ESSAYS ON RETAIL LENDING

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ESSAYS ON RETAIL LENDING

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Essays on Retail Lending

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Abstract

My dissertation consists of three essays on retail lending. In the first essay, using data from Lending Club and Prosper, the two largest peer-to-peer lenders in the U.S., I provide evidence of adverse selection in the online personal lending market. Borrowers who were rejected by a competitor are twice as likely to default as borrowers who were not rejected, conditional on receiving the same contract. Borrowers are also more likely to default when offered higher interest rates or smaller loan amounts by a competitor. Surprisingly, the loan amount effect is larger than the interest rate effect. Loan amount is also more closely related to lender choice than is interest rate.

In the second essay, with Dimuthu Ratnadiwakara and Kevin Roshak, we find that modest differences in the interest rate at loan origination can have long-lasting effects on mortgagors. We use monthly fluctuations in the national mortgage rate at loan origination to study small changes in interest rates across home purchases made in the same year, in the same area, and which eventually reach similar levels of negative equity. A 50bp higher national rate at origination corresponds to an extra $550 in payments per year and, during the bust, an increase in defaults of 68–88 bp within 12 months of reaching negative equity. The effect is large relative average default rates of 3.78% (5.39%) for homes with 10% (30%) negative equity. Consistent with liquidity constraints, the magnitude of the effect is relatively constant across different levels of negative equity. The national mortgage rate is not correlated with worse borrower credit quality. During the boom, smaller mortgage payments result in increased consumption of non-durables and services from

2001–2007, while total expenditure is unchanged. If intermediaries resist large concessions to borrowers, small concessions may be more effective.

In the third essay, I investigate loan stacking, a phenomenon wherein borrowers receive multiple loans from different lenders at approximately the same time, often without the lenders being aware of each other's loans. I evaluate the performance of stacked loans, expecting that stacked loans should perform worse. I also evaluate whether the poor performance of stacked loans is driven by fraud, or simply by borrowers taking on more debt than they can afford.

Contents

List of Figures

List of Tables

1 Price and Quantity Adverse Selection in Online Lending

1.1 Introduction

Adverse selection, especially adverse selection occurring due to information asymmetries among lenders (Sharpe, 1990), is an important phenomenon in lending markets. This form of adverse selection, which is sometimes termed the "winner's curse",[1] is the focus of a large theoretical literature. (Broecker, 1990; Sharpe, 1990; Von Thadden, 2004)

Obtaining direct empirical evidence of adverse selection in lending markets is difficult, however. Ideally, such analysis should use data on competing bids (in the example of lending, this means contract offers or rejections). Such analysis has been performed for oil lease auctions (Capen et al., 1971), eBay auctions (Bajari and Hortacsu, 2003), and other kinds of auctions. For lending markets, however, no such analysis exists as losing bids (contract terms of loan offers which are not accepted and rejections) are not generally observed.

We make use of a novel data set, which includes multiple bids for a specific loan product for a specific borrower, as well as the subsequent performance of that borrower's loan. At the time of issue, each lender only observes its own rejection or offer, not the rejection or offer of competitors. We identify borrowers who were rejected at one lender but given a loan by

[1] We should distinguish this use of the term "winner's curse" from the usage common in the behavioral economics literature. We use the word winner's curse to refer to adverse selection, whereas in the behavioral literature the term is often used to indicate irrationally bidding as if there were no adverse selection.

another. We also identify borrowers who had a pending loan application at one lender, with an offered interest rate and loan amount, but had that application terminated, and eventually received a loan from another lender. This allows us to consider the informational content of these offers and rejections; in other words, to investigate the relationship between competitors' offers/rejections and default.

We shed light on multiple dimensions along which adverse selection can occur. Specifically, we consider rejection decisions, interest rates, and loan amounts as factors which lead to adverse selection. While other papers have empirically considered adverse selection as relates to differing interest rate offers between lenders (e.g., Berger and Udell (1995)), we are the first to highlight empirically the importance of rejections and competing loan amount offers in producing adverse selection. The rejection decision is perhaps the most obvious case. Borrowers rejected by a competitor are more likely to be riskier than expected and also more likely to appear in a lender's sample. Interest rate is a natural extension of the rejection case—borrowers offered a high interest rate by a competitor are also more likely to appear in the sample and to be riskier than expected. Less obviously, adverse selection can also occur due to loan amount offers, with riskier borrowers being more likely to take the largest loan offered to them.

We investigate adverse selection in the context of an online personal lending market consisting of the two major peer-to-peer lenders in the United States, Lending Club and Prosper. We compare the performance of borrowers who were rejected by Lending Club, but accepted by Prosper to borrowers who were accepted by Prosper but not rejected by Lending Club, finding that borrowers rejected by Lending Club are twice as likely to default.

We next investigate the selection involved in adverse selection: what causes borrowers

to accept contracts? We consider borrowers who received offers from Prosper but eventually decided to get their loan from Lending Club. We find that 73% of borrowers received a lower interest rate by choosing Lending Club instead of Prosper. 98% of borrowers, however, receive a larger loan amount, suggesting that loan amount may actually be the more important factor in borrowers' contract choice.

Information asymmetry between lenders only gives rise to adverse selection when multiple lenders are involved. We compare cases where Lending Club borrowers are known to have applied to and been accepted by Prosper to cases where they did not. We find that borrowers with Prosper offers are about 40% more likely to default.

Borrowers select into contracts that are lower interest rate and/or higher loan amount. We ask whether this selection is a form of adverse selection. Are relatively higher (lower) interest rate (loan amount) offers from a competitor associated with increased default risk? We find that default rates are higher both when the competitor's interest rate offer is higher than expected and when the competitor's loan amount offer is lower than expected. The loan amount effect is at least as strong as the interest rate effect.

Ours is not the first study to consider adverse selection stemming from asymmetries in lenders' information empirically. Shaffer (1998) uses bank-level data and finds evidence consistent with the winner's curse, namely that newer banks and banks in more competitive markets experience more charge-offs. In the same vein as Shaffer (1998), a number of studies consider situations in which one lender has superior information, because they are a repeat lender (Berger and Udell, 1995), or are located physically closer to the borrower (Agarwal and Hauswald, 2010). These studies consider the effects of superior information on pricing and

loan performance.

However, our study is the first to consider adverse selection in lending using data on competing bids. The need for such a study is evident from the literature. For example, Agarwal and Hauswald (2010) interpret the relationship between borrower-lender distance and price and loan performance as the consequence of informational frictions, whereas Degryse and Ongena (2005) interpret similar results as the consequence of transportation costs. Since we actually obtain the interest rate offers and internal credit evaluations of two competing lenders, however, our identification strategy is much cleaner than those in the existing literature.

There are two important differences between the market we study, and the corporate lending markets that have been the focus of the existing literature. Firstly, we study consumer lending. Secondly, and more importantly, the lenders in our sample are online, "arms-length" lenders, meaning that the information asymetries leading to adverse selection in these data are not "soft" information asymmetries arising from lenders knowing different facts about loan applicants, but are rather asymmetries in information processing techniques. These can stem from differences in algorithms, historical training data sets, methods of turning credit reports into variables, or choice of credit bureau. The informational competition between lenders is one between machines rather than between tradiational loan officers. This type of competition has been considered in the theoretical literature (Hauswald and Marquez, 2006; Hauswald and Marquez, 2003), but we are not aware of any empirical investigations into the effects of imperfect information in competitive lending markets where interest rates are set by algorithm. Credit evaluation by algorithm alone is becoming increasingly common (Berger,

2003). Credit card and personal lending markets have already made the transition to algorithmic rate setting. Mortgage markets are rapidly transitioning to algorithmic rate setting, and small-business lenders using algorithmic rate setting are beginning to appear.

The principal contribution of our research is that it shows *how* adverse selection occurs. We consider which contracts borrowers accept, which is the basis for potential adverse selection. We consider acceptance vs rejection, as well as both interest rate and loan amount offers, as channels through which adverse selection can occur. The importance of amount in creating adverse selection has received little attention in the existing literature, and none of the three channels has been empirically shown to be an important component of the mechanism through which information asymmetries give rise to adverse selection. We show that all three channels (acceptance, interest rate, and loan amount) are important in creating adversely selected loan portfolios.

1.2 Theory and Predictions

The setting at hand is one in which two banks (and potentially more) are competing to provide loans to a pool of applicants. Since our data are limited to matched observations from only two competing lenders, in the following discussion and hypothesis development we refer to this market as a duopoly with Lenders A and B, for simplicity. A key observation is that consumers are free to apply to both lenders before deciding what loan terms to accept. This introduces a standard adverse selection problem (Rajan, 1992; Sharpe, 1990; Stiglitz and Weiss, 1981). For example, when a loan offered by Lender A is accepted by an applicant, Lender A does not know whether the applicant solicited a loan offer from the competing Lender B and if so what

the terms of this competing loan are. Therefore, from the perspective of the lender, an offer that is accepted was probably dominating from the perspective of the applicant relative to her competing offer—if such exists. To set ideas, one can also think of this as the winnerâĂŹs curse. That is, the lender that wins the business of the applicant worries whether the loan terms were too favorable.

It is important to explain why one loan applicant might be granted different loan terms from the two competing lenders. This follows since the market examined is far from "perfect." The online lending market is a new technology and little is known regarding the performance of these loans. As time passes, more and more information is gathered, new competitors enter the market, and better models are developed. Given this high level of uncertainty, even if a particular applicant provides quite similar (or even identical) information to the two lenders, they might provide loan offers that are quite different. In addition, the information requested by the two competing lenders is not identical. Of course, if the market really is close to being competitive and efficient, none of the above will be true. It is an empirical question whether, or to what extent, adverse selection in the form described above is economically relevant to this market.

We start with the most obvious form of adverse selection. Lenders' borrower pools should be polluted with borrowers rejected by competitors. It is obvious that borrowers rejected by Lender A are more likely to accept offers from Lender B (relative to Lender A). But is this associated with higher default rates? Our first hypothesis is that it is:

Hypothesis 1 (H1): *Loans issued to applicants who were rejected by a competitor underperform, all else equal.*

In cases where borrowers have two offers (i.e., are accepted by both lenders), it is less obvious which contract they will select. Several studies in finance have emphasized the importance of interest rates in managing adverse selection. For example, in the seminal work of Stiglitz and Weiss (1981), individual borrowers that differ in their unobservable risk profiles self select based on the interest rate offered by the lender. Consequently, an increase in the interest rate can lead to a dilution of the average quality of loans accepted—fewer high quality loans are issued since only individuals with more risky profiles tend to remain.

A similar form of adverse selection might be taking place in the industry at hand. But here individuals might be choosing between loan offers based on interest rate and/or loan amount. While loan amount has received less attention in the theoretical literature, borrowers choosing lenders based on loan amount would have very similar implications. In most forms of consumer lending, larger loan sizes are associated with default, possibly because of adverse selection (e.g., Adams et al., 2009). Since we observe in our sample individuals with two confirmed loan offers we can empirically evaluate whether individuals choose the loan offer with the lower interest rate and/or the highest loan amount. If individuals choose the lower (higher) interest rate (loan amount) offers by the competitor, this will serve as support for the presence of adverse selection. Moreover, the data allow us to identify the mechanism by which adverse selection materialized—i.e., whether through interest rate or loan size.

Hypothesis 2 (H2): *Individuals self select into platforms, i.e., are more likely to choose the platform that offers them a lower interest rate or higher loan amount, all else equal.*

If, as suggested by H2, borrowers select lender based on interest rate and loan amount, not simply based on whether or not a lender accepts them, adverse selection should occur

not merely because a lender's sample is polluted with borrowers rejected by a competitor, but because a lender's sample is polluted with borrowers the competitor offered less attractive contracts to. We start by identifying pairs of competing offers. Since not all individuals might approach several lenders before accepting a loan offer it is important for us to identify those that we suspect have competing offers. Thus, one would expect that since lenders do not have information on competing offers, those loans that are accepted by individuals with a competing offer will underperform the appropriate benchmark. In other words, we can test whether a competing loan with less favorable terms is marginally informative about the creditworthiness of the applicant.

Hypothesis 3 (H3): *Loans issued to applicants who had loan offers with a competitor underperform, all else equal.*

Finally, in order to further tie loan performance to the presence of adverse selection we examine whether the extent of private information of the borrower significantly impacts her loan performance. That is, by examining the loan terms of two competing loan offers one can measure the "distance" so to speak between these loans. In particular, when the two loan offers are very similar in terms of their overall attractiveness, e.g., loan size and interest rate, then we would imagine that the two competing lenders might have very similar beliefs regarding the applicantâĂŹs quality or creditworthiness. In this case, the availability of a competing loan does not provide much private information to the applicant. And whether she chooses one platform over the other will not make a big difference to her. For the same reason, as this is a zero sum game, the extent of adverse selection is quite muted when the loan terms are similar. Actually, one might argue that when the loan terms are similar then

this adds confidence to the model of the platform and the loan should perform even better. We can, however, observe loans that were canceled in order to switch to a competing platform. In this case, the loan terms are indeed different. This allows us to test whether the extent of "distance" between the two loan terms, in terms of loan amount and interest rate, as a measure of the degree of adverse selection, can explain loan performance. Theory would indeed predict a positive relation.

Hypothesis 4 (H4): *For Lending Club borrowers with competing offers from Prosper, the higher (lower) the difference between interest rate (loan amount) offers from Prosper and Lending Club, the higher the probability of default.*

To summarize, we expect adverse selection to occur because there is a possibility borrowers were rejected by the other lender. Thus, we expect borrowers rejected by the other lender perform worse (H1). However, adverse selection may also occur simply because there is a possibility the competitor offered a less attractive contract. We first determine on what basis borrowers select contracts (H2). We then determine whether borrowers who had (unobservable) applications with a competitor underperform (H3). We then tie the contract selection and resulting underperformance together by considering whether the distance between contract offers is related to loan performance. Do borrowers with vastly inferior competing offers perform much worse and borrowers with only slighly worse competing offers perform less poorly? (H4)

1.3 Background and Data

1.3.1 Background

We use data from the two main P2P lenders in the U.S., Lending Club and Prosper. During the primary sample period (2009-2013),[2] these two lenders dominated the online personal lending market, and to a lesser extent, the entire personal lending market.[3] P2P lenders during this period (and today) functioned as arm's-length lenders, setting interst rates via algorithm without the intervention of a credit officer.

Prior to our sample period, P2P lenders functioned much differently than they did during our sample period. The lending was truly peer-to-peer. Before being shut down by the SEC in 2008, Prosper, which was the first P2P lender in the U.S., was in many ways a social website. Borrowers would post profiles, complete with pictures and biographies. Lenders would form groups and make lending recommendations to each other. They would ask questions of prospective borrowers and make lending decisions in part based on the responses. Interest rates were set by Dutch auction. Banks, registered securities, and underwriting models were not a part of the process. Lending Club initially functioned similarly to Prosper. It was a Facebook application, rather than a standalone website, but was social, just like Prosper. Like Prosper, it allowed lenders to set rates by Dutch auction, though it did recommend an interest rate.

Lending Club quickly transitioned into being a website rather than a Facebook application and began to set interest rates via its own underwriting model rather than facilitating an auc-

[2] We also use data from 2014–2017 for a limited number of tests. During this time, the Lending Club and Prosper face much more external competition.

[3] Traditional banks have always been hesitant to make unsecured personal loans, and were especially hesitant in the post-crisis period.

tion. Lending Club registered with the SEC in mid-2008. In November 2008, the SEC issued a cease and desist order to Prosper, who then changed their business model to mimic Lending Clubs and reopened to sell SEC-registered securities in July 2009. For our analysis, we will consider only loans issued in July 2009 and after. We wish to study competition and informational frictions between competing lenders, which occurs only from July 2009 and on. Before July 2009, Lending Club competed with individual investors using the Prosper platform, not with Prosper directly.

1.3.2 The Lending Process

On both Lending Club and Prosper, loans are now issued via the following process. A prospective borrower applies, reporting information about himself, his finances, and his need for a loan. The lender checks the borrower's score and report, then assigns him a risk subgrade and a corresponding interest rate. If the borrower accepts the interest rate, the loan is listed on the lender's website. Prospective investors can browse the loans listed on the website and agree to fund a portion of the loan. The loan is issued through a bank affiliated with the lender, so these loans are legally the same as traditional unsecured loans. However, the lender does not make loans until borrowers have agreed to fund the entirety of the loan. All issued loans are thus immediately securitized. The lender may choose to verify the borrower's income while the loan is in funding but does not delay the listing of the loan to verify income. If the borrower's income cannot be verified, the loan is removed from the site (canceled). If the loan becomes fully funded before the borrower's income can be verified, the loan is issued without verification. Some loans expire without being funded. This is relatively rare on Prosper. Lending Club claims this is essentially unheard of, but data on expired listings are not

available from Lending Club. The borrower may also withdraw his listing from the site at any time until the loan is funded.

1.3.3 P2P Lending Literature

The existing P2P literature primarily makes use of data from before SEC regulation, primarily focusing on social aspects such as pictures (Duarte et al., 2012), text analysis (Michels, 2012; Herzenstein et al., 2011), and group membership (Lin et al., 2013; Everett, 2010). These social factors are not involved in the P2P data we use, which comes from 2009–2017, by which time the SEC had shut down the more social kind of P2P lending.

P2P lending after SEC regulation has been studied by a very limited number of papers, most of which focus on forecasting. However, Balyuk (2016) finds that P2P lending mitigates informational frictions in personal credit markets. This paper is closely related to ours in that it studies informational frictions and the P2P lending market. Balyuk (2016) shows that P2P lending reduces informational frictions, whereas we show that large informational frictions still remain within the P2P lending market.

1.3.4 Data Sources

We use the following data sources:

1. Data on the borrower and loan for every loan issued via Prosper. These include the credit score, many variables extracted from the credit history, borrower-reported variables, loan contract variables, and repayment information.[4]

2. Data on the applicant and loan for every listing on Prosper that is not eventually issued.

[4]These data are publicly available on Prosper.com. However, our application requires a legacy data set.

These include canceled, withdrawn, and expired listings. The data available is identical to that available for issued loans, except of course, there is no loan performance data.[5]

3. Data on the borrower and loan for every loan issued via Lending Club. The specific data fields are somewhat different, but otherwise these data are very similar to those available for Prosper loans.[6]

4. Data on every applicant rejected by Lending Club. For rejected applicants, we have the following 8 data fields: amount requested, listing creation date, loan title, credit score, debt-to-income, city, state, and employment length.[7]

We report summary statistics for all Lending Club loans in Table 1.1 and for all Prosper loans in Table 1.2. A key portion of our analysis, that on rejected borrowers, uses data on only loans issued in 2013 or before. We report summary statistics for these loans in Table 1.3. A typical P2P borrower borrows $\$10,000$–$15,000$, and, relative to the U.S. population as a whole, has a high income (mean of $\$75,000$ annually for Prosper and $\$77,000$ for Lending Club) and a slightly below average credit score. Lending Club and Prosper borrowers have similar credit histories and financial situations in most ways, the main exception being that Lending Club borrowers have on average 0.51 fewer credit inquiries in the last 6 months. Although Lending Club and Prosper borrowers appear very similar based on summary statistics, Lending Club offers their borrowers much more favorable terms, on average lending $\$15,000$ at 13% interest as opposed to $\$12,000$ at 16% for Prosper borrowers. We show a histogram of interest rates at both lenders in Figure 1. The modal interest rate at both lenders is 13%, but Prosper has

[5]These data are publicly available on Prosper.com
[6]These data are publicly available on LendingClub.com. However, our application requires a legacy data set.
[7]These data are publicly available on LendingClub.com

a much fatter right tail with sizable issuance even at 35%. Surprisingly, Prosper borrowers actually default 34% less frequently than Lending Club borrowers, despite paying higher interest rates. Prosper has historically had much more trouble attracting investors than Lending Club, with 19% of Prosper listings expiring without being funded.[8] The exact figure for Lending Club is not disclosed, but is very close to 0. Prosper's difficulty in attracting investor's and desire to build repuatational capital to attract investors in the future likely explains the outperformance of Prosper loans.

