |
| **6** | 3.21 | 3.62 | 5.2 | 4.52 | 4.8 | 5.25 |
| **7** | 2.31 | 2.71 | 4.06 | 3.42 | 3.83 | 4.25 |
| **8** | 1.66 | 2.06 | 3.19 | 2.68 | 2.93 | 3.41 |
| **9** | 1.19 | 1.58 | 2.54 | 2.16 | 2.41 | 2.83 |
| **10** | 0.85 | 1.22 | 2.03 | 1.74 | 1.94 | 2.34 |

## Figure A-4

### Distribution of Four-Year Samples and Percentage of Borrowers by Number of Sequences (Loan 3)



# Appendix B: Number of Loans per Sequence

## Table B-1
### Number of Loans per Sequence (Loan 2)

| | Restricted States | | No Restriction | | January 2010 Cohort | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Full Period | | Full Period | | No Restriction | | Not Seen in Previous 6 Months | | Not Seen in Previous 3 Months | | Not Seen in Previous 1 Month | |
| | 351,633 | 975,843 | 2,936,680 | 13,065,180 | 1,584,642 | 3,936,201 | 198,419 | 331,195 | 253,406 | 459,842 | 430,007 | 832,704 |
| | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years |
| **Statistic** | Subsample 1 | Subsample 1 | Full Sample | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 4 | Cohort 5 | Cohort 5 | Cohort 6 ≈ CFPB New Borrower | Cohort 6 ≈ CFPB New Borrower |
| **Minimum** | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| **Maximum** | 49 | 208 | 46 | 370 | 46 | 370 | 72 | 199 | 60 | 204 | 50 | 204 |
| **Mean** | 3.58 | 5.26 | 3.18 | 5.29 | 3.71 | 6.78 | 3.66 | 4.69 | 3.51 | 4.50 | 3.40 | 4.36 |
| **Standard Deviation** | 3.99 | 8.41 | 3.31 | 8.50 | 3.81 | 10.84 | 4.28 | 7.65 | 4.05 | 7.30 | 3.85 | 7.02 |
| **Q1- 25%** | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| **Median (Q2)-50%** | 2 | 2 | 2 | 2 | 2 | 3 | 2 | 2 | 2 | 2 | 2 | 2 |
| **Q3- 75%** | 4 | 6 | 4 | 6 | 5 | 7 | 4 | 5 | 4 | 5 | 4 | 5 |
| **90%** | 9 | 13 | 7 | 13 | 9 | 17 | 9 | 11 | 9 | 10 | 8 | 10 |
| **95%** | 11 | 20 | 10 | 20 | 12 | 26 | 12 | 18 | 12 | 17 | 11 | 16 |
| **99%** | 21 | 43 | 17 | 44 | 19 | 52 | 21 | 40 | 21 | 38 | 20 | 36 |
| **100% Max** | 49 | 208 | 46 | 370 | 46 | 370 | 72 | 199 | 60 | 204 | 50 | 204 |