Table 1.1: Lending Club Loans

Statistic	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)	N
Income	76,909.080	86,918.010	46,000.000	65,000.000	91,000.000	1,415,299
Credit History Length	199.830	91.876	137	181	246	1,321,818
Debt-to-Income	18.369	8.546	12.050	17.840	24.240	1,415,134
Employment Length	34.972	30.205	12	12	60	1,335,710
Home Ownership	0.603	0.489	0	1	1	1,414,576
Incomce Verification	0.696	0.460	0	1	1	1,415,303
Inquiries	0.641	0.949	0	0	1	1,415,273
Interest Rate	0.132	0.046	0.098	0.128	0.159	1,415,303
Loan Amount	14,756.880	8,675.654	8,000	12,800	20,000	1,415,303
Term	42.796	10.813	36	36	60	1,415,303
Percent Funded	1.000	0.013	1.000	1.000	1.000	1,415,303
Loan Age	25.666	17.235	13	22	35	1,415,303
Default	0.091	0.287	0	0	0	1,415,303
Open Credit Lines	11.666	5.488	8	11	14	1,415,274

Notes to Table: Summary statistics for all loans issued by Lending Club are reported above. Credit history length is in months. Debt-to-Income is the ratio of monthly debt payments (including the P2P loan to monthly income). Employment length is in months, but is censored at 10 years. Credit inquiries are hard inquiries in last 6 months. Loan age is months since issue. Term is an indicator variable taking a value of 1 if the loan is a 60-month maturity loan and 0 if a 36-month maturity loan.

[8] The percentage of listings which eventually become loans is 52%. The other 29% are canceled or withdrawn.

Table 1.2: Prosper Loans

Statistic	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)	N
Income	74,532.320	1,019,969.000	41,600.000	60,000.000	85,000.000	384,965
Credit History Length	217.616	102.248	148	205	275	367,452
Debt-to-Income	25.850	12.475	17.000	24.000	33.000	372,324
Employment Length	67.707	39.977	28	76	108	376,879
FICO	5.685	1.977	4	5	7	313,336
Home Ownership	0.436	0.496	0	0	1	384,965
Incomce Verification	0.968	0.176	1	1	1	384,965
Inquiries	1.150	1.728	0	1	2	367,465
Interest Rate	0.156	0.063	0.108	0.146	0.193	384,967
Loan Amount	11,828.230	8,150.527	5,300.000	10,000.000	16,000.000	384,967
Term	43.452	11.105	36	36	60	384,967
Percent Funded	0.999	0.014	1.000	1.000	1.000	384,965
Loan Age	26.290	29.856	8	18	32	384,967
Default	0.060	0.237	0	0	0	384,967
Open Credit Lines	10.147	5.032	7	9	13	360,889

Notes to Table: Summary statistics for all loans issued by Prosper are reported above. Credit history length is in months. Debt-to-Incomeis the ratio of monthly debt payments (including the P2P loan to monthly income). Employment length is in months, but is censored at 10 years. Credit inquiries are hard inquiries in last 6 months. Loan age is months since issue. Termis an indicator variable taking a value of 1 if the loan is a 60-month maturity loan and 0 if a 36-month maturity loan.

Table 1.3: Summary Statistics

	Prosper	Lending Club	LC Reject
Income	71227	72218	65764
Inquiries	1.33543	0.816393	0.9171271
FICO	696.5957	700.7903	676.5
Loan Amount	9678.785	13657.75	8738.807
Credit History Length	17.69986	14.52355	19.4054
Interest Rate	0.1800368	0.1394538	0.1954779

Notes to Table: Summary statistics for all loans issued by Prosper (column 1) and Lending Club (column 2) are reported above. Summary statistics for all Prosper loans made to borrowers rejected by Lending Club are reported in column 3.

We use the Lending Club rejection data only for determining which loans in the Prosper data set were made to borrowers who were rejected by Lending Club. We describe the procedure for matching individuals between the Lending club rejection data and Prosper loan data in the next section. We use data on Prosper listings that did not eventually become loans to determine which Lending Club borrowers received loan offers from Prosper and at what interest rate they received these offers. We describe the procedure for matching borrowers between the Prosper listing and Lending Club loan data sets in Section 2.6.

1.3.5 Identifying Rejected Borrowers

Identifying the same applicant at multiple lenders is very difficult.[9] For privacy reasons, Lending Club and Prosper, like all other lenders, do not publish personally identifiable information on their borrowers or applicants. However, until 2013, Lending Club and Prosper both published the (mailing address) cities of their borrowers. Both lenders have since stopped providing such specific data on borrower location, but it leaves us with ample data that are surprisingly granular.

We identify a Prosper borrower as having been rejected by Lending Club if there is an applicant in the Lending Club rejection dataset that:

1. Applied on the same date as the Prosper borrower
2. Lives in the same city as the Prosper borrower
3. Has the same employment length as the Prosper borrower

This measure of rejection is very noisy if we include borrowers from larger cities. However, by

[9] We are aware of one other paper which identifies rejected borrowers who were subsequently accepted by another lender: Agarwal et al. (2016). Their matching technique is very similar to our ours.

restricting the sample to borrowers from small cities, we are able to determine which borrowers were rejected with a rate of false positives that is very close to 0. We consider a city to be small if there are fewer than 10 applications from that city in the rejection dataset. For robustness, we also use cutoffs other than 10. We also construct alternate samples using restrictions based on FICO score and debt to income, in addition to the three listed above.

At first glance, this matching technique may seem inaccurate. We can determine the accuracy of the match (that is, the rate of false positives) by using a date offset. If a rejection from the same city with the same employment length appears in the Lending Club data set after a borrower has already received a loan from Prosper, he is not the same borrower.[10] The matching technique described above yields 192 matches, whereas an identical technique with an offset of 30 days yields on average 4 matches. This implies that the matches are 98% accurate. Measurement error exists, but is small enough to be ignored when considering the magnitude of the coefficient of interest.

The restriction that the Lending Club and Prosper applicants apply on the same day may seem stringent. But, in reality, very few applicants apply to Lending Club on one day, get rejected, and then go to Prosper the next. Using the above matching technique with a one-day offset (rejection before acceptance), we observe 15 matches. This implies a much lower matching accuracy of 73%. There is no need to introduce additional measurement error into the sample, so we use the same day restriction throughout this paper.

[10] He could actually be the same individual, but even if he is, he is now shopping for an additional loan, not the same loan.

1.3.6 Identifying Borrowers with Competing Offers

To analyze the relationship between Prosper listing status and Lending Club loan performance, we must first match borrowers between the Prosper listing and Lending Club loan data sets. This process is similar to, but slightly different from, that used in the previous section to identify borrower rejected by Lending Club, but accepted by Prosper. The matching procedure needs to be different because there is no reason to expect the Prosper and Lending Club applications to take place on the same day. In fact, the Prosper and Lending Club applications will likely not take place on the same day. If the borrower had applied simultaneously to Lending Club and Prosper, and the Lending Club terms were more favorable, the borrower would have accepted the Lending Club offer and never have had a Prosper listing. Thus, the borrower must first apply to Prosper and initiate a listing, and *then* apply to Lending Club.

Although we cannot use the same day restriction like we do for Lending Club rejects, the Prosper listing data set is much richer than the Lending Club rejection data set in terms of credit and employment variables, which allows for an accurate match based on month of application and credit variables, rather than date of application and employment length alone. Because we do not need the exact date of application for the Lending Club loans, we can use a more updated data set. More recent Lending Club data sets include three-digit zip code and month of application. We match this data set to the Prosper listings data set using the following procedure: a borrower in the Lending Club data set is considered to have had a withdrawn (canceled, expired) Prosper listing if the Prosper listing data includes a listing with the following characteristics:

1. The same month of application as the Lending club loan

2. The same three-digit zip code

3. The same annual income (rounded down to nearest $100)

4. The same number of years (not exact months) of credit history

5. The same number of open accounts

6. The same homeownership status

By design, these matching criteria are highly restrictive. This means that many loans to borrowers that actually had Prosper withdrawn/canceled/expired listings are not classified as such. Lending Club and Prosper pull credit information from different bureaus, so it is very likely that data fields on the same individual will be observed as different by the two lenders. However, given that having a Prosper withdrawn/canceled/expired listing is a relatively rare event, the high rate of our algorithm's failure to identify matching Prosper listings will not cause significant attenuation bias.[11] However, misclassifying borrowers without withdrawn/canceled/expired listings as having them can result in significant attenuation bias. This is why we design our match to be highly restrictive.

We cannot estimate the rate of false negatives. However, the rate of false positives can be estimated by constructing a placebo sample. To construct the placebo sample we add (subtract) 365 days to the Lending Club application date for loans accepted before (after) January 2014.[12] We then perform our matching algorithm using these modified data. The algorithm

[11] According to Aigner (1973), $\hat{\beta} = \hat{\beta}_{OLS}(1 - \eta - \nu)$ is an unbiased consistent estimator of β, where β is the true effect of a binary variable (in our case withdrawal, cancellation, or expiration) on an outcome variable (in our case, default), $\hat{\beta}_{OLS}$ is the OLS estimator of β, η is the rate of false negatives (fraction borrowers marked as not having withdrawn/canceled/expired listings which in reality do), and ν is the rate of false positives (borrowers marked as having withdrawn/canceled/expired listings which in reality do not). In the case where the treatment is a rare event (which withdrawal, cancellation, and expiration all are), η will usually be low. It is much more important to construct a matching algorithm that results in a high ν.

[12] We must add to the earlier dates and subtract from the later ones to ensure that all modified dates correspond to dates in the Prosper listings data

yields 203 placebo matches, as compared to 1007 matches when using the correct dates. Thus, we estimate the rate of false positives (ν) to be 203/1007, or 20%.

1.4 Borrowers Rejected by a Competitor

Our first piece of evidence for adverse selection in an online lending market involves the most obvious case of adverse selection, that in which borrowers are offered loans by one lender and rejected by another. We compare the loan performance of Prosper borrowers rejected by Lending Club to borrowers not rejected by Lending Club. Our hypothesis is that borrowers rejected by Lending Club should perform worse, even when nothing in Prosper's data set suggests that this should be the case. Specifically, out hypothesis is:

Hypothesis 1 (H1): *Loans issued to applicants who were rejected by a competitor underperform, all else equal.*

This hypothesis is supported in the theoretical literature (Broecker, 1990; Hauswald and Marquez, 2006; Fishman and Parker, 2015) and is intuitive, but has not been tested empirically in the existing literature. We first match borrowers between Lending Club and Prosper to construct a dummy variable indicating whether the borrower was rejected. Then, we use a Cox proportional hazards model of default. We use a number of different techniques to ensure that we are comparing rejected borrowers to non-rejected borrowers that appear equally creditworthy to Prosper. These techniques include running the hazard model on a matched sample of rejected borrowers and non-rejected borrowers who received identical loan contracts.

1.4.1 Rejected Borrowers and Default: Methods

We expect borrowers rejected by one lender to default more frequently when accepted by another lender. We must ensure, however, that we are comparing rejected and non-rejected borrowers who appear identical to the lender. In an attmept to do so, we will use four different methods to test our hypothesis. All four of these methods include a proportional hazards model, but we will use various matching and weighting schemes. We will use a standard proportional hazards model with no weighting or matching, an inverse probability of treatment weighting (IPTW), propensity score matching, and exact matching based on contract variables.

The first of these methods is a straightforward proportional hazards model, without any weighting or matching. The specification is the following:

$$\lambda_i(t) = \lambda_0(t) \exp(X_i'\beta + \gamma Rejected_i)$$

where $\lambda_i(t)$ is the instaneous default hazard rate for loan i at time t, $\lambda_0(t)$ is the baseline hazard rate at time t, X_i is a vector of borrower and contract variables, and *Rejected* is an indicator variable taking a value of 1 if the borrower was rejected by Lending Club. The coefficient of interest is γ, and $\gamma > 0$ is our hypothesis.

The second method, IPTW, consists of weighting each observation by the inverse probability of receiving the treatment that was received (reciprocal of probability of rejection for rejected borrowers and reciprocal of probability of not having been rejected for non-rejected borrowers). We calclulate the treatment probabilities using a logistic regression with the fol-

lowing specification:

$$Rejected'_{it} = x_{it}^{\top}\beta + \epsilon_{it} \tag{1}$$

where rejected is the latent variable corresponding to *Rejected*, and is greater than 0 where the borrower was rejected by Prosper. This logit yields probabilities of the following form:

$$\frac{p(Rejected'_{it})}{1 - p(Rejected'_{it})} = e^{x_{it}^{\top}\beta} \tag{2}$$

We then use the extracted treatment probabilities, to assign weightings of $1/p(Rejected'_{it})$ for rejected borrowers, and $1 - 1/p(Rejected'_{it})$ for non-rejected borrowers. These weights are then used in a weighted proportional hazards model, which is identical to the one used in method 1, except for the weighting scheme.

The third method, propensity score matching, makes use of the probabilities from the same logit, but instead of weighting all the observations, we create a matched sample of observations with similar rejection probabilities (propensity scores). Since there are many more non-rejections than rejections, we match each rejection to the three non-rejections with the nearest propensity scores, weighting each match with one third the weight of each rejected borrower. We run the same proportional hazards model as in method 1, but only on the matched sample, not on the full sample.

The last method consists of exact matching on all of the contract terms (loan term, interest rate, and amount). We match each rejected loan-month observation to 3 observations with the same term, interest rate, amount, and time to maturity. Where an exact amount match is not available, we use the closest available amount. As in method 3, we run the same proportional

hazrds model as in method 1, but on the matched sample rather than the full sample.

1.4.2 Rejected Borrowers and Default: Empirical Results

We report regression statistics for a Cox proportional hazards model without any matching or weighting in Table 1.4. We are interested in the coefficient on the dummy variable indicating whether or not the borrower was rejected by Lending Club. These coefficients of interest can be interpreted as follows: rejected borrowers are e raised to the coefficient size times more likely to default at an given instant. Since default is a relatively rare event, it is in general a good approximation that lifetime probability of default is affected by the same percentage as the instantaneous probability of default. Rejected borrowers are 2.2 times more likely to default. This reduces the expected return on a typical 19% interest loan from 7% to −6%, or a difference in total payments received of about \$5900 for a \$15000, 3-year maturity loan (\$10000 for a 5-year loan).

In Table 1.5, we report the regression statistics for a proportional hazards model using IPTW (column 1) and propensity score matching (column 2). Both of these methods are based on a first stage propensity scoring regression (not reported). This first stage regression is a logistic regression modeling whether or not a borrower was rejected. For IPTW, we calculate the probability of rejection for every borrower using the first stage logit. We then weight every observation in the second stage by the inverse probability of receiving the treatment that was actually received. So if $p(X)$ is the probability of rejection for a given borrower with covariates X, rejected borrowers are given weight $1/p(X)$, and non-rejected borrowers are given weight $1/(1-p(X))$. For propensity score matching, we use the same first state regression as for IPTW to calculate $p(X)$ for every borrower. In the second stage hazard model, however, we

Table 1.4: Rejected Borrowers and Default: Hazard Model

	Dependent variable:
	Default
Interest Rate	5.919*** (0.688)
Rejected	0.798*** (0.240)
City Size	−0.046*** (0.016)
Term	0.170* (0.103)
Issue Month	−0.007 (0.005)
FICO	0.001 (0.001)
Income	−0.0001*** (0.00001)
Inquiries	0.135*** (0.021)
Credit History Length	−0.006 (0.005)
Open Credit Lines	−0.008** (0.003)
Observations	3,386
R^2	0.067

Notes to Table: Regression statistics for a Cox proportional hazards model of default for all Prosper loans issued before 2014 in cities that have fewer than 10 entries in the Lending Club rejection data set. Default is defined as the first missed payment. Interest Rate is the interest rate on the loan (e.g. 0.10 if the interest rate is 10% annually). Rejected is an indicator taking a value of 1 for loans that correspond to borrowers who were rejected by Lending Club (matching procedure described in body of paper). City Size is the number of Lending Club rejections given to all borrowers in a particular city. Issue Month assigns an integer to every month (starting with 1 for the first month). The other control variables are standard credit report variables. *p<0.1; **p<0.05; ***p<0.01.

use a sample that consists of every rejected borrower and the 3 non-rejected borrowers with the closest values for p. Other non-rejected borrowers are not included in the sample. Using these two propensity-score-based methods, we estimate that rejected borrowers are 2.2 (IPTW) and 1.8 (matching) times more likely to default, very much consistent with the 2.2 we calculate for the hazard model without matching or weighting.

Table 1.5: Rejected Borrowers and Default: Propensity Scoring

	Dependent variable:	
	Default	
	(IPTW)	(Propensity Matching)
Interest Rate	5.920*** (0.969)	9.472*** (2.134)
Rejected	0.797** (0.341)	0.572** (0.234)
City Size	−0.046** (0.023)	−0.087* (0.047)
Term	0.170 (0.145)	−0.473 (0.320)
Issue Month	−0.007 (0.007)	−0.025* (0.014)
FICO	0.001 (0.001)	0.008** (0.003)
Income	−0.0001** (0.00002)	−0.00002 (0.00004)
Inquiries	0.135*** (0.030)	0.128 (0.099)
Credit History Length	−0.006 (0.008)	−0.004 (0.018)
Open Credit Lines	−0.008* (0.005)	−0.007 (0.010)
Observations	3,386	200
R^2	0.034	0.245

Notes to Table: Regression statistics for a Cox proportional hazards model of default for Prosper loans issued before 2014 in cities that have fewer than 10 entries in the Lending Club rejection data set. Column 1 is an inverse probability of treatment weighted logit, and includes the full sample. Column 2 is a logit on a propensity score matched sample in which each rejected borrower is matched to 3 non-rejected borrowers. *p<0.1; **p<0.05; ***p<0.01.

In Table 1.6, we report the regressions statistics for a Cox proportional hazards using exact matching based on all contract variables. This is a 3-to-1 match, just like the propensity

score matching, but instead of finding the closest 3 propensity score matches, we find 3 exact matches for all contract terms (interest rate, maturity, and loan amount). Sometimes, exact amount matches are not available, and so we use the closest available amount. Exact matching on all contract variables is the most effective method for confirming that we are comparing borrowers who appear to have the same creditworthiness to Prosper. There is no plausible explanation for why Prosper would see a difference in creditworthiness between two borrowers but offer them the same contract.[13] According to the exactly matched hazard model, rejected borrowers are 2.0 times more likely to default.

Table 1.6: Rejected Borrowers and Default: Exact Matching

	Dependent variable:
	Default
Interest Rate	8.671*** (2.239)
Rejected	0.679*** (0.233)
City Size	−0.068 (0.047)
Term	−0.339 (0.307)
Issue Month	−0.019 (0.015)
FICO	0.006* (0.003)
Income	0.00002 (0.00004)
Inquiries	0.078 (0.091)
Credit History Length	0.006 (0.018)
Open Credit Lines	−0.00001 (0.009)
Observations	193
R^2	0.186

Notes to Table: Regression statistics for a Cox proportional hazards model of default for Prosper loans issued before 2014 in cities that have fewer than 10 entries in the Lending Club rejection data set. The sample is exactly matched on all contract terms (so that treated borrowers are being compared to untreated borrowers who received exactly the same contract). Contracts consist of 3 attributes: interest rate, term, and loan amount. Each rejected borrower is matched to 3 non-rejected borrowers. *p<0.1; **p<0.05; ***p<0.01.

[13] We also know that borrowers are only given the same contract when they are assigned the same risk grade.

We use several alternative methods of determining whether borrowers were rejected. As mentioned previously, we use different small city size thresholds and also use FICO score and debt to income as additional criterion for mapping between the Lending Club rejections and Prosper acceptances. For brevity, we do not report summary or regression statistics using any of these alternative samples. Use of FICO score and/or debt to income restrictions does not change the results, except that they slightly decrease the sample size (and therefore the t-stats). Decreasing the city size threshold also merely reduces the sample size. Increasing the city size threshold decreases the magnitude of the coefficients because it introduces measurement error, but increases the t-stats because it increases the sample size.

1.4.3 Economic Magnitude

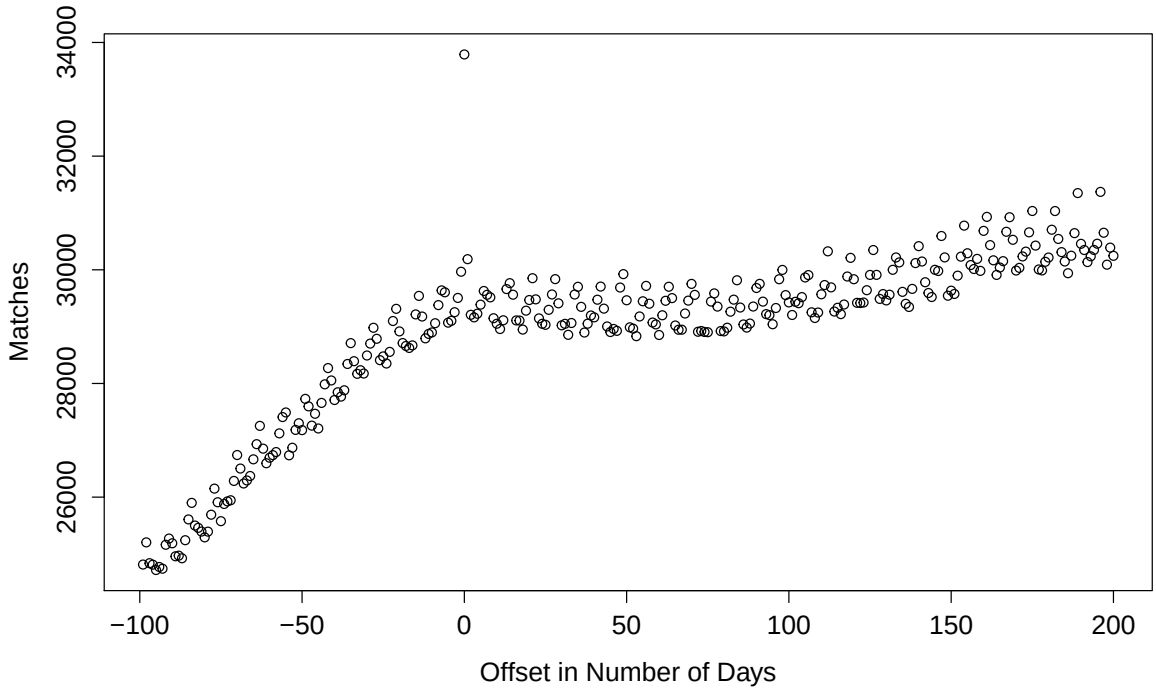
These results provide evidence of the winner's curse. Borrowers rejected by Lending Club have negative returns on their Prosper loans. However, just because a rejection by a competitor is a very negative signal when it occurs does not mean that the possibility of a rejection has a large distorting effect on the market as a whole. What is the overall impact of the winner's curse from the possibility of rejections on this market?

The first question we need to answer is what fraction of borrowers were rejected by a competitor. To answer this question, we must use a matching technique that eliminates the possibility of false negatives. With this objective in mind, we construct 300 different matches between the Lending Club rejection data set and the Prosper loan data set. For loan and rejection to be considered matched, they must be from the same city and occur n days apart, we let n vary between -100 and 200.

We graph these matches in Figure 1.1. At 0 on the x-axis are same-day matches. At 1 on

the x-axis are matches where the Prosper loan application comes 1 day after the Lending Club rejection, and so on. In the figure, we clearly see a baseline of false positives, with a signal for the same day matches, as well as the matches. We can see just over $4,000$ genuine matches for the same day, and about $5,000$ total genuine matches including the same day matches and the matches where the rejection occurs one day before or after the acceptance at the competitor. The total number of loans is $180,000$, meaning that 2–3% of Prosper borrowers were rejected by Lending Club.

Figure 1.1: Placebo Matches



Notes to Figure: Graph shows the number of Prosper loans (out of $180,000$ total) which match to an application in the Lending Club rejection data set. To match to a Lending Club loan, the Prosper loan must come X days after the Lending Club rejection from the same city (where X is the x-axis in the graph).

This means that the possibility of rejection by a competitor impacts expected annual returns by 0.25%. For high interest rate (30%) loans, the probability of rejection is higher, as is the effect of rejection on returns. High interest rate loans suffer a 2.5% decrease in returns due to the possibility of rejection by a competitor. Loans to borrower rejected by Lending Club experience $10 million dollars more charge-offs annual than similar loans to non-rejected borrowers. It is important to keep in mind that this is the figure for only one lender based on rejections from only one other lender. It is not a market-wide measure.

1.5 Borrowers' Contract Choice

In the extreme case of adverse selection, discussed in the previous sections, borrowers select lenders because they are rejected by the other lender. However, many borrowers are offered a contract by more than one lender. In this case adverse selection should still occur. Most existing studies focus on interest rate as the primary driver of adverse selection—risky borrowers are more likely to accept high interest rate contracts, especially in the case where they apply to multiple lenders and can accept the best contract. However, low interest rate is not the only reasonable notion of the "best" contract. An applicant might also deem a contract best because of the loan's amount. If borrowers taking out higher loan amounts are riskier, and borrowers select based on loan amount, a form of adverse selection based on loan amount will occur.

Before testing directly for adverse selection, we will first consider selection, ignoring it's relationship with borrower risk. In this section, we consider the drivers of contract choice. Do applicants choose contracts because of low interest rates or because of high loan amount? We hypothesize that they do both.

Hypothesis 2 (H2): *Individuals self select into platforms, i.e., are more likely to choose the platform that offers them a lower interest rate or higher loan amount, all else equal.*

However, the magnitude of these effects is an empirical question, as is whether borrowers select primarily based on loan amount or on interest rate.

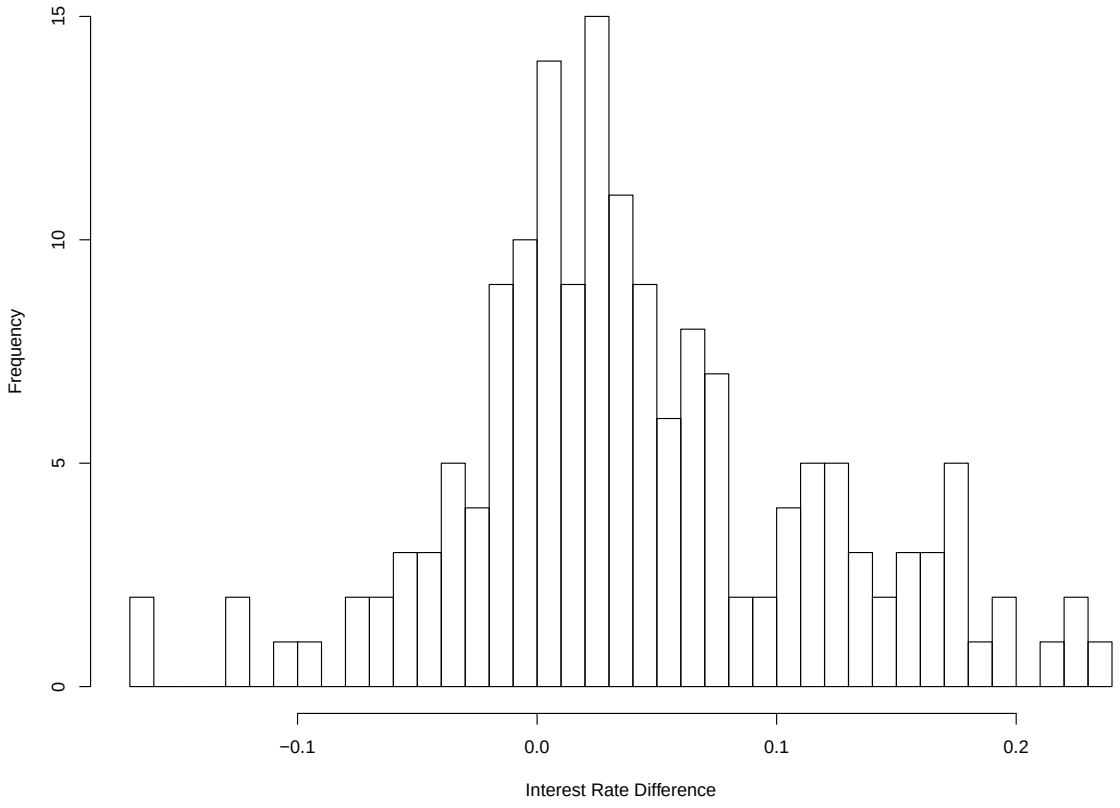
For our analysis of contract choice, we focus on Lending Club borrowers who withdrew their Prosper applications. We do not have data on borrowers who initially turn down the lenders' contract offers, and we do not include canceled and expired listings because cancellation and expiration are outside the borrowers' control, and thus the contract terms on these loans should not have an impact on lender choice.

Unfortunately, our data set includes only borrowers who decided to withdraw their Prosper applications and take out Lending Club loans. We do not observe the Lending Club offers for borrowers who may have applied to Lending Club, but decided not to withdraw their Prosper applications. We also do not observe hypothetical offers for borrowers who did not apply to Lending Club.

For all Lending Club loans which match to a withdrawn Prosper listing, we show a histogram of interest rate differential (offered Prosper rate minus the rate on the Lending Club loan) in Figure 1.2. 73% of borrowers who withdrew their loans from Prosper received a lower interest rate from Lending Club. This is consistent with the idea that borrowers withdraw their loans from Prosper to take out lower interest loans from Lending Club. However, the magnitude is much too low for interest rate to be borrowers' only reason for switching lenders.[14]

[14] We would expect 90% in this case: 100% for the 80% of matches that are true positives and 50% for the 20% that are false positives.

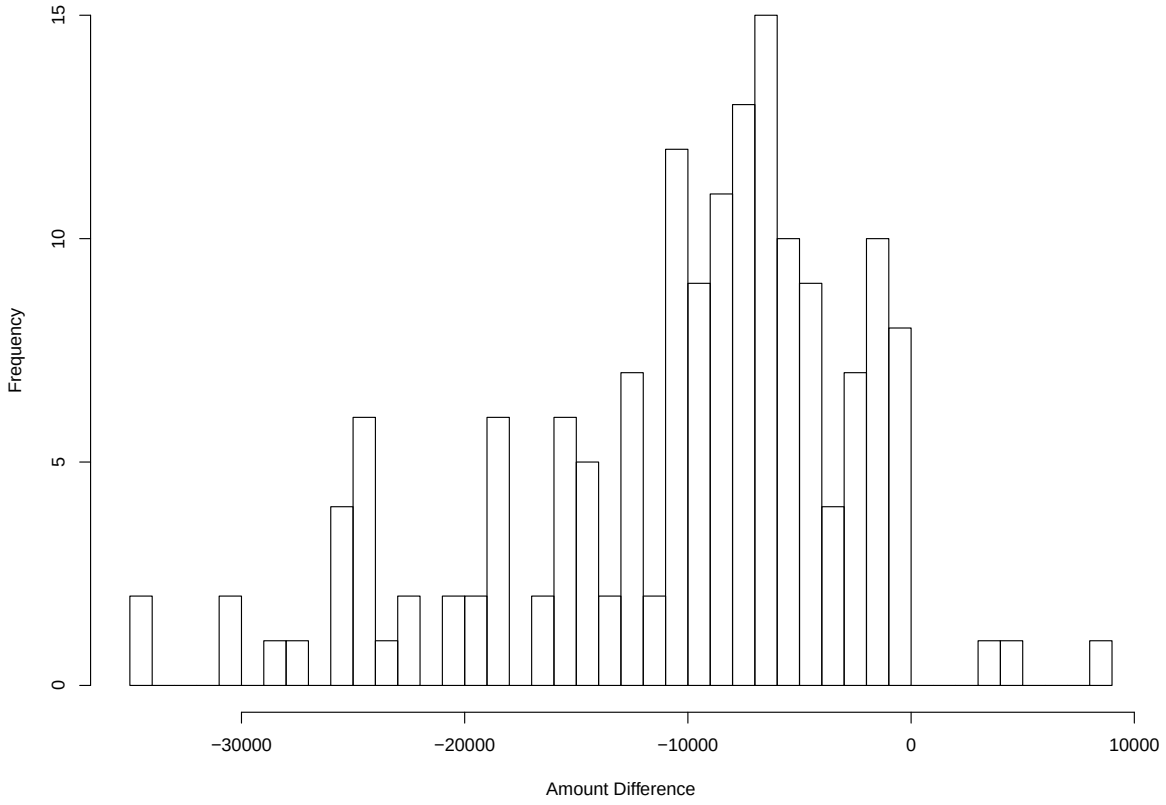
Figure 1.2: Interest Rate Difference Between Lenders



Notes to Figure: Histogram of the interest rate difference (Prosper minus Lending Club) for borrowers with withdrawn Prosper listings and Lending Club loans.

Because interest rate difference does not seem to fully explain why borrowers withdraw their Prosper listings and take Lending Club loans, we also consider loan amount. Perhaps some lenders are choosing to withdraw listings in order to receive a larger loan rather than a lower interest rate loan. In Figure 1.3, we show a histogram of loan amount difference (offered Prosper amount minus Lending Club loan amount). The figure shows that 98% of borrowers who withdrew from Prosper to take out Lending Club loans received larger loans from Lending Club.

Figure 1.3: Loan Amount Difference Between Lenders



Notes to Figure: Histogram of the loan amount difference (Prosper minus Lending Club) for borrowers with withdrawn Prosper listings and Lending Club loans.

Loan amount thus seems to be a more important consideration than interest rate to borrowers choosing which loan offer to accept. It is impossible to say what loan amounts borrowers who didn't switch lenders could have received, but the fact that almost all switching borrowers receive larger loan amounts strongly suggests that loan amount is an important factor in borrowers' choice of loan.

1.6 Borrowers with Other Applications

In this section, we consider a more mild case of adverse selection, that in which borrowers made applications to other lenders. We cannot in general determine which borrowers made applications to other lenders. However, we can identify a group of borrowers who had loan listings on Prosper, but eventually took out loans from Lending Club instead. Only borrowers who apply to Prosper and initially accept their contract offers will have listings, but many borrowers apply to Prosper and are listed on the site, but then do not eventually receive loans. This can occur if borrowers don't complete the verification tasks required (or fail verification) and have their listings canceled by Prosper, withdraw their loans, or have their listings expire without being funded. We expect that loans to borrowers who applied to the competitor should perofrm worse. This is our Hypothesis 2.

Hypothesis 3 (H3): *Loans issued to applicants who had loan offers with a competitor underperform, all else equal.*

1.6.1 Empirical Results: Borrowers with Other Applications

We assess the relationship between having an outside offer and default using the same techniques we used to assess the relationship between rejection and default. The specification remains exactly the same as described previously, except that the variable "Prosper Listings" replaces "Rejected." Just as for the rejection analysis, we use a Cox proportional hazards model of default with four different matching or weighting schemes (equal weights, inverse probability of treatment weights, propensity score matching, and contract-variable matching). The regressions statistics are reported in Table 1.7.

Table 1.7: Competing Offers and Default: Cox Regressions

	Dependent variable:			
	Default			
	(Standard Cox)	(IPTW)	(Propensity Matching)	(Exact Matching)
Interest Rate	11.470*** (0.066)	12.243*** (0.043)	12.236*** (0.814)	10.192*** (0.821)
Prosper Listing	0.455*** (0.080)	0.415*** (0.004)	0.156** (0.068)	0.310*** (0.080)
City Size	0.00000*** (0.00000)	0.00000*** (0.00000)	0.00001 (0.00001)	0.00001 (0.00001)
Term	0.0003 (0.0003)	−0.002*** (0.0002)	0.002 (0.003)	0.006* (0.003)
Issue Month	−0.008*** (0.0002)	−0.008*** (0.0001)	−0.008*** (0.002)	−0.007*** (0.002)
Income	−0.00000*** (0.00000)	−0.00000*** (0.00000)	−0.00000*** (0.00000)	−0.00000*** (0.00000)
Inquiries	0.104*** (0.002)	0.110*** (0.002)	0.101*** (0.029)	0.148*** (0.030)
Credit History Length	−0.0002*** (0.00001)	−0.0005*** (0.00002)	−0.001** (0.0005)	−0.001** (0.0005)
Open Credit Lines	0.010*** (0.001)	0.004*** (0.0004)	0.011 (0.008)	0.009 (0.008)
Observations	1,415,273	1,415,273	4,028	4,028
R^2	0.036	0.018	0.091	0.082

Notes to Table: Regression statistics for a Cox proportional hazards model of default for all Lending Club loans. Prosper Listing is an dummy variable indicating whether the borrower has a Prosper listing. Column 1 is a standard Cox. Column 2 uses inverse probability of treatment weighting (IPTW). Column 3 uses propensity score matching. Column 4 uses exact matching based on all contract variables (interest rate, term, and loan amount). Columns 3 and 4 use 3-to-1 matching (3 non-canceled loans matched to every canceled loan). *p<0.1; **p<0.05; ***p<0.01.

The different weighting and matching techniques yield a range of different coefficients on Prosper Listings. However, in all specifiations, having a Prosper listing is associated with at least a 17% increase in the default hazard rate (unadjusted for attenuation bias due to noise in the variable of interest). The increase is statistically significant in all specifications.

1.6.2 Canceled Loans

In this section, we repeat the analysis of the previous section, but focus on only canceled loans instead of expired, withdrawn, or canceled loans. We expect cancelation to be associated with increased default risk, but with a magnitude in between that of a rejection or an outside application. This is because an outside application implies that other lenders evaluated the borrower as riskier, a rejection implies that other lenders saw him as too risky to give any contract offer, and a cancelation is somewhere in between. A cancellation could be similar to a rejection–it can occur when lenders identify the borrower's application as fraudulent. But cancellations can also occur when borrowers simply decide not to follow through with the verification process, perhaps becuase they received a better offer from Lending Club. In Table 1.8, we report the results of our Cox proportional hazards using all four of the weighting/matching techniques.