## Table B-2
### Number of Loans per Sequence (Loan 3)

| | Restricted States | | No Restriction | | January 2010 Cohort | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Full Period | | Full Period | | No Restriction | | Not Seen in Previous 6 Months | | Not Seen in Previous 3 Months | | Not Seen in Previous 1 Month | |
| | 355,229 | 800,989 | 2,610,227 | 12,268,692 | 1,414,245 | 3,867,224 | 184,250 | 331,195 | 238,126 | 443,953 | 411,552 | 811,158 |
| | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years |
| **Statistic** | Subsample 1 | Subsample 1 | Full Sample | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 4 | Cohort 5 | Cohort 5 | Cohort 6 ≈ CFPB New Borrower | Cohort 6 ≈ CFPB New Borrower |
| **Minimum** | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| **Maximum** | 47 | 503 | 113 | 489 | 113 | 478 | 82 | 322 | 70 | 204 | 86 | 307 |
| **Mean** | 5.27 | 6.41 | 3.28 | 5.63 | 3.86 | 7.25 | 3.77 | 4.94 | 3.62 | 4.74 | 3.50 | 4.56 |
| **Standard Deviation** | 5.65 | 12.54 | 3.59 | 9.46 | 4.19 | 12.19 | 4.55 | 8.43 | 4.32 | 8.04 | 4.09 | 7.64 |
| **Q1- 25%** | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| **Median-50%** | 3 | 2 | 2 | 2 | 2 | 3 | 2 | 2 | 2 | 2 | 2 | 2 |
| **Q3- 75%** | 7 | 6 | 4 | 6 | 5 | 8 | 4 | 5 | 4 | 5 | 4 | 5 |
| **90%** | 13 | 15 | 7 | 13 | 9 | 18 | 10 | 11 | 9 | 11 | 9 | 10 |
| **95%** | 20 | 25 | 10 | 21 | 12 | 28 | 13 | 19 | 12 | 18 | 11 | 17 |
| **99%** | 23 | 60 | 18 | 47 | 20 | 59 | 22 | 43 | 22 | 41 | 21 | 39 |
| **100% Max** | 47 | 503 | 113 | 489 | 113 | 478 | 82 | 322 | 70 | 310 | 86 | 307 |

# Appendix C: Maximum Sequence Duration per Borrower

## Table C-1
### Maximum Sequence Duration (Loan 2)

| | Restricted States | | No Restriction | | January 2010 Cohort | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Full Period | | Full Period | | No Restriction | | Not Seen in Previous 6 Months | | Not Seen in Previous 3 Months | | Not Seen in Previous 1 Month | |
| | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years |
| **Statistic** | Subsample 1 | Subsample 1 | Full Sample | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 4 | Cohort 5 | Cohort 5 | Cohort 6 ≈ CFPB New Borrower | Cohort 6 ≈ CFPB New Borrower |
| **Minimum** | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| **Maximum** | 49 | 208 | 65 | 370 | 46 | 370 | 72 | 199 | 60 | 204 | 50 | 204 |
| **Mean** | 4.18 | 9.06 | 8.15 | 9.49 | 5.53 | 15.62 | 5.30 | 8.99 | 5.13 | 8.86 | 5.15 | 9.25 |
| **Standard Deviation** | 3.88 | 12.5 | 6.56 | 12.81 | 4.69 | 17.68 | 5.28 | 12.26 | 5.01 | 11.96 | 4.81 | 11.97 |
| **Q1- 25%** | 1 | 2 | 3 | 2 | 3 | 4 | 1 | 2 | 1 | 2 | 2 | 2 |
| **Median (Q2)-50%** | 3 | 4 | 6 | 5 | 4 | 9 | 3 | 5 | 3 | 5 | 4 | 5 |
| **Q3- 75%** | 6 | 11 | 11 | 12 | 8 | 21 | 7 | 11 | 7 | 11 | 7 | 11 |
| **90%** | 9 | 23 | 18 | 24 | 12 | 39 | 12 | 22 | 11 | 22 | 11 | 23 |
| **95%** | 12 | 34 | 23 | 35 | 15 | 49 | 17 | 33 | 16 | 32 | 15 | 33 |
| **99%** | 19 | 59 | 24 | 61 | 21 | 87 | 23 | 57 | 23 | 56 | 22 | 56 |
| **100% Max** | 49 | 208 | 46 | 370 | 46 | 370 | 72 | 199 | 60 | 204 | 50 | 204 |