The coefficients on cancellation are all between 0.42 and 0.50, suggesting that borrowers who had canceled listings are about 60% more likely to default. Adjusting for attenuation bias, this figure is 75%. The effect size, especially for the matched samples, is higher for canceled loans than for all expired/withdrawn/canceled loans, suggesting that cancelation implies a more negative evaluation on the part of the competitor.

This result is consistent with each lender facing a similar winner's curse problem. The

Table 1.8: Canceled Listings and Default: Cox Regressions

	Dependent variable:			
	Default			
	(Standard Cox)	(IPTW)	(Propensity Matching)	(Exact Matching)
Interest Rate	11.469*** (0.066)	11.871*** (0.042)	10.925*** (0.926)	10.112*** (0.894)
Canceled	0.498*** (0.088)	0.478*** (0.004)	0.447*** (0.082)	0.425*** (0.093)
City Size	0.00000*** (0.00000)	−0.00000*** (0.00000)	−0.00000 (0.00001)	−0.00000 (0.00001)
Term	0.0003 (0.0003)	−0.004*** (0.0002)	0.001 (0.004)	0.004 (0.004)
Issue Month	−0.008*** (0.0002)	−0.011*** (0.0001)	−0.017*** (0.003)	−0.007** (0.003)
Income	−0.00000*** (0.00000)	−0.00000*** (0.00000)	−0.00000 (0.00000)	−0.00000* (0.00000)
Inquiries	0.104*** (0.002)	0.057*** (0.002)	0.023 (0.041)	0.035 (0.040)
Credit History Length	−0.0002*** (0.00001)	−0.0004*** (0.00002)	−0.001** (0.001)	−0.001* (0.001)
Open Credit Lines	0.010*** (0.001)	0.001*** (0.0004)	0.010 (0.010)	0.012 (0.008)
Observations	1,415,273	1,415,273	3,360	3,360
R^2	0.036	0.018	0.081	0.069

Notes to Table: Regression statistics for a Cox proportional hazards model of default for all Lending Club loans. Canceled indicates whether the borrower had a canceled Prosper listing. Regression statistics for a Cox proportional hazards with default as the dependent variable. Column 1 is a standard Cox. Column 2 uses inverse probability of treatment weighting (IPTW). Column 3 uses propensity score matching. Column 4 uses exact matching based on all contract variables (interest rate, term, and loan amount). Columns 3 and 4 use 3-to-1 matching (3 non-canceled loans matched to every canceled loan). *p<0.1; **p<0.05; ***p<0.01.

coefficient on rejection for Prosper loans are a third larger, compared to the coefficient on cancellation for Lending Club loans. This is to be expected given that a cancellation is an ambiguous signal, whereas a rejection is not. Taken together, our results for rejected and canceled applications suggest that both Lending Club and Prosper face a large winner's curse problem in that their pool of applicants includes borrower rejected by their competitor. The size of the winner's curse is relatively small when the borrower has simply made an application with a competitor, larger in the case where his loan was canceled by the competitor, and largest when his application was rejected by the competitor.

1.7 Adverse Selection: Interest Rate and Amount Dimensions

The evidence we have presented suggests that lenders face an adverse selection problem, and that borrowers select based on interest rate and, more importantly, loan amount. We now

attempt to tie these to strands of evidence together. We know that applicants who applied to a competitor perform worse. We also know that applicants pick their loans based on interest rate and loan amount. The obvious explanation for these two pieces of evidence is that they are related. Borrowers apply to multiple lenders, who assess their riskiness. Less risky borrowers are assigned lower interest rates and higher loan amounts.[15] Borrowers accept contract offers based on some combination of low interest rate and high interest rate. This means that borrowers with competing offers should perform worse, because the competitor evaluated them as worse. If this is true, however, we would expect not only that borrowers with competing offers should perform worse, but also that borrowers with worse competing offers should perform worse and borrowers with better competing offers should perform better. In other words, we expect that the higher the extent of adverse selection, the higher the level of loan underperformance.

Empirically, we break this down into a two-part hypotheses, the first part based on loan amount and the second on interest rate. We use the differences is interest rate (or loan amount) between what the two lenders offered as a measure of adverse selection. If the interest rate offered by the competitor is only slightly higher, the lender should theoretically face a small adverse selection problem, but if it is much, higher the adverse selection problem is likely much larger, and we would thus expect a much higher default rate. The same argument applies to situations where the competitor offers a much lower loan amount. Our hypothesis

[15] While it is obvious why riskier borrowers receive higher interest rates, it is not obvious why they must receive lower loan amounts. Lower interest rate offers result in lower returns mechanically. Higher loan amounts do not mechanically result in lower returns. However, the additional debt burden associated with higher loan amounts can result in lower returns. (See Fuster and Willen (2017) and Tracy and Wright (2016) for evidence on payment size and default.) Adverse selection (of a different form than that discussed in this paper), where the worst borrowers choose to take out the largest loans, may also be a factor. In any case, higher loan amounts are associated with higher default rates, and borrowers may experience a winner's curse on the loan amount dimension as well as on the interest rate dimension.

is thus:

Hypothesis 4 (H4): *For Lending Club borrowers with competing offers from Prosper, the higher (lower) the difference between interest rate (loan amount) offers from Prosper and Lending Club, the higher the probability of default.*

We test whether the interest rates and loan amounts on canceled/withdrawn/expired Prosper listings have explanatory power for the performance of Lending Club loans made to the same individual. As in the previous sections, we use a Cox proportional hazards model of default. In this case, the difference between the competitor's interest rate and the true interest rate on the loan (*Interest Rate Diff*) is the variable of interest. To avoid multicollinearity issues, we use interest rate dummy variables instead of a continuous measure of interest rate. All loans are included in the sample, not just canceled/withdrawn/expired loans. Loans without corresponding Prosper listings are coded as having an *Interest Rate Diff* and *Amount Diff* of 0. The choice of this value has no effect on the value or standard error of the coefficients of interest, as the choice is absorbed by the canceled/withdrawn/expired coefficients. In our regressions specification, the hazard rate at time t for individual i is specified as:

$$\lambda_i(t) = \lambda_0(t) \exp(X_i'\beta_i + \textit{Interest Rate}'\beta_2 + \gamma \textit{Interest Rate Diff}_i + \delta \textit{Amount Diff}_i)$$

where $\lambda_0(t)$ is the baseline hazard rate at time t, X_i is a vector of controls and *Interest Rate*$_i$ is a vector of interest rate indicators. γ and δ are the coefficients of interest. If $\gamma > 0$, then the competitor's interest rate offer provides credit risk information not present in Lending Club's own model. If $\delta < 0$, then the competitor's amount offer provides such information.

We report regression statistics in Table 1.9. In Column 1, we include only interest rate fixed effects, expired/canceled/withdrawn indicators, and the difference in interest rate offers. In Column 2, we add state, and issue time indicators; in Column 3, we add other contract variables besides interest rate (as well as the differences between the contract offers at Prosper versus Lending Club); in Column 4, we add credit variables. All variables in the regressions are standardized.

The coefficient on *Interest Rate Diff* is significantly positive in all specifications. A one standard deviation change in *Interest Rate Diff* is associated with a 16% increase in default hazard. For every percentage point increase in interest rate difference, the default hazard rises by 2.3%. These values should be interpreted as a lower bound. Although our matching algorithm has a rate of false positives of about 20%, the coefficient of interest is likely attenuated more than this. This is because the largest interest rate differences are the most likely to be false positives. We cannot, however, estimate the exact level of attenuation bias.

The coefficient on *Amount Diff* is significantly negative. A one standard deviation change in amount difference is associated with a 22% increase in default hazard, as opposed to 16% for a one standard deviation change in interest rate difference. For every $\$1,000$ decrease in the competitor's amount offer, borrowers are 2% less likely to default. Competitor's offered loan amount seems to matter at least as much as competitor interest rate.

These results demonstrate that Lending Club faces a winner's curse problem because of the possibility of higher interest rate offers from Prosper, as well as because of the possibility of smaller loan amount offers from Prosper. When the Prosper interest rate is higher, the default rate is higher, and when the Prosper loan amount is lower, the default rate is also higher. In

Table 1.9: Interest Rate Difference and Default

	Dependent variable:			
	default			
	(1)	(2)	(3)	(4)
Interest Rate Diff	0.146**	0.163**	0.166**	0.149**
	(0.072)	(0.072)	(0.072)	(0.072)
Amount Diff			−0.206**	−0.195**
			(0.086)	(0.087)
Term Diff			−0.003	−0.002
			(0.006)	(0.006)
Expired	0.0001	−0.004*	−0.005**	−0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
Withdrawn	0.005**	0.001	−0.001	−0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Canceled	0.011***	0.010***	0.009***	0.009***
	(0.002)	(0.002)	(0.002)	(0.002)
Term			−0.012	−0.014
			(0.063)	(0.063)
Loan Amount			−0.014***	0.052***
			(0.003)	(0.004)
Income				−0.286***
				(0.007)
Inquiries				0.091***
				(0.002)
Credit History Length				−0.634***
				(0.058)
Open Credit Lines				0.051***
				(0.003)
Interest Rate	Yes	Yes	Yes	Yes
State	No	Yes	Yes	Yes
Half-Year Indicators	No	Yes	Yes	Yes
Observations	1,415,364	1,415,364	1,415,364	1,415,334
R^2	0.032	0.040	0.040	0.042
Note:				*p<0.1; **p<0.05; ***p<0.01

Notes to Table: Regression statistics for a Cox proportional hazards model of default for all Lending Club loans. Expired, Withdrawn, and Canceled indicates whether the borrower had a Prosper listing that expired or was withdrawn or canceled. Interest Rate Diff is the Prosper's interest rate (on the listing) minus Lending Club's interest rate (on the issued loan). Amount Diff (Term) is the same, but for amount (term) instead of interest rate. Specifications contain interest rate, state, and half-year fixed effects as indicated above. All columns are a standard equally-weighted Cox proportional hazards model, but with different specifications. Variables are standardized. *p<0.1; **p<0.05; ***p<0.01.

Section 4, we showed that the winning Lending Club loan offer has an interest rate 3.5 p.p. lower and a loan amount $9,950 larger than the losing Prosper loan offer. This means that lenders face an adverse selection problems—borrowers offered higher interest rates or lower loan amounts by competitors default at higher rates, and these are the borrowers who usually accept offers. Adverse selection based on offered amount seems to be at least as strong as selection based on interest rate.

1.8 Conclusion

We provide evidence of adverse selection in an online personal lending market. Only borrowers who were rejected or offered less attractive contracts by competitors should accept offers. This results in adverse selection, where each lender makes loans only to borrowers evaluated as riskier by its competitors. We show that adverse selection occurs along multiple dimensions in the same market. Borrowers rejected by competitors, borrowers offered higher interest rates by competitors, and borrowers offered lower loan amounts by competitors all perform worse. Although the existing literature has focused mostly on interest rate, loan amount actually seems to more important to borrowers when choosing a loan, and at least as important in creating adverse selection.

We show that borrowers rejected by a competitor are twice as likely to default. Riskier borrowers are offered higher interest rates and lower loan amounts. We provide evidence that borrowers select loan contracts based on interest rate and, more importantly, loan amount. In other words, borrowers evaluated as risky by the competitor are more likely to accept an offer. This creates an adverse selection problem; we show that borrowers with competing offers are

about 30% more likely to default. Furthermore, borrower offered relatively higher interest rates by the competitor are even more likely to default, as are borrower offered relatively lower loan amounts from the competitor. In other words, when a lender is the most attractive offer, it is underestimating default, and the more attractive its offer, relative to that of competitors, the more it is underestimating.

On the whole, our research sheds new light on adverse selection in competitive lending markets, both by providing empirical evidence for longstanding hypotheses concerning rejection and adverse selection, and by exploring new channels through which adverse selection emerges, such as loan amount.

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2 Interest Rates, Default, and Consumption: Evidence from Small Changes in the National Mortgage Rate

2.1 Introduction

The interest rate at loan origination has long-term effects on a household. In the short-term, higher interest payments reduce household cash flow and can crowd out other consumption and investment. In the longer term, higher interest payments result in increased exposure to liquidity shocks and deflation if refinancing is difficult.[1] When high interest rates are combined with high housing volume, many households will be particularly exposed to a negative macroeconomic shock. Paradoxically, the main way central banks address "overheating" housing markets is to increase interest rates, possibly begetting fragility. It is therefore critical to know the sensitivity of household behavior to interest rates.

Higher interest rates can lead to default during a housing bust, and this has been a focus of policy and research. There is vast evidence of liquidity constraints among households,[2] so some defaults may be prevented through systematic payment reductions.

[1] This household balance sheet channel is theoretically described by Bernanke and Gertler (1995), Mishkin (2007), and Garriga, Kydland, and Sustek (2017). These papers focus on the effect of inflation on the real cost of nominal mortgage payments.

[2] For example, Kaplan and Violante (2014) classify a third of households in 2001 as "wealthy hand-to-mouth," who have illiquid wealth but are unable to smooth their consumption with liquid wealth. Housing is a primary source of illiquid wealth—when it crashes, these households become even more constrained.

Eberly and Krishnamurthy (2014) provide theoretical support for such policies, while Fuster and Willen (2017) and Tracy and Wright (2012) find empirical support by examining defaults around rate resets of adjustable rate mortgages (ARMs). Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017) further show that large payment reductions cause durable consumption and deleveraging to increase. Policy makers, through the Home Affordable Refinancing Plan (HARP), attempted to reduce defaults with a payment reduction strategy by subsidizing lenders who refinance underwater borrowers that are still current on their payments. A primary challenge with HARP is low lender participation. The average 30-year fixed mortgage rate from 2004 to 2007 ranged from 5.5-6.8%; from 2009 to 2012 the rate ranged from 3.3-5.6%. Thus, refinancing a mortgage involved a substantial transfer from the lender to the borrower (Remy, Lucas and Moore (2011)). HARP's success was therefore tightly tied to the level of competition in the lending market (Amromin and Kearns (2014), and Agarwal, Amromin, Chomsisengphet, Piskorski, Seru and Yao (2015)).[3] In short, financial intermediaries play an important role in mortgage modifications of all kinds, and these intermediaries generally do not want to implement the modifications subsidized by policy makers during the crisis. It is worth asking, then, whether smaller rate reductions (which may be more amenable to lenders) could still prevent defaults and insulate consumption.

[3] In addition to HARP, the HAMP (Home Affordable Modification Program) created incentives for lenders to reduce the principal on delinquent homes. Like HARP, A primary challenge with HAMP is incentivizing financial intermediaries to restructure loans (Agarwal, Amromin, Ben-David, Chomsisengphat, Piskorski, and Seru (2015)).

This paper studies the effect of relatively modest differences in interest rates at origination on fixed-rate borrower behavior, particularly default and consumption. Rate resets studied by Fuster and Willen (2017) and Agarwal, Amromin, Chomsisengphet, Piskorski, Seru and Yao (2015) average over 3 percentage points (p.p) and 1.4 p.p., respectively. But lenders resist changes this large. If smaller reductions in interest rates can still prevent defaults, then policy makers may have an extra arrow in their quiver to combat defaults when lenders do not want to participate in bigger reductions. Further, studying differences in rates at loan origination gives central bankers a fuller picture of the potential long-term costs of rate increases—fixed rate borrowers may be unable to refinance in response to future rate cuts.

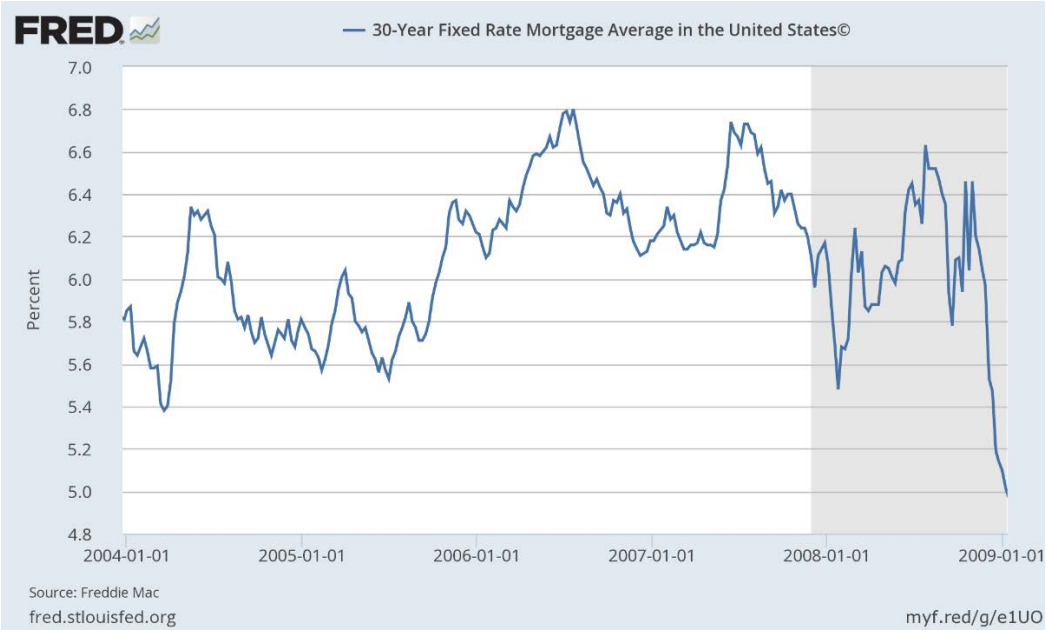
Identifying the effect of modest differences in interest rates on borrower behavior is empirically challenging. The first problem is that interest rates are a function of credit quality, so higher rates conflate worse credit with higher payments. To get around this problem, the literature uses natural experiments such as the HARP program or the timing of adjustable-rate-mortgage (ARM) resets. These interest rate resets are quite large, however, and policy makers did not attempt a program of smaller changes in rates. Further, these experiments are confined to the crisis and its aftermath.

To study modest differences in rates, we use within-year changes in the 30-year fixed rate mortgage average from Freddie Mac. Figure 2.1 plots the rate from 2004 to 2008. This rate fluctuates month-to-month, and is strongly predictive of the contracted rate at origination. This rate had a range of 84 basis points in 2005; 69 bps in 2006; 60bps in 2007; and 153 bps in 2008 (115 if we exclude the last month of 2008). The standard deviation of

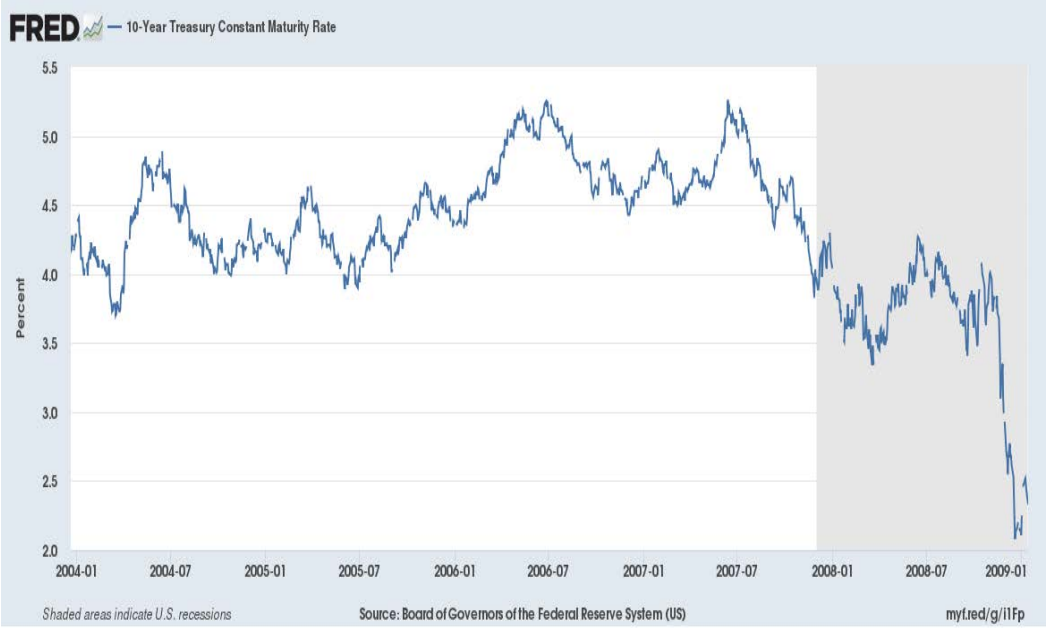
Figure 2.1: Variation in average mortgage rates

This figure plots our instrumental variable. We use within-year variation in the 30-year fixed rate mortgage average to estimate differences in payment size for mortgages originated in the same year and in the same 3-digit zip code. This series is plotted in Panel A. Panel B plots the 10-year Treasury Rate for comparison.

Panel A: 30-year fixed rate mortgage average in the U.S.



Panel B: 10-year Treasury constant maturity rate



this rate is 36 bps in our sample, and 24 bps within-year.[4] Further, the 30-year fixed rate is closely linked to the 10-year Treasury rate in our sample—from 2004-2007, the 30-year fixed rate moves one-for-one with the 10-year Treasury with a spread of 2 p.p. and an r-squared of over 80%. The primary driver of this rate, therefore, is likely investor's required return for long-term, safe debt, rather than borrower characteristics.

The intuition for our research design is as follows. Consider two identical homebuyers who purchase a home in the same year and in the same area. Further, each homebuyer will eventually encounter similar levels of negative equity in the same year after house prices crash. There is some randomness, however, in exactly when the buyer finds a suitable house and closes the deal. The first borrower may close when national rates are slightly higher than when the second borrower closes; going forward, the first borrower will have slightly higher payments. To give a sense of magnitudes, consider the median mortgage in our sample of $200,000. With a 6.5% rate in August 2007, the monthly payment is $1,264; with a 6% rate in December 2007, the monthly payment is $1,199, for an annual difference of $780. This modest difference in payments may impact borrower behavior.

The first stage of our analysis shows that contracted rates are indeed tied to the prevailing national rate. The relationship between the contracted rate and the national rate is mechanical (the national rate is the average of contracted rates), but also statistically strong amid other reasons for rate variation, like credit risk. A 50 basis point change in

[4] We will use a 50bp change in the national rate as a point of reference to interpret magnitudes throughout the paper. This is slightly more than two standard deviations of the within-year benchmark rate.

the national rate at origination leads to a predicted 36 basis point difference in the contracted rate, which amounts to about 3.7-4.7% of the total mortgage payment.

Our second stage uses variation in the national rate to study the effects of contracted rates on borrower behavior. A 50 basis point change in the national interest rate at origination leads to an increased default probability of 68-88 basis points for Freddie Mac loans. This effect size is consistent across a range of samples—from homes with 10% negative equity to homes with 50% negative equity. Relative the unconditional default probability, the effect is large—the effect is 20% of the average default probability for homes with 10% and 20% negative equity, and between 11-12.5% of the average default for homes with 30-50% negative equity. Given the small size of the differences in payment size, and the stability of the coefficient across a wide range of negative equity, our results are consistent with severe financial constraints.

While it is difficult to distinguish liquidity-driven defaults from strategic defaults[5], we find little direct evidence that borrowers strategically default due to small differences in rates. Our estimates are similar for a wide range of negative-equity borrowers—borrowers with 10% negative equity respond roughly the same to a small increase in rates as borrowers with 30% or 50% negative equity. But the strategic motives to default should vary substantially when moving from 10% negative equity to 30% or 50% negative equity. This leads us to conclude that our instrument is picking up liquidity-driven defaults

[5] Underwater borrowers with a slightly higher mortgage rate will be more likely to default for both liquidity and strategic reasons. The higher rate causes a higher payment today, which can cause a liquidity default. But the higher rate also causes a higher stream of future payments with a higher present value, which can make a strategic default more likely.

rather than strategic defaults. Further, our estimates are similar for homes in states with judicial foreclosures and homes in states without judicial foreclosures; likewise our results are largely similar for recourse and non-recourse states. But again, the strategic motives to default should vary substantially across these legal regimes, leading us to conclude that our results are primarily driven by liquidity defaults.

We next apply our instrument to households in the Consumer Expenditure (CE) survey that have recently purchased a home from 2001 to 2007. A 50 basis point change in the national rate at origination leads to a predicted change in monthly payment of 3-3.7%, similar to the Freddie Mac data. In the second stage, this reduction in payment increases consumption of nondurables by 2% and services by 20%, while durables decrease by a statistically insignificant 10%. At the median of the distribution, 3% of the mortgage payment is $330; 2% of nondurable spending is $360; 10% of durables is $351; 20% of services is $400. Thus, the reduction in mortgage payments is roughly equal to the increase in the sum of nondurable, durable, and service consumption. Further, we find no statistically distinguishable effect on total expenditures, suggesting that the mortgage payment largely crowds out consumption. The magnitude and timing of the differences in payment size across households correspond to the tax rebates studied by Johnson, Parker, and Souleles (2006), who find a similar response for nondurable consumption.

The main threat to our identification strategy is the possibility that some homebuyers time their purchases when rates are low. If more sophisticated buyers are better at buying when rates are low, for example, then our identification strategy will conflate lower payment size with sophistication. We think this is unlikely for a variety of

reasons. First, DeFusco and Paciorek (2017), using discontinuous changes in rates at the conforming loan limit that are much larger than what we study, find that borrowers respond little at origination to the change in rates. In particular, borrowers hardly reduce their loan amount even when doing so would substantially lower their rate. Second, we use within-year variation in interest rates, which should be difficult to forecast. Likewise, there is uncertainty regarding the time required to close a contract, which makes any attempt to time the market imprecise. Third, we follow Pei, Pischke, and Schwandt (2017) and perform balancing tests. We find that the instrument is not associated with poor credit quality beyond the higher payment size, nor is it correlated with total household expenditure in the CE survey. In fact, the instrument is correlated with lower loan balances and LTV ratios in the Freddie Mac data; this is consistent with higher interest rates deterring relatively low-quality borrowers, thus biasing our results toward zero. Finally, the instrument predicts prepayments. This is consistent with standard refinancing motives (the refinancing threshold is higher if the initial rate is higher), but is difficult to reconcile with worse credit quality or sophistication.

Our paper is most closely related to Fuster and Willen (2017), Tracy and Wright (2012), and Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017), who find that payment reductions reduce default by a substantial margin. Our study differs along several important margins. First, as mentioned previously, we study relatively modest changes in mortgage rates. If these small changes can also prevent default, then policy makers have an extra tool at their disposal. Second, we study 30-year fixed-rate mortgages rather than ARMs, which is a substantially larger market. The Federal

Housing Finance Agency's monthly interest rate survey shows ARMs peaking in the recent past during 2004, at which point they comprised 35% of the market; from 2005-2015, however, their share averaged 12% per year. Third, borrowers in the ARM market are likely different from typical mortgagors. ARM borrowers may be more sophisticated and sensitive to the long-term bond risk premia (Koijen, Van Hemert, and Van Nieuwerburgh (2009)). Similarly, ARMs tend to be riskier than agency mortgages, which may affect their sensitivity of default to payment size. With these differences in mind, Fuster and Willen (2017) find that cutting a borrower's payment in half reduces delinquency by 55%; we find that a more modest reduction of about 4% (about $600 per year) in the borrower's payment can still reduce delinquency by between 12-20% for underwater borrowers. Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017) find a large response for durable consumption and deleveraging; we find no effect for durables, but an increase in nondurables and services. We find the opposite effect of interest rates on deleveraging: households with higher interest rates are more likely to prepay their mortgage, though the magnitudes are economically small. Finally, our research design can be applied throughout the business cycle, whereas rate resets and HARP refinancings are largely restricted to the crisis.

This paper further relates to a growing literature on the role of monetary policy for household behavior. The 10-year Treasury Bond rate is closely tied to the national mortgage rate, and we show that small differences in the T-bond rate at origination can similarly predict default. Thus, we show that tight monetary policy can have a hangover effect years later during the housing crisis. This contrasts with the literature that focuses

on the role of concurrent monetary policy for stimulating household demand (Hurst and Stafford (2004), Mian, Rao and Sufi (2013), Chen, Michaux and Roussanov (2013), Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017), Agarwal, Chomsisengphet, Mahoney and Strobel (2015), Auclert (2017), Beraja, Fuster, Hurst and Vavra (2017)).

2.2 Related Literature

Our research is most closely related to the literature on mortgage payment size and default. Several existing papers attempt to assess the causal relationship between payment size and default. Tracy & Wright (2016), Fuster & Willen (2013), and Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017) use plausibly exogenous resets in ARM rates to assess the impact of mortgage payment size on default. ARMs are usually sold with a fixed rate for a period of time (usually 3, 5, 7, or 10 years) with interest rates resetting every year after the fixed term. Depending on the time of issue and on the contract chosen (3-year, 5-year, etc.) mortgage rates reset at different times, resulting in very large decreases in mortgage payments for loans resetting in 2008 or later. Using this variation in mortgage payment size, these papers find a large effect of mortgage payment size on default. Compared to these papers, our paper uses a broader sample, focuses on fixed-rate mortgages, and considers borrowers with negative and positive equity separately, since negative and positive equity borrowers have very different opportunities and incentives.

Our research is also related to the literature on the effectiveness of mortgage modification and refinancing programs such as HARP and HAMP. McManus, Janowiak, Ji, Karamon, & Zhu (2015) and Zhu (2012) consider the effectiveness of HARP directly. Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, & Seru (2016) consider the effectiveness of HAMP, focusing on the effectiveness of government intervention in modification rather than of modification generally. Haughwout, Okah, & Tracy (2016) find that larger modifications make re-defaults less likely and that principal forgiveness is more effective at preventing default than interest rate reduction. Mayer, Morrison, Piskorski, & Gupta (2014) caution that showing willingness to modify mortgages can encourage strategic default. Agarwal, Amromin, Ben-David, Chomsisengphet, & Evanoff (2011) warn that modification is more effective for on-balance sheet loans, meaning that incentives and information problems may reduce the effectiveness of modification programs in many contexts. These papers generally find modification to be helpful for preventing default, but sample selection issues and unwanted side effects make the overall effectiveness of modification unclear. Our research fits into the this stream of literature by isolating the effect of small differences in mortgage payment size, which is a necessary step in understanding whether or not modification programs as a whole are beneficial.

Our research is, more broadly, related to research on what causes mortgage default, particularly with respect to contract variables as opposed to borrower characteristics. A significant stream of literature discusses the relative importance of negative equity and liquidity constraints (Elul, Souleles, Chomsisengphet, Glennon, &

Hunt, (2010); Foote, Gerardi, & Willen, (2008); Bhutta, Dokko, & Shan, (2017); Gerardi, Herkenhoff, Ohanian, Willen, (2015)). Gross and Souleles (2002) and Agarwal, Liu, & Souleles (2007) have shown that liquidity constraints are often binding, but disentangling strategic concerns from liquidity concerns is difficult. Existing research has also considered the relationship between interest rate and default. Bajari, Chu, & Park (2008) use a measure of the difference in NPV between future payments using the actual contract rate and current prevailing rates, which they take from Deng, Quigley, and van Order (2000). In contrast to the existing literature on what causes mortgage default, we hope to provide a detailed assessment of the effects of a particular contract term.

2.3 Data and Sample Construction

We use two main datasets to test the effects of small changes in interest rate. First we use the Freddie Mac loan performance data to study defaults and prepayments, conditioning on the level of equity in the home. Second, we use the Consumer Expenditure (CE) survey to study differences in consumption from 2001-2007 for buyers with slightly different interest rates.

a) Mortgage Performance Data

Our primary dataset is the single family home loan level dataset from Freddie Mac, downloaded in November 2016. This data set contains loan level data at origination such as FICO score, loan-to-value, loan amount, interest rate, property

location to the 3-digit zip-code (ZIP3), and monthly loan performance data on a portion of fully amortizing fixed-rate mortgages guaranteed by Freddie Mac. The dataset covers approximately 22.1 million fixed-rate mortgages originated between January 1, 1999 and September 30, 2016. Monthly loan performance data is available through March 31, 2016. We use loans for new purchases (not refinancings) and exclude mortgages that were modified after origination, which leaves us with 4.81 million 30-year, fixed rate, single family, owner-occupied mortgages.

We supplement the Freddie Mac data with data from Zillow and from the Federal Reserve Economic Database (FRED). To calculate updated home values, we use Zillow's ZHVI Single-Family Homes Time Series, which provides the median single family home value in each zip code at a monthly frequency. We also use the 30-Year Fixed Rate Mortgage Average in the United States and 10-Year Treasury Constant Maturity Rate from FRED.

b) Mortgage Performance Sample Construction

Our central hypothesis is that borrowers with higher mortgage payments should default more frequently. We want to flexibly control for other factors that can drive default. In particular, there is a large literature on the effect of home equity on default and household behavior.[6] We thus build samples of borrowers facing similar

[6] For example: Palmer (2013), Posner and Zingales (2009), Geanakoplos (2010), Mian and Sufi (2014), and Bernstein (2017). The Home Affordable Modification Program (HAMP) was the Treasury's main program to reduce negative equity through principal reduction. HAMP is studied by SIGTARP (April 24, 2013), Agarwal, Amromin, Ben-David, Chomsisengphat, Piskorski, and Seru (2015) and Scharlemann and Shore (2016).

levels of home equity and run tests separately for each sample. This allows other control variables, such as credit score, to respond differently when the borrower becomes further underwater. We construct samples as follows.

First, we use the ZHVI Single-Family Homes Time Series (HPI) to calculate home equity for every borrower in the Freddie Mac sample for every month t using the following formula. We assume that the borrower paid fair market value for his home at time 0 and that the value of his home changes over time according to the HPI for his ZIP3 code.

$$House\ Value_t = \frac{Loan\ Amount / Loan\text{-}to\text{-}value * HPI_t}{HPI_0} \quad [1]$$

$$Equity\ in\ House_t = \frac{House\ Value_t - Mortgage\ Balance_t}{House\ Value_t}$$

Negative Equity Samples

We identify the time at which each borrower's equity first drops below a certain threshold (e.g., -10%,−20%,-30%,-40%, −50%). We exclude the loans that are not current (i.e. 'CURRENT LOAN DELINQUENCY STATUS' in the Freddie Mac performance data not equal to 0) when equity drops below the threshold. We observe each borrower for 12 months after his equity drops below the threshold, recording if he defaults, which we define as becoming 60 day delinquent, or voluntarily prepays. A borrower whose equity never drops below the threshold is not included in the sample, and a borrower whose equity drops below the threshold more than once is only included the first time his equity drops below the threshold.

Positive Equity Samples

To make our sample of loans with positive equity comparable to our samples of loans with negative equity, we require that the house value is falling. In particular, we require that the home equity drops by at least 10% before we add it to a sample. For example, in order to be added to the sample of loans with 10% positive equity, we require that the loan at one point had 20% positive equity, but suffered a fall. As soon as the equity of this loan crosses 10%, we add it to our sample. Just as we did for the negative equity samples, we exclude loans that are not current when the borrower first crosses the threshold. Once the loan is in our sample, we observe default and prepayment behavior for the following 12 months. We construct samples similarly for 0% positive equity through 50%.

c) Mortgage Performance: Key Variables

Default

For the purpose of this paper, we define default to mean delinquency of at least 60 days within 12 months of crossing the equity threshold. Delinquency is indicated by number 2 in the 'CURRENT LOAN DELINQUENCY STATUS' in the Freddie Mac performance data.

Prepayment

Prepayment is when the borrower repays the loan in full voluntarily before the maturity date, again within 12 months of crossing the equity threshold. Prepayment is indicated by 'ZERO BALANCE CODE' 01 in the Freddie Mac performance data.

d) Mortgage Performance: Descriptive Statistics

Descriptive statistics for selected samples are provided in Table 2.1. Panel A provides descriptive statistics for mortgages upon reaching −30% equity. Panel B compares the interest rate, default rate and prepayment rate in each negative equity sample, and panel C provides descriptive statistics for mortgages upon reaching +30% equity. Most 30% negative equity loans were originated between 2004 and 2007 and reach negative equity between 2009 and 2011. Default rate increases with negative equity and prepayment decreases. Most 30% positive equity loans were originated between 2003 and 2005 and reach the positive equity threshold between 2008 and 2010.

e) Consumption data

For our consumption analysis, we use data from the Consumer Expenditure (CE) Survey. The CE includes quarterly data on household expenditures, income, and demographics, which are obtained quarterly via interviews with 30,000 consumer unites (households). Consumers are interviewed for four quarters, with new consumers being added every quarter. The CE contains comprehensive data on household expenditures divided into highly granular categories. For our analysis, we

Table 2.1: Descriptive Statistics

Panel A of this table provides descriptive statistics for all Freddie Mac, 30-year fixed rate, single family, owner-occupied mortgages upon reaching −30% equity. Refinanced mortgages were excluded. A loan is considered in default (prepaid) if it becomes at least 60-days late (is prepaid) within 1 year of reaching −30% equity. Credit information is from origination date. Equity for each borrower for every month estimated using the initial house value, outstanding mortgage balance and Zillows ZHVI Single-Family Homes House price index (HPI). Equity at time *t* for loan *i* originated at time 0 is given by $Equity_{it} = \frac{HouseValue_{i0} * HPI_t}{HPI_0}$. Panel B compares the interest rate, default rate and prepayment rate in each sample. These samples are constructed similarly to the −30% equity sample, but using other equity cutoffs instead of −30%. Panel C provides descriptive statistics for all Freddie Mac, 30-year fixed rate, single family, owner-occupied mortgages upon reaching +30% equity.

Panel A

Statistic	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)	N
Default	0.0539	0.2258	0	0	0	45,282
Prepayment	0.1092	0.3119	0	0	0	45,282
Mortgage Interest Rate	6.2422	0.4824	5.8750	6.2500	6.5000	45,282
National mortgage rate	6.1947	0.3582	6.0300	6.2400	6.4200	45,282
FICO	726.99	52.19	690	732	769	45,251
Mortgage payment	1,367	550	941	1,287	1,737	45,282
Loan-to-value	90.43	8.49	80	95	95	45,282
Debt-to-income	38.57	10.83	31	39	46	44,431
Loan amount	207,548	85,329	141,000	195,000	265,000	45,282
Origination year	2006.31	1.31	2005	2006	2007	45,282
Negative equity year	2009.76	1.20	2009	2010	2011	45,282

will focus primarily on total expenditure, but will also consider several more granular expenditure categories. For a list of categories we will use in our analysis, see Appendix Table A.1. The CE also contains detailed mortgage information, including mortgage amount, interest rate, month of issue, term, and type of loan.