## Table C-2
### Maximum Sequence Duration (Loan 3)

| | Restricted States | | No Restriction | | January 2010 Cohort | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Full Period | | Full Period | | No Restriction | | Not Seen in Previous 6 Months | | Not Seen in Previous 3 Months | | Not Seen in Previous 1 Month | |
| | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years | 11 Months | 4 Years |
| **Statistic** | Subsample 1 | Subsample 1 | Full Sample | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 4 | Cohort 5 | Cohort 5 | Cohort 6 ≈ CFPB New Borrower | Cohort 6 ≈ CFPB New Borrower |
| **Minimum** | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| **Maximum** | 47 | 503 | 113 | 489 | 113 | 478 | 82 | 322 | 70 | 204 | 86 | 307 |
| **Mean** | 8.63 | 11.21 | 8.33 | 10.13 | 5.56 | 16.86 | 5.46 | 9.60 | 5.31 | 9.46 | 5.34 | 9.81 |
| **Standard Deviation** | 6.57 | 18.46 | 7.02 | 14.3 | 4.97 | 20.08 | 5.60 | 13.59 | 5.36 | 13.25 | 5.14 | 13.11 |
| **Q1- 25%** | 3 | 2 | 3 | 2 | 2 | 4 | 1 | 2 | 1 | 2 | 2 | 2 |
| **Median (Q2)-50%** | 7 | 5 | 6 | 5 | 4 | 10 | 3 | 5 | 3 | 5 | 4 | 5 |
| **Q3- 75%** | 12 | 13 | 11 | 12 | 7 | 22 | 8 | 12 | 7 | 11 | 7 | 12 |
| **90%** | 20 | 28 | 19 | 25 | 12 | 42 | 12 | 24 | 12 | 23 | 12 | 24 |
| **95%** | 22 | 43 | 22 | 38 | 16 | 54 | 17 | 36 | 17 | 35 | 16 | 35 |
| **99%** | 23 | 91 | 30 | 69 | 22 | 97 | 24 | 64 | 23 | 63 | 23 | 62 |
| **100% Max** | 47 | 503 | 113 | 489 | 113 | 478 | 82 | 322 | 70 | 310 | 86 | 307 |

# Appendix D: Distribution of Maximum Sequence Duration per Borrower (Four Years)

## Table D-1

### Maximum Sequence Duration per Borrower (Loan 2)

| Number | Subsample 1 | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 5 | Cohort 6 ≈ CFPB "New Borrower" |
|---|---|---|---|---|---|---|
| Equal to or Less Than 3 Loans | 44.47 | 41.47 | 22.65 | 42.57 | 41.86 | 38.77 |
| Equal to or Less Than 4 Loans | 51.44 | 48.64 | 28.77 | 49.8 | 49.48 | 46.73 |
| Equal to or Less Than 5 Loans | 56.84 | 54.66 | 34.24 | 56.11 | 56.08 | 53.59 |
| Equal to or Less Than 6 Loans | 61.22 | 59.81 | 39.42 | 61.34 | 61.53 | 59.4 |
| Equal to or Less Than 7 Loans | 64.88 | 63.6 | 43.44 | 65.29 | 65.54 | 63.57 |
| Greater Than 7 Loans | 35.12 | 37.4 | 56.56 | 34.71 | 34.46 | 36.43 |

## Table D-2

### Maximum Sequence Duration per Borrrower (Loan 3)

| Number | Subsample 1 | Full Sample | Cohort 3 ≈ CFPB White Paper | Cohort 4 | Cohort 5 | Cohort 6 ≈ CFPB "New Borrower" |
|---|---|---|---|---|---|---|
| Equal to or Less Than 3 Loans | 42.15 | 40.11 | 21.31 | 41.27 | 40.53 | 37.41 |
| Equal to or Less Than 4 Loans | 48.98 | 47.21 | 27.22 | 48.46 | 48.06 | 45.2 |
| Equal to or Less Than 5 Loans | 54.3 | 53.19 | 32.55 | 54.69 | 54.53 | 51.96 |
| Equal to or Less Than 6 Loans | 58.59 | 58.33 | 37.63 | 59.82 | 59.91 | 57.71 |
| Equal to or Less Than 7 Loans | 62.12 | 62.16 | 41.61 | 63.78 | 63.9 | 61.92 |
| Greater Than 7 Loans | 37.88 | 37.84 | 58.39 | 36.22 | 36.1 | 38.08 |

nonPRIME101

nonPrime101.com provides research studies and articles about non-prime consumer behavior to help the public and researchers better understand them. The rate of non-prime consumers, which include thin-file, no-file and prior prime consumers, continues to rapidly grow and nonPrime101.com provides unbiased and empirical studies that show the credit usage behaviors, activities and needs of non-prime consumers as a whole.

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# Searching for Harm in Storefront Payday Lending

## A Critical Analysis of the CFPB's "Debt Trap" Data