For our analysis, we use the CE from Q1 2001-Q1 2007. Before Q1 2000 and after Q1 2007, the survey no longer includes data on refinancings and origination interest rate. Since origination interest rate is crucial to our experimental design, and current interest rate, which is reported, may not match the origination rate, we restrict ourselves to this period. We also exclude the year 2000 because some expenditure categories (specifically, vehicle purchase, transportation, and housing outlays) are not available in these (and earlier) surveys. Aggregate consumption is available for the year 2000 and results for aggregate consumption are unaffected by the inclusion of data from 2000. We consider only consumer units with 30-year fixed rate mortgages on their primary residence. We exclude refinanced and renegotiated mortgages by excluding all loan for which the interest rate at origination is different than the current rate, and by explicitly excluding all loans which are marked as renegotiated or refinanced. These maturity, refinancing, and renegotiation restrictions match those used for the default analysis. We include in the sample only borrowers with a mortgage origination in the last 2 years.[7] This is because older mortgages are likely to have been refinanced. While these refinancings should, in principle, be reported in the CE, they often are not: mortgage rates as much as 10 p.p. below the benchmark rate are sometimes reported for older mortgages. We also exclude from the samples all CU-

[7] Benmelech, Guren and Melzer (2017) also study consumption around the purchase of a home. They study within-household consumption around the purchase; our instrument is across-households.

quarters for which total expenditure is less than $100, or where income is reported to be $0.

Quarterly interviews are not conducted at the end of the quarter. The interviews are distributed equally across each month in a quarter and record expenditure in the previous months in the quarter. For example, an interview conducted in February records expenditure in January and an interview conducted in March records total expenditure in January and February. We therefore adjust the recorded expenditure to arrive at the quarterly expenditure. If the interview was conducted in the second (third) month of the quarter, we multiply all the expenditures by 3 (1.5).

2.4 Research Design

We exploit within-year variation in the national mortgage rate. Regardless of local economic conditions or borrower characteristics, loans originated when the national rate is higher will have a higher contracted interest rate. Consequently, these borrowers will have larger mortgage payments (assuming, as we will check, that loan size does not shrink much with higher rates) and greater liquidity problems. The key assumption behind this design is that borrower and loan characteristics for loans originated in May of one year look comparable to loans originated in, say, October of the same year.

a) Loan repayment

We attempt to establish the causal effect of mortgage payment on loan repayment. Most of our results will use a two-stage least squares regression, with a linear probability model of default (or in some cases, prepayment) as the second stage.

We use the linear probability model to avoid imposing structure on the correlation between the error terms in the first- and second-stages. The benchmark interest rate is the instrument. For most results, we will also report an ordinary least squares regression equivalent to the second stage, but using the actual mortgage rate rather than the instrumented rate. We will use the samples constructed in the previous section 3.B, so that each mortgage will appear in the sample at most once upon reaching a specified equity threshold. By conditioning the samples on home equity, we can flexibly account for a primary driver of mortgage default.

In the Freddie Mac mortgage data we are able to control directly for variables such as FICO score, loan-to-value, the natural log of the loan amount, and the debt-to-income ratio. Further, we account for region-year fixed effects as well as region-cohort fixed effects, where the region is given by the three-digit zip code (ZIP3), the year fixed effects correspond to the year in which a loan reached the equity threshold, and the cohort fixed effects correspond to the year of mortgage origination. The region-year fixed effects control for time-varying local economic conditions that can drive payment behavior, similar to the county-time fixed effects in Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017). For context, there are 3.5 times as many counties in the U.S. as there are three-digit zip codes, so we lose some granularity with the Freddie Mac data. When measured by land area, the 25th percentile Zip3 is actually smaller than the 25th percentile county, though the average Zip3 is 3.5 times bigger than the average county. The cohort-year fixed effects allows for changes in lending standards over the course of the housing boom.

More formally, the first-stage regression has the following specification:

$$ContractedRate_i = a + b_1 Benchmark\ Rate_i + X_i' b_2 + e_i$$

ContractedRate is interest rate on the mortgage, X is a matrix of control variables, including indicator variables for ZIP3 code/ origination year and ZIP3 code/ observation year, and *Benchmark Rate* is the average interest rate for all mortgages issued in the same month as mortgage i. From this first stage, we extract fitted values: $\widehat{Rate}_i$.

The second stage then has the following specification:

$$Y_i = \alpha + \beta_1 \widehat{Rate}_i + X_i' \beta_2 + \varepsilon_i$$

where Y is an outcome variables, either default of prepayment. β_1 is thus the coefficient of interest, with a positive sign indicating that borrowers with larger mortgage payments are more likely to default (or prepay).

b) Consumption

Our regression specifications are analogous in the Consumption Expenditure (CE) data. The primary difference is the availability of control variables. We use state-level indicators when controlling for region-year and region-cohort fixed effects. Household controls include the natural logarithm of family income as in Green, Melzer, Parker and Rojas (2018), family size, and the squared age of the head-of-household as in Melzer (2017).

2.5 Main Results – How do differences in rates affect behavior?

A) Mortgage default – OLS results

We consider the relationship between rates and default. We first present suggestive, rather than causal evidence. For this evidence, we turn to the samples constructed in Section 3. Loans appear in the sample for one year after reaching a specified level of equity and are considered to have defaulted if they become at least 60 days delinquent at any point during this year.

We model default using an OLS regression with default, a binary indicator, as the lefthand-side variable. We report the regression statistics in Table 2.2. The five columns use samples that include loans when they reach different negative equity thresholds for the first time. The table's five columns all use the same specification: FICO, LTV, log(amount), and DTI are included as controls, ZIP3 Code/Negative Equity Year and ZIP3 Code/Origination Year fixed effects are included, and standard errors are clustered by ZIP3 Code/Negative Equity Year.

The key variable is the interest rate, which measures the relative size of the monthly cash flow. For all negative equity thresholds, the coefficient on the interest rate is about 0.032. A 50 bp change in interest rate leads default to rise by about 1.6%. A similar relationship holds for all samples. Clearly this variable is important in predicting default—the implied change in default is 25-44% of the sample average default rate. However, it is likely that the size of the mortgage payment is not random—in particular, borrowers with worse credit will need to make higher

Table 2.2: Negative Equity, Default, OLS

This table examines the relationship between mortgage interest rate and default for negative equity loans. Regression statistics are reported for ordinary least squares regressions (linear probability model) with default as the dependent variable. Loans are put into the sample upon reaching the designated equity levels and defined to have defaulted if the become 60 days delinquent within the next year. Loans are put into the sample at most once, the first time they reach a given level of negative equity. Control variables are from origination date. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10% NE	20% NE	30% NE	40% NE	50% NE
	(1)	(2)	(3)	(4)	(5)
Interest Rate	0.033***	0.031***	0.034***	0.032***	0.032***
	(0.002)	(0.003)	(0.003)	(0.005)	(0.004)
FICO	-0.0003***	-0.0003***	-0.0004***	-0.0004***	-0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.005***	0.005***	0.005***	0.005***	0.003***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
log(Loan amount)	0.001	0.004	0.006*	0.010**	0.012**
	(0.002)	(0.002)	(0.004)	(0.005)	(0.005)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.117	0.106	0.117	0.118	0.122

payments. Thus, the OLS results are picking up both the effect of payment size and unobserved credit quality.

B) Mortgage default – Instrumental variable approach

We attempt to address the omitted variable problem by using the benchmark interest rate as an instrument for contracted rate. The benchmark interest rate mechanically affects the contracted rate, but it is not clearly related to default in any other way. In the long run, benchmark interest rate and average creditworthiness of borrowers may be correlated, but given that we only consider within-ZIP3/Origination Year variation, this should be absorbed in the fixed effects. We use a two stage least squares with the monthly benchmark interest rate (mean interest rate for all 30-year, single-family, Freddie Mac loans issued in a given month) as the instrument.

First stage results

In Table 2.3, we report regression statistics for the first stage regression, in which the contracted rate is regressed on the benchmark interest rate. We use the same samples as in Table 2.2 (equity cutoffs of −10%, −20%, −30%, −40%, and −50%), as well as the same set of control variables and fixed effects, and the same standard error clustering. The coefficient on benchmark interest rate is 0.75 for the −30% equity sample. This means that borrowers' contracted rate do not quite move 1:1 with the benchmark interest rate, likely because there is within-month variation in the benchmark rate that creates noise. Negative equity levels from -10% to −40% have similar coefficients of

Table 2.3: Negative Equity, First Stage

This table examines the relationship between 30 year benchmark mortgage rate and mortgage interest rate. Regression statistics are reported for an ordinary least squares regression with mortgage interest rate as the dependent variable and 30 year benchmark mortgage rate as the independent variable. This regression is the first stage of a two stage least squares, in which monthly mean of 30-year fixed rate mortgage rate is an instrument for mortgage interest rate. Loans are put into the sample upon reaching the designated equity levels. Loans are put into samples at most once, the first time they reach a given level of negative equity. Control variables are from origination date. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10% NE	20% NE	30% NE	40% NE	50% NE
	(1)	(2)	(3)	(4)	(5)
Mortgage Rate	0.725***	0.737***	0.751***	0.759***	0.888***
(US Average)	(0.006)	(0.008)	(0.010)	(0.012)	(0.013)
FICO	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.007***	0.008***	0.008***	0.009***	0.010***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
log(Loan amount)	-0.185***	-0.182***	-0.181***	-0.181***	-0.178***
	(0.003)	(0.004)	(0.005)	(0.007)	(0.008)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.67	0.592	0.548	0.544	0.538

0.72-0.76, whereas the most extreme negative equity level of -50% has a larger coefficient of 0.88.

Generally speaking, a 50 bp change in the benchmark rate causes the mortgage payment to change by less than 5% of the average mortgage payment. Given that the within-year benchmark rate has a standard deviation of 24bps, we are dealing with relatively small changes in payment size. The other coefficients behave as we would expect—interest rates decreases with FICO, and increases as the size of the loan increases (both in absolute terms and relative the value of the house and the income of the borrower).

Second stage results

We now turn to the second stage regression, a regression of default on the instrumented interest rate. With this result, we attempt to determine the causal effect of payment size on default. This second stage is a linear probability model identical to that in Table 2.2, except that it uses only the variation in interest rate coming from the benchmark interest rate to identify the relationship between rates and default.

We report the regressions statistics in Table 2.4. The coefficient on the interest rate varies between .018 and .024, depending on the sample used. This means that a change in the national rate of 50 bp (which leads the predicted interest rate to change from 36 bp to 44 bp) causes the default probability to rise by 66 bp to 88 bp. The constancy of the coefficient is striking relative the large differences in default rates across the samples. As a fraction of the average default probability, a 50bp increase in

Table 2.4: Effect of Interest Rate on Default, Negative Equity Samples, IV

This table examines the causal relationship between mortgage interest rate and default for negative equity loans. Regression statistics are reported for instrumental variables regressions where default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is used as the instrument. Loans are considered to have defaulted if the become at least 60-days delinquent within 1 year after reaching the specified level of equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10% NE	20% NE	30% NE	40% NE	50% NE
	(1)	(2)	(3)	(4)	(5)
Interest Rate	0.021***	0.024***	0.018***	0.018**	0.018***
	(0.003)	(0.005)	(0.007)	(0.009)	(0.006)
FICO	-0.0003***	-0.0004***	-0.0004***	-0.0004***	-0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.005***	0.005***	0.005***	0.005***	0.003***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
log(Loan amount)	-0.001	0.003	0.003	0.007	0.009*
	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes
Cond. F. Stat	2,500	1,544	1,192	816	978
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.088	0.076	0.083	0.087	0.093

the benchmark rate causes default to rise between 10% and 19%; the -10% and -20% equity samples experience the largest increase in default probability relative the sample average (because the sample average is low, while the marginal effect of interest rates is relatively constant across samples).[8] The relative constancy of the effect across samples suggests that the effect is driven by liquidity constraints rather than strategic considerations. Borrowers with -50% equity have much different incentives for strategic default than borrowers with -10% equity, but both react similarly to a change in their mortgage payment. Similar to the first-stage, the coefficients on the control variables generally behave as we would expect. Finally, the Sanderson and Windmeijer (2016) conditional F-statistic, a test of weak instruments, is substantially higher than conventional critical values.

Figure 2.2 illustrates a simple version of our two-stage results graphically for the -30% equity sample. Panel A plots the first-stage results: we absorb ZIP3*(year of origination) and ZIP3*(year of negative equity) from both the contracted rate and the benchmark interest rate. We plot the residuals, which we denote "FE-adjusted rate" and "FE-adjusted IV" against each other and fit a kernel regression. There is a strong and precise relationship between the benchmark rate and the contracted rate. Most of the data is within the middle 50 basis points, for which we see an increase in the contracted rate of about 25 bp.

[8] These coefficients are substantially below the OLS results reported in Table 2.2, suggesting that raw mortgage payments reflect credit risk.

Panel B of Figure 2.2 plots a version of our second-stage. The y-axis is the default probability after absorbing our fixed effects and adding back the sample average, and the x-axis is the effect of our instrument on the interest rate. More precisely, the x-axis is the predicted impact of our instrument on the contracted rate from a first-stage regression of contracted rate on our instrument and fixed effects; we standardize this quantity by its standard deviation. We see that a one standard deviation move in our instrument around 0 leads to an increase in default of 31.5 bps, or 6% of the sample average. A two-standard deviation move in the instrument has a proportional impact—defaults fall by about 63 bp. This figure shows our basic result without any controls for the credit quality of individual loans beyond the fixed effects.

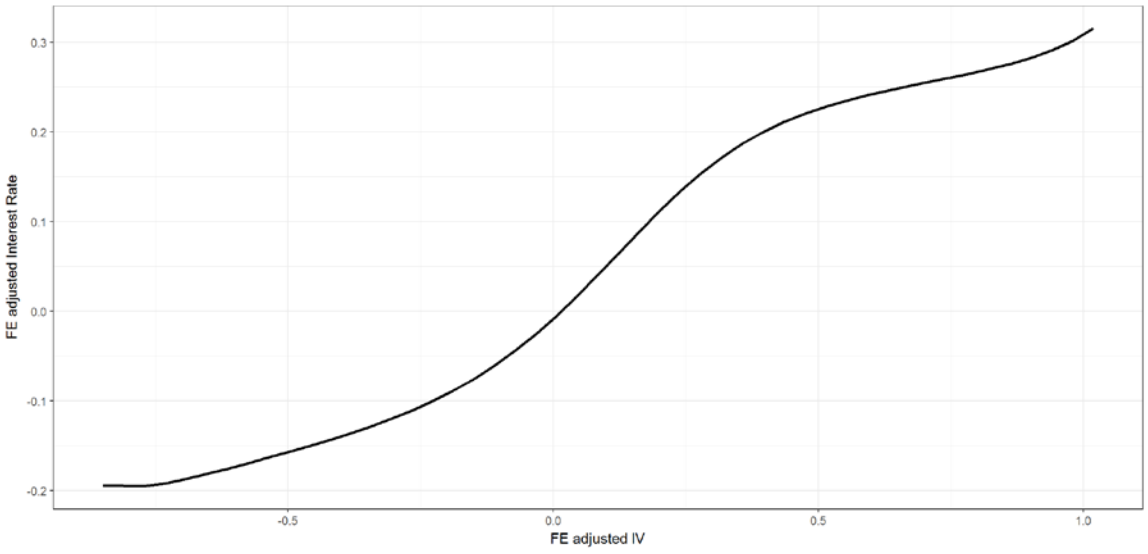
Our design is not limited to negative equity loans. If payment size is important for default, it may matter even for borrowers with positive equity, particularly if there are liquidity constraints. Table 2.5 shows our results for homes with between 0% and 50% positive equity. To make our samples comparable to the negative equity table, we require that home values have fallen by at least 10% before they cross a given positive equity threshold and are added to our sample.

Loans with less than 20% positive equity have similar default behavior as loans with negative equity. The coefficient on interest rate is around 0.015—indistinguishable from loans with negative equity despite the much smaller average default probability for positive equity loans of around 2%. Further, the sensitivity of default to interest rates becomes substantially weaker around 20% positive equity, which is a typical

Figure 2.2: Graphical Illustration: 30% Negative Equity Sample

This figure shows the relationship between instrumented mortgage interest rate and default rate for the 30% negative equity sample. Panel A shows the relationship between the instrument (30 year benchmark mortgage rate) and the endogenous variable (mortgage interest rate). Both variables are adjusted for ZIP3 * Negative Equity Year and ZIP3 * Origination Year fixed effects. Panel B plots the Nadaraya-Watson kernel regression estimates (solid line) of default versus instrumented mortgage interest rate (normalized by a standard deviation). ZIP3 * Negative Equity Year and ZIP3 * Origination Year fixed effects are included in the instrumented mortgage interest rate regression, but both the x-and y-axis are fixed-effect adjusted. The bandwidth in panel A is 0.19, and 0.14 in panel B.

Panel A



Panel B

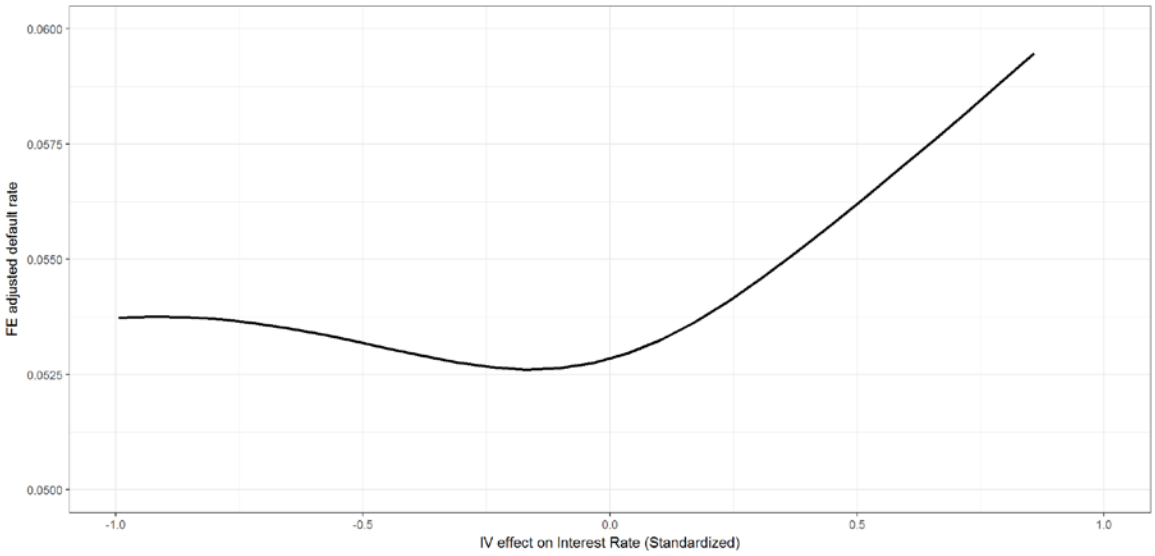

0.0600
0.0575
0.0550
0.0525
0.0500
FE adjusted default rate
-1.0
-0.5
0.0
0.5
1.0
IV effect on Interest Rate (Standardized)

Table 2.5: Positive Equity, Default, IV

This table examines the causal relationship between mortgage interest rate and default for positive equity loans. Regression statistics are reported for instrumental variables regressions. Default (60-day delinquency within 1 year of reaching the specified level of equity) is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is the instrument. Loans are required to be above the specified levels of equity and then cross below the specified threshold for the first time. Standard errors are clustered at ZIP3 Code * Positive Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	0% PE	10% PE	15% PE	20% PE	25% PE	30% PE	50% PE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Interest Rate	0.015***	0.011***	0.015***	0.007**	0.004	0.007	0.00003
	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.005)
FICO	-0.0003***	-0.0002***	-0.0003***	-0.0002***	-0.0002***	-0.0003***	-0.0002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.003***	0.002***	0.002***	0.001***	0.001***	0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log(Loan amount)	-0.002	0.0005	0.001	-0.0001	-0.0002	-0.0004	-0.002
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Debt-to-income	0.001***	0.0003***	0.0003***	0.0002***	0.0002***	0.0002***	0.0001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	129,645	120,611	88,672	73,542	62,825	54,488	16,803
Adjusted R^2	0.083	0.066	0.069	0.059	0.056	0.058	0.051

threshold for easy refinancing. These facts suggest that liquidity constraints are driving the defaults stemming from small changes in mortgage payments.

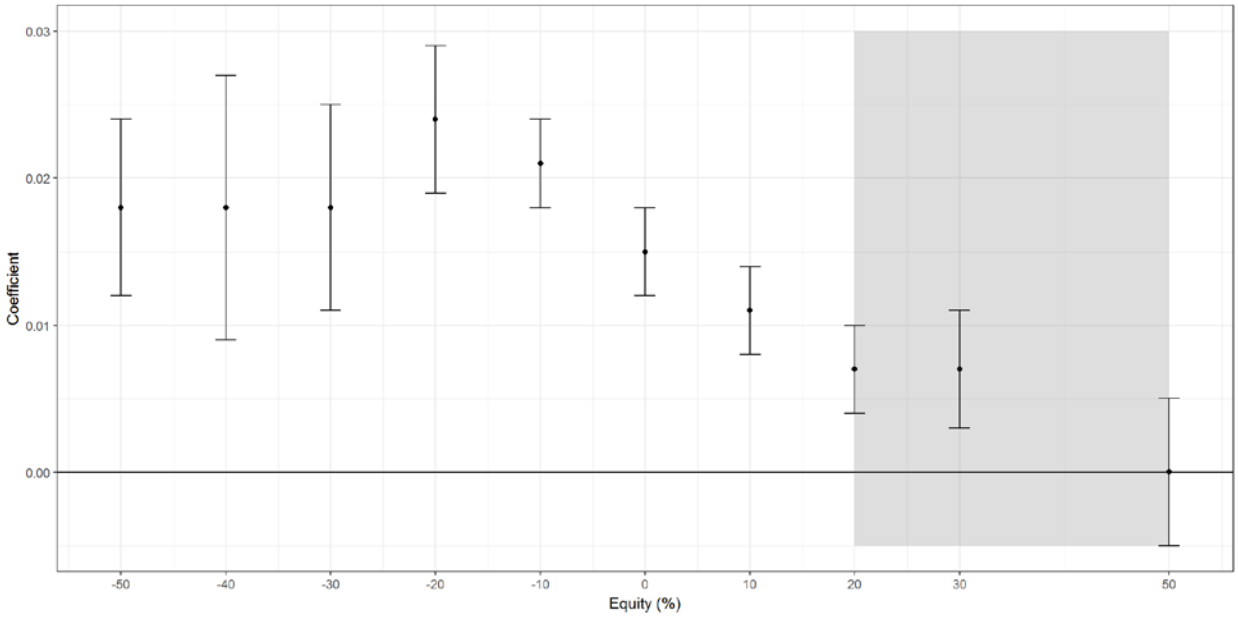
Figure 2.3 graphically illustrates our results for a range of samples. For a given level of equity, Figure 2.3 plots the second-stage regression coefficient on the contracted interest rate along with the standard error. We see that a one standard deviation change in our instrument has a similar effect on default for all of our negative-equity samples. The marginal effect is roughly constant across negative-equity samples—a one standard deviation move in our instrument can explain between 5-10% of each sample's average default.

While our instrument continues to predict default for the positive equity samples, the coefficients shrink toward zero. The drop becomes particularly noticeable around 20% positive equity, where many borrowers are likely able to refinance.

To conclude this section, we perform balancing tests to address concerns about omitted variables. The primary threat to our identification is that better quality borrowers happen to buy when national rates are low, in which case our design may be picking up differences in credit quality rather than payment size. While we think it is difficult to time the market with precision within a given year, it is valuable to examine how observable borrower characteristics correlate with our instrument. To do this, we perform balancing tests by re-running our two-stage design as follows: in the first-stage, we regress the contracted rate on the benchmark rate and our fixed effects, but not the other control variables. The second stage regresses each control variable on

Figure 2.3: Impact of interest rate on default

This figure plots the coefficient estimates of the instrumental variable regression for each equity threshold where, mortgage interest rate is the independent variable being instrumented for and benchmark 30-year mortgage rate is used as the instrument. Error bars represent standard errors.



our fixed effects and the predicted contracted rate from the first-stage. This method allows us to see how the control variables correlate with the instrument.

Table 6 shows our balancing tests for loan-to-value, log(Loan amount), FICO score, and debt-to-income. For the first three variables, we see that our instrument for higher mortgage payments is correlated with better credit quality—a 1pp increase in

Table 2.6: Balancing tests for control variables

This table examines the relationship between instrumented mortgage interest rate and the control variables used in previous regressions for 30% negative equity sample. Estimates are reported for instrumental variables regressions with the control variables from our main IV regressions as dependent variables. The first stage of these regressions includes benchmark mortgage rate as the instrument, and fixed effects (ZIP3 * Neg. Equity Year and ZIP3 * Origination Year) as RHS variables. The second stage includes only fixed effects and the instrumented mortgage interest rate as RHS. Loans are included in the sample upon reaching −30% equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

Dependent Var	Coefficient	Std. Err	Observations	Adjusted R^2
Loan-to-value	-1.621***	(0.294)	44,401	0.741
log(Loan amount)	-0.032**	(0.013)	44,401	0.376
FICO	1.905	(1.631)	44,401	0.029
Debt-to-income	1.643***	(0.301)	44,401	0.025

the interest rate is associated with a reduction in loan-to-value of 1.6 (less than 2% of the sample average), a 3.2% reduction in loan balance, and 2 extra points on the FICO score, all of which work to reduce the probability of default. Particularly important is the reduction in loan-to-value of -1.62, which can reduce default by 81 basis points according to our second-stage coefficient. This suggests that our instrument may be an underestimating the effect of payment size on default because the instrument is correlated with better credit.

Debt-payment-to-income (D/I) is positively correlated with our instrument, as expected. When the interest rate increases by a greater proportion than the loan balance falls, then the monthly debt payment will rise, driving D/I.[9] When we control for debt-to-income in our regressions, then, the interest rate variable will pick up the incremental effect of the higher mortgage payment on default beyond what is already incorporated in the higher debt-to-income. In any event, a 50 bp increase in our instrument causes debt-to-income to rise by 0.62 (0.5*0.75*1.64), which causes the predicted default probability in the second stage to rise by under 10 bp—a relatively small amount compared to the effect of our instrument on default.

[9] Back of the envelope calculations reveal the mechanical effect of our instrument on D/I. A two standard deviation move in our instrument (50bps) causes D/I to move by 0.62 (1.6*0.5*0.75), which is 1.6% of the sample average. The same move in the instrument leads to an increase of 37.5bp (50*0.75) in the interest rate and a lower debt balance of-1.2% (-3.2*0.5*0.75)—combined, the debt payment (D) should increase by a little under 3%. The fact that the implied increase in D is about 3%, but the change in D/I is 1.6%, the instrument is likely correlated with higher income—again making it harder to find an effect on default.

C) Deleveraging through full prepayments

We follow Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017) and next turn to the effect of interest rates on voluntary deleveraging and consumption. Our primary measure of deleveraging is whether the borrower prepays the remainder of the mortgage balance. In the data, this prepayment could be outright or it could be through a refinancing—we are unable to distinguish the two. Regardless, given the difficult refinancing environment and the negative equity of the sample's borrowers, any prepayment is likely to entail a reduction in leverage.

Interest rates have an ambiguous effect on deleveraging. On the one hand, a higher interest rate at origination gives the borrower greater incentive to refinance. The optimal refinancing decision is typically characterized as an interest rate threshold—once the market rate drops far enough below the origination rate, then the borrower should refinance. Given an equal drop in interest rates, the borrower with a higher rate at origination will want to refinance first. On the other hand, Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017) find that borrowers use reductions in interest rates to delever. That is, it may be that constrained borrowers want to delever, but only those with lower interest rates will be able to afford to.

The economic effects are important during a recession. When negative equity mortgagors prepay their mortgage, they must delever and therefore consume less. Positive equity mortgagors, on the other hand, can extract equity from a prepayment, which can stimulate consumption. Table 2.7 uses our instrument for payment size to predict prepayments for negative equity loans.

Table 2.7: Negative Equity, Prepayment, IV

This table examines the causal relationship between mortgage interest rate and prepayment for negative equity loans. Regression statistics are reported for instrumental variables regressions. Prepayment (within 1 year of reaching the specified level of equity) is the dependent variable, mortgage interest rate is the independent variable being instrumented for by benchmark 30-year mortgage rate. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10% NE	20% NE	30% NE	40% NE	50% NE
	(1)	(2)	(3)	(4)	(5)
Interest Rate	0.047***	0.039***	0.037***	0.047***	0.041***
	(0.006)	(0.008)	(0.010)	(0.011)	(0.011)
FICO	0.0004***	0.0004***	0.0004***	0.0003***	0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	-0.003***	-0.003***	-0.003***	-0.003***	-0.002***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
log(Loan amount)	0.107***	0.097***	0.099***	0.083***	0.077***
	(0.004)	(0.005)	(0.006)	(0.008)	(0.008)
Debt-to-income	-0.001***	-0.001***	-0.001***	-0.0004**	-0.0005**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.087	0.104	0.12	0.118	0.118

We see that a 50bp increase in the benchmark rate leads to an increased prepayment probability of 1.4% to 1.8% depending on the sample. Relative the sample average prepayment rate, the effect varies from 11.5% for the loans with -10% and -20% equity to 19% for loans with

-50% equity. Our instrument therefore has power to potentially explain a large range of household decisions.[10]

Table 2.8 replicates Table 2.7, but for the samples of homes with positive equity. We see that our instrument has power for homes that have positive equity and presumably face fewer financial constraints. In fact, the coefficients in Table 2.8 are higher than in Table 2.7, which suggests that positive equity borrowers are better able to use their improved position to prepay and avoid relatively high interest rates.[11] While the sensitivity to interest rates is higher for positive equity borrowers, the level of prepayments is also higher, so the effect as a fraction of the sample average is very

[10] Since our instrument predicts prepayments, it is possible that high-quality borrowers prepay before they reach negative equity, which could contaminate our default regressions in the previous section because we condition the sample on reaching negative equity. We note that the number of prepayments that can be attributed to our instrument is small—it would remove 1-2% of the sample that is unlikely to default, which could scale our default coefficient by about 1/0.98. In untabulated results, we re-run the default regression on all loans but code refinances as non-defaulters and find equally strong results.

[11] Directly comparing our positive equity samples with our negative equity samples is not straightforward. We require that the positive equity homes experience a fall of at least 10% in value, which is likely a lower bound in the drop in home values for the negative equity samples. The fact that the coefficients are relatively stable across all negative equity and positive equity homes, however, suggests that the exact dynamics of the price path are not of first-order importance for our results.

Table 2.8: Positive Equity, Prepayments, IV

This table examines the relationship between mortgage interest rate and prepayment for positive equity loans. Regression statistics are reported for instrumental variables regressions. Prepayment (within 1 year of reaching the specified level of equity) is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is the instrument. Loans are required to be above the specified levels of equity and then cross below the specified threshold. Standard errors are clustered at ZIP3 Code * Positive Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	0% PE	10% PE	30% PE	50% PE
	(1)	(2)	(3)	(4)
Interest Rate	0.067***	0.085***	0.056***	0.073***
	(0.008)	(0.008)	(0.010)	(0.016)
FICO	0.0005***	0.0005***	0.0005***	0.0004***
	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	-0.002***	-0.002***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.001)
log(Loan amount)	0.123***	0.133***	0.115***	0.132***
	(0.004)	(0.005)	(0.006)	(0.010)
Debt-to-income	-0.001***	-0.001***	-0.001***	-0.0004
	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes
Observations	129,645	120,611	54,488	16,803
Adjusted R^2	0.091	0.095	0.127	0.126

similar to the negative equity sample. A 50 bp move in the national rate can lead to higher prepayments of 13-18% of the sample average.

So far, we have shown that the interest rate at loan origination has important consequences during house price declines. In particular, higher interest rates cause higher defaults, but also induce higher prepayments across a wide range of home equity levels. For a fuller picture of the effect of interest rates over the business cycle, we apply our design to consumption during the house price boom.

D) Interest rate at origination and consumption

We now study the consumption response to lower interest rates. The empirical design is similar to the default analysis. The main differences, as stated in section 3.E, are 1) the consumption data covers 2001-2007[12], whereas the default analysis conditioned on negative equity (which occurred after 2008), and 2) We look at consumption for households that recently purchased a home (similar to Benmelech, Guren and Melzer (2017)). Table 2.9 reports summary statistics for the consumption data. We see that the mortgage balance and payments are 25% smaller in the CE than in the Freddie Mac data; this difference is likely driven by the earlier loan originations in the CE sample. Most loans in the CE sample are originated between 2001 and 2004, whereas loans in the Freddie Mac sample were originated from 2005 to 2007 after house prices had appreciated. It is also likely that the Freddie Mac borrowers are of higher

[12] The CE survey does not collect the interest rate at loan origination after 2007Q1, which is critical to our design. Rather, the survey only collects the interest rate as of the interview. Given the halving of interest rates from 2007 thru 2013Q1, the current interest rate at the interview date is a noisy proxy for the interest rate at origination.

Table 2.9: Descriptive Statistics: CE Survey

This table provides descriptive statistics for mortgage characteristics, household characteristics and main expenditure categories in the regression sample—borrowers with a 30 year fixed rate mortgage originated in the last 2 years since the interview year, who did not refinance and interview quarter from 2001 quarter 1 to 2007 quarter 1.

Statistic	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)	N
Mortgage interest rate	6.65	1.09	5.88	6.50	7.35	4,782
National mortgage rate	6.43	0.80	5.81	6.24	7.01	4,782
Mortgage amount	149,630	90,363	90,000	132,000	189,000	4,782
Non-durable exp.	5,273	3,541	3,240	4,460	6,235	4,782
Durable exp.	1,473	3,404	195	891	1,680	4,782
Services exp.	1,063	2,376	165	510	1,243	4,782
Mortgage payment	3,766	2,394	2,340	3,300	4,500	4,782
Total exp.	15,069	12,534	8,460	11,836	17,170	4,782
Principal repayment	30	230	0	0	0	4,782
Origination year	2002.71	1.94	2001	2003	2004	4,782
Interview year	2003.71	1.77	2002	2004	2005	4,782
Family income after tax	70,983	49,315	40,095	61,279	89,001	4,782
Family size	2.96	1.52	2	3	4	4,782
Age of ref. person	39.83	11.82	31	38	47	4,782

quality than the population and thus able to secure larger mortgages. The median quarterly expenditure for a household is $11,836; the plurality of this consists of nondurable consumption ($4,460), followed by reported mortgage payments ($3,300), durables ($891) and services ($510).

Table 2.10 applies our instrument to these spending categories. Column 1 shows the first-stage. The coefficient on the benchmark rate is similar to the coefficients in Table 2.4, though slightly smaller. If the CE respondents do not perfectly recall the beginning month of their mortgage, then the first stage will have more measurement error than in the Freddie Mac sample (where we know the starting month exactly), leading to a lower estimate.[13] The conditional F-stat is 11.8, so the national mortgage rate still strongly predicts the reported rates in the CE sample. A two standard deviation move in our instrument is roughly 50 basis points. Such a movement leads to an increase in the reported interest rate of 29 basis points—this will be our benchmark in interpreting the consumption results.

Columns 2-9 report coefficients for each spending category, alternating from the OLS specification to the IV model. The primary omitted variable in the OLS specification is credit quality—more risky borrowers will have higher interest rates. In columns 2 and 3, we see that higher interest rates lead to lower spending on non-durables in both specifications. In the OLS specification, a 30.6 bp move in the interest rate corresponds to a drop of 1.6% in nondurables; a similar move in the IV specification (which corresponds to a 50bp change in the national rate) leads to a drop of 2.0% in nondurables. Durables are not statistically impacted in either the OLS or IV specifications, though the IV points to an increase in durables while the OLS points in

[13] It is also possible that the national mortgage rate, which is an average of Freddie Mac backed mortgages, is not as strongly correlated with the interest rates of non-Freddie mortgages.

Table 2.10: Impact of Interest Rate on Consumption

This table examines the causal relationship between mortgage interest rate and various expenditure types for borrowers with a 30 year fixed rate mortgage originated in the last 2 years since the interview year, who did not refinance and interview quarter from 2001 quarter 1 to 2007 quarter 1. Column (1) reports regression statistics for an ordinary least squares regression with mortgage-to-loan (multiplied by 100,000 to make coefficients readable) as the dependent variable and 30 year benchmark mortgage rate as the independent variable. This regression is the first stage of the IV regressions reported in this table. Columns (2), (4), (6), and (8) report regression statistics for ordinary least squares regression of each expenditure category on mortgage-to-loan. Columns (3), (5), (7), and (9) report regression statistics for instrumental variable regressions where each expenditure category is the dependent variable, mortgage interest rate is the independent variable being instrumented for and benchmark 30-year mortgage rate is used as the instrument. Standard errors are clustered at household and interview year and reported in parentheses below coefficient estimates. *, **, *** denote two-tailed significance at the 10%, 5% and 1% levels, respectively.

	Interest Rate	log(Non-durable exp.)		log(Durable exp.)		log(Services exp.)		log(Principal reduction)		log(Mortgage payment)		log(Total exp.)	
	First Stage	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Interest Rate		-0.055***	-0.070*	-0.058	0.342	-0.234***	-0.690**	0.065	0.371***	-0.069***	0.145	-0.051***	0.039
		(0.006)	(0.036)	(0.075)	(0.211)	(0.059)	(0.277)	(0.044)	(0.099)	(0.022)	(0.122)	(0.007)	(0.071)
Mortgage Rate	0.580***												
(US Average)	(0.084)												
log(Family income)	-0.114***	0.216***	0.214***	0.418***	0.470***	0.530***	0.471***	0.062	0.102**	0.183***	0.211***	0.297***	0.309***
	(0.029)	(0.026)	(0.025)	(0.046)	(0.068)	(0.077)	(0.076)	(0.038)	(0.046)	(0.028)	(0.039)	(0.026)	(0.026)
Family size	0.062***	0.093***	0.094***	0.112***	0.086***	0.194***	0.224***	0.032	0.012	0.049***	0.035***	0.066***	0.060***
	(0.015)	(0.007)	(0.008)	(0.029)	(0.033)	(0.033)	(0.028)	(0.030)	(0.029)	(0.011)	(0.012)	(0.007)	(0.009)
Age of ref. person^2	0.000	0.00003**	0.00003**	-0.0002***	-0.0002***	0.0003***	0.0003***	0.00005*	0.00005*	-0.00001	-0.00001	0.00002*	0.00002
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
State * Org. Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * Int. Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cond. F Stat			11.8		11.8		11.8		11.8		11.8		11.8
Observations	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782	4,782
Adjusted R^2	0.622	0.372	0.372	0.303	0.283	0.264	0.247	0.44	0.405	0.501	0.44	0.408	0.397

the opposite direction. Both specifications show a drop in services after a 30.6 bp move in the rate—6.8% in the OLS and 20% in the IV. In dollar terms, a higher interest rate can account for the changes in consumption spending. At the median of the distribution, a 50 bp increase in the national rate leads to an increase in the implied mortgage payment of $320 per year (a 3.3% change). This change in interest rates corresponds to an estimated drop in nondurable spending by $360, an increase in durables by an (imprecise) $351, and a drop in services of $400, for a total change in consumption of $409—roughly in line with the increased mortgage payment.

Columns 8-11 of table 2.10 turn to mortgage payments rather than consumption. Columns 8-9 examine principal reductions, defined as mortgage payments in excess of the scheduled payment. Our IV estimate shows an increase in principal reductions for the borrowers with higher interest rates at origination. Economically, the effect is small given that the sample average for principal reductions is only $30 per quarter. The effect is at least consistent with our results on prepayments, where borrowers with higher interest rates are more likely to prepay their mortgage. Further, the economic magnitude of prepayments is in line with Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017)—for conforming ARMs, they find an increase of $12-$17 per month in mortgage prepayments after a 2-3 p.p. cut in interest rates. We find that a 3 p.p. cut in rates leads to $33.30 (3*30*0.371) extra per quarter in mortgage prepayments.

The dependent variable in columns 10-11 is the log of the total reported mortgage payment. The OLS coefficient in column 10 is particularly instructive when comparing the OLS and IV coefficients for consumption in columns 2-7. Column 10 shows that borrowers with a higher interest rate also have a 6.9% lower mortgage payment; this is consistent with reduced borrowing capacity for higher risk/interest

borrowers. The coefficient also suggests a reason why our IV estimates for non-durable and service spending are lower than the OLS estimates. Worse borrowers take smaller mortgages, which attenuates the effect of interest rates on consumption. To the extent our IV cleans the estimates of unobserved credit quality, then the IV estimates of consumption spending in columns 3 and 7 will not be biased upwards by the endogenous choice of a smaller mortgage. Column 11 reports the IV estimate of interest rates on mortgage payment. A 1 p.p. move in interest rates should mechanically move mortgage payments on 30-year fixed rate mortgages by about 10%; our IV estimate is 14.5%, though the standard error is 12%. The large standard errors in column 11, like the coefficient in column 10 discussed above, imply that there is substantial variation in the choice of mortgage size and this reduces the power of our IV.

Column 12 reiterates the endogeneity of the contracted interest rate. We see that borrowers with higher interest rates spend 5.1% less for a 1p.p. increase in rates; the magnitude of this effect is very similar to the effect for non-durable consumption (the largest category of expenditure) as well as mortgage payments (the second largest category of expenditures). High interest borrowers appear, then, to have worse prospects, even after controlling for income. We see no statistically significant relation between our instrument and total expenditures in column 13. Column 13 therefore alleviates concerns that our instrument is correlated with an omitted variable that affects the overall credit quality or income of borrowers.

Finally, we perform balancing tests on household characteristics. We put each control on the left of our second-stage IV regressions (where the only controls are the fixed effects and the instrument). The results are reported in table 2.11.

We find no statistically significant relationship between our instrument and three common control variables: squared age of the reference person, family size, and

Table 2.11: Balancing regressions – Household characteristics on mortgage-to-loan

This table examines the relationship between instrumented mortgage interest rate and control variables used in previous regressions. Estimates are reported for instrumental variable regressions with the control variables from our consumption IV regression (See Table 2.13) as dependent variables. First stage of these regressions includes benchmark mortgage rate as the instrument, and fixed effects (State * Origination year and State * Interview year) as RHS variables. The second stage includes only fixed effects and the instrumented mortgage interest rate as RHS. Standard errors are clustered by household and interview year.

Dependent Var	Coefficient	Std. Err	Observations	Adjusted R^2
log(Family income)	-0.379***	(0.142)	4,782	0.23
Age of ref. person^2	-1.578	(2.055)	4,782	0.33
Family size	0.130	(0.259)	4,782	0.31
log(Mortgage amount)	0.191	(0.117)	4,782	0.34
Org . Int ≠ Curr. Int	5.927	(7.539)	5,209	0.41

log(mortgage amount). We do, however, find a correlation between our instrument and log(family income)—a 50bp increase in our instrument corresponds to a drop in family income of 17%. It is therefore important to control for income in our regressions. Further, the magnitude of the correlation would yield a smaller effect on expenditures than our instrument, which alleviates concerns about omitted variables being correlated with our instrument.

As a final check, we expand our sample to all households that purchased a home in the previous year (n=5209) as opposed to those who also still have the same interest rate as at origination. One possible issue is that the households who changed their interest rate are of a different quality than those who did not. Further, the motive to refinance is tied to the initial

interest rate, and thus may be correlated with our instrument. The last row of Table 2.11 shows that our instrument is not correlated with the decision to change the interest rate within two years of buying the home.

2.6 Heterogeneity in default: liquidity constraints or strategic default?

We have shown that relatively small changes in mortgage payments can have a significant effect on default. While there is suggestive evidence that the effect is driven by liquidity constraints (the effect is relatively constant across a wide range of borrowers with different levels of negative equity, but the effect goes away when a borrower has 20% equity and can refinance), there could still be a strategic motive for default. Because we are looking at fixed-rate mortgages, a higher rate at origination

leads to a future stream of higher payments; the present value of this stream of payments acts to further reduce a borrower's equity and can lead to strategic default.

Table 2.12 examines financing constraints by reporting results separately for each quartile of FICO score. The analysis is identical to prior tables, but is performed on each FICO subsample and for each negative equity sample. Generally speaking, we see that the effect is larger for lower-credit borrowers. For the sample of loans with 10% negative equity, the lowest quality borrowers are three times as responsive to interest rates as the highest quality borrowers, and this difference is highly statistically significant. We lose observations and statistical power with increasing negative equity, but the coefficient for the worst quartile is around 0.02 higher than the best-quality quartile for loans with 20-30% negative equity. Given that the average effect is 0.02, the heterogeneity is economically significant. Further, there is monotonicity in the coefficient for these samples as we move from low to high-quality credit. These results imply that lower-FICO borrowers have higher borrowing costs and limited access to credit (as in Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017)), making it more difficult to make slightly higher interest payments. The heterogeneity becomes weaker for the loans with 40-50% negative equity. This is consistent with strategic motives becoming more important (relative financing constraints) as negative equity increases, though the imprecision of the estimates limits any conclusions.

To study strategic behavior, we split our sample by state foreclosure law. In particular, we look at two components of foreclosure law: whether the state has judicial foreclosure and whether the state has recourse mortgages. Judicial foreclosure states

Table 2.12: Negative Equity, Default, IV, FICO Split

This table examines the causal relationship between mortgage interest rate and default for 20% negative equity loans with different FICO score levels. This table reports regression statistics for an instrumental variables regression. Default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is the instrument. The sample is split into FICO score quartiles, and 2SLS regressions are performed separately on each quartile. Median FICO score of each sample is given in the column heading. Loans are considered to have defaulted if they become 60 days delinquent within 1 year of reaching −30% equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	Q1 ~ 662	Q2 ~ 712	Q3 ~ 752	Q4 ~ 787
	(1)	(2)	(3)	(4)
10% NE	0.037***	0.022***	0.015***	0.012***
	(0.008)	(0.006)	(0.005)	(0.004)
20% NE	0.035***	0.026***	0.011	0.018***
	(0.012)	(0.009)	(0.008)	(0.007)
30% NE	0.028*	0.022*	0.022*	-0.002
	(0.016)	(0.013)	(0.012)	(0.009)
40% NE	0.027	0.023	-0.005	0.025*
	(0.022)	(0.018)	(0.016)	(0.014)
50% NE	0.014	0.017	0.009	0.030***
	(0.016)	(0.013)	(0.013)	(0.011)

are preferred by mortgagors because they require a lender to go through the court system when foreclosing. This requirement slows the foreclosure process, allowing mortgagors more time to make back payments and/or live in the home while looking for a new residence. Mortgagors in judicial states therefore have more leverage and have greater incentive to strategically default. Similarly, borrowers in non-recourse states have greater incentive to strategically default. If a state has non-recourse laws, then the mortgage lender cannot seize the borrower's other assets in foreclosure.

Table 2.13 shows no clear relationship between state foreclosure laws and the sensitivity of default to payment size for loans with -30% equity. The coefficient of interest bounces around the coefficient for the full sample (0.018) from Table 2.3, and is not statistically different for any subsample. Further, the direction of the effect is inconsistent. Judicial states are more favorable to mortgagors but do not exhibit a higher sensitivity to interest rates than non-judicial states; likewise, non-recourse states are more favorable to mortgagors and yet they are weakly less sensitive to payment size than recourse states. In short, there is little direct evidence that our instrument is picking up strategic incentives for default. This non-result is broadly consistent with the literature, which finds that consumers' sensitivity to strategic default incentives is moderated by non-economic factors (Guiso, Sapienza and Zingales (2013) and Bhutta, Dokko and Shan (2017))—the small effect of our instrument is likely second-order to these factors.

Table 2.13: Negative Equity, Default, IV, Judicial and Recourse State Split

This table examines the causal relationship between mortgage interest rate and default for 30% negative equity loans for Judicial foreclosure states vs non-judicial foreclosure states and recourse states vs non-recourse states . This table reports regression statistics for an instrumental variables regression. Default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is the instrument. 2SLS regressions are performed separately on each quartile. Loans are considered to have defaulted if they become 60 days delinquent within 1 year of reaching −30% equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	Non-judicial	Judicial	Non-recourse	Recourse
	(1)	(2)	(3)	(4)
Interest Rate	0.018**	0.018	0.006	0.025***
	(0.007)	(0.012)	(0.011)	(0.008)
FICO	-0.0004***	-0.0004***	-0.0004***	-0.0004***
	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.005***	0.006***	0.005***	0.005***
	(0.000)	(0.001)	(0.000)	(0.000)
log(Loan amount)	0.001	0.007	0.015**	-0.002
	(0.004)	(0.006)	(0.006)	(0.004)
Debt-to-income	0.001***	0.0003	0.001***	0.0004***
	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes
Observations	29,390	15,011	15,442	28,959
Adjusted R^2	0.081	0.085	0.079	0.087

2.7 Application to monetary policy and robustness exercises

There is a growing literature on the consequences of monetary policy for household behavior (Hurst and Stafford (2004), Mian, Rao and Sufi (2013), Chen, Michaux and Roussanov (2013), Di Maggio, Kermani, Keys, Piskorski, Ramcharan, Seru and Yao (2017), Agarwal, Chomsisengphet, Mahoney and Strobel (2015), Auclert (2017), Beraja, Fuster, Hurst and Vavra (2017)). The primary mechanism is through refinancing—households have equity in their home that they only access when interest rates drop. Thus, monetary policy has a role in stimulating demand through the relaxation of this credit constraint.

Our instrument can contribute to this literature. We have shown that small changes in mortgage rates at origination can influence default, deleveraging and prepayments, and consumption. To tie this paper more closely to monetary policy, we replicate our main result using the interest rate on the 10-year Treasury Bond instead of the benchmark mortgage rate.

Table 2.14 replicates our main results from Table 2.4, but uses the 10-year Treasury Bond rate at mortgage origination as the instrument instead of the benchmark mortgage rate. The resulting coefficients on contracted rate are generally stronger when using this different instrument, though not statistically distinguishable from what we reported in Table 2.4. A tight connection between mortgage rates and the 10-year Treasury Bonds suggests that monetary policy today can have long-acting consequences for the future. In particular, tight monetary policy during the housing boom had a hangover effect during the bust in the form of increased defaults stemming

from high mortgage payments (which could not be refinanced due to the lack of home equity).

Table 2.14 can also address concerns about the exogeneity of our main instrument. If the benchmark mortgage rate is influenced by compositional changes in the mortgage market that also impact default (eg the benchmark rate tends to be higher within a given year when there are riskier borrowers) then it may be an invalid instrument. The 10-year Treasury Bond, however, is unaffected by these concerns as it is not directly connected to the mortgage market. The similarity in the estimated coefficients under both instruments suggests that variation in the benchmark mortgage rate is not driven by compositional changes in the mortgage market; rather, it seems that changes in the 10-year Treasury Bond rate are passed through to the benchmark mortgage rate for conforming loans.

Differences by house price dynamics

The housing bust exhibited substantial heterogeneity across the country. Some areas were largely untouched, while others faced declines of over 60%. When testing for the effects of interest rates on mortgage payment, we condition on implied negative equity, where we infer negative equity using an area's house price index. Further, there may be differences in expectations of future price declines for borrowers who purchased a home in the same area and in the same year. If within-year interest rate variation is correlated with mismeasurement in home equity or in house price expectations, then our estimates will not capture the effect of interest rates. To address

Table 2.14: Negative Equity, Default, IV=Tbond

This table examines the causal relationship between mortgage interest rate and default for negative equity loans. Regression statistics are reported for instrumental variables regressions where default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and 10-year treasury rate is used as the instrument. Loans are considered to have defaulted if the become at least 60-days delinquent within 1 year after reaching the specified level of equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10% NE	20% NE	30% NE	40% NE	50% NE
	(1)	(2)	(3)	(4)	(5)
Interest Rate	0.026***	0.044***	0.042***	0.058***	0.029***
	(0.006)	(0.009)	(0.012)	(0.016)	(0.009)
FICO	-0.0003***	-0.0003***	-0.0004***	-0.0003***	-0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.005***	0.005***	0.005***	0.005***	0.003***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
log(Loan amount)	-0.0002	0.007**	0.008*	0.015***	0.011**
	(0.002)	(0.003)	(0.004)	(0.005)	(0.006)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes
Cond. F. Stat	663	416	260	235	112
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.089	0.076	0.084	0.085	0.093

these concerns, we perform our main default analysis from Table 2.4 on different subsamples cut by house price dynamics.

Appendix Table A.2 separately shows results for areas with large house price collapses and areas with small house price collapses, with little difference in the coefficients. Appendix Table A.3 splits the samples by the degree of correlation in house prices within each ZIP3. The estimates are all similar to the baseline estimate, though we lose power when splitting the sample in quartiles. The coefficients imply that our results are strongest in areas with the highest degree of house price correlation (where the 10th percentile of the pairwise zip-code price correlations is over 90%), and it is these areas where mismeasurement within-ZIP3 is likely to be the smallest.

Within-quarter variation

Our primary specification uses within-year variation in interest rates. We can sharpen the analysis by using within-quarter variation, with some cost in statistical power. It seems particularly unlikely that borrowers can change their home purchase decisions in reaction to interest rates over a three-month period. Appendix Table A.4 replicates Table 2.4 using quarterly fixed effects in place of yearly fixed effects. The table shows that the benchmark national mortgage rate is still strongly linked to default for homes that reach 10-20% negative equity. Appendix Table A.5 replicates the analysis using the 10-year Treasury bond rate as the instrument and finds very similar coefficients.

2.8 Conclusion

This paper studies the effect of modest differences in mortgage rates at origination on household behavior. There is substantial evidence in the literature that large reductions in rates (over 1.5 p.p) can prevent many defaults and spur consumption. However, policy makers had difficulty incentivizing financial intermediaries to make such large reductions during the crisis. It may be easier to successfully subsidize smaller reductions in interest rates, on the order of magnitude of 50bp or less. We show that these modest reductions can still significantly reduce defaults. In fact, these smaller reductions have more "bang-for-the-buck"—cutting payments in half can reduce defaults by 55% (Fuster and Willen (2017)), but we show that cutting payments by as little as 5% can reduce defaults by 10-20%. Our results are consistent with severe liquidity constraints during the crisis; we find little evidence of strategic default as a response to small changes in payment size. By studying the interest rate at loan origination (as opposed to the contemporaneous interest rate), we can quantify the long-term consequences of monetary policy.

To identify our effect, we use small, within-year fluctuations in the 30-year fixed rate mortgage rate. For two borrowers who buy a home in the same year, within the same 3-digit zip code, and whose homes eventually reach similar levels of negative equity, monthly changes in the national rate mechanically impact the size of their payments but should not otherwise impact default or prepayment behavior. Balancing tests show that increases in our instrument tends to be slightly correlated with better credit quality except for the mechanical increase in payment size.

We validate our instrument's first-stage using the Consumer Expenditure survey, and find a relationship between payment size and nondurable consumption. Total expenditure is unchanged, which suggests that the change in payment size is substituting for a change in nondurable spending. This provides further evidence that the instrument is not picking up credit quality, income or wealth.

Our instrument is strong due to the mechanical effect of interest rates on payments, and has potentially wide applicability. We show that payment size effects default, prepayments and consumption. Conceivably, any household outcome can be studied with our simple instrument.

Implementing small changes in interest rates may be practically difficult in a crisis given the fixed cost of refinancing a loan. Somebody must be willing to bear this cost. It seems plausible that financially constrained borrowers would be willing to add the cost to their loan balance, in which case somebody (an intermediary or the government) would have to make the loan. Subsidizing closing costs through a government loan would resemble the "national line of credit" policy advocated by Kimball (2012). In any case, streamlining the refinance process could greatly reduce liquidity-driven defaults.

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Appendix Table A.1: Variable definitions – CE Survey

Variable	Definition
Non-durable consumption	Total amount paid for food, entertainment, utility, transport, apparel and footwear during the interview quarter
Durable consumption	Total amount paid for purchasing major appliances, small appliances and vehicles during the interview quarter
Services consumption	Total amount paid for education, domestic services and health related services during the interview quarter
Mortgage principal reduction	Unscheduled mortgage principal payments during the interview quarter
Total expenditure	Sum of all expenditure types in the interview quarter
Mortgage payment	Monthly mortgage payment is calculated using the loan amount, loan term and interest rate
Mortgage-to-loan	Mortgage-to-loan scales the mortgage payment by the mortgage amount at origination

Appendix Table A.2 : Variation in Interest Rate Effect on Default by House Price Change

This table reports the results of regressions aimed at understanding how the effect of interest rate on default varies with the size of the house price decline. Columns (1) and (2) report results for the 10% negative equity sample and results for the 30% negative equity sample are reported in columns (3) and (4). A zip code is defined as a 'big collapse' area if the price decline (defined as the $log\left(\frac{maximum\ HPI\ from\ 2002\ and\ 2008}{minimum\ HPI\ from\ 2008\ and\ 2012}\right)$) is more than the median. 'Small collapse' areas are the zip codes where price decline is less than the median. Regression statistics are reported for instrumental variables regressions where default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is used as the instrument. Loans are considered to have defaulted if the become at least 60-days delinquent within 1 year after reaching the specified level of equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10% NE		30% NE	
	Big Collapse (< median)	Small Collapse (> median)	Big Collapse (< median)	Small Collapse (> median)
	(1)	(2)	(3)	(4)
Interest Rate	0.024***	0.018***	0.020**	0.018**
	(0.005)	(0.005)	(0.010)	(0.008)
FICO	-0.0003***	-0.0004***	-0.0004***	-0.0004***
	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.004***	0.009***	0.004***	0.008***
	(0.000)	(0.001)	(0.001)	(0.001)
log(Loan amount)	0.004*	-0.006**	0.016***	-0.008*
	(0.002)	(0.003)	(0.006)	(0.005)
Debt-to-income	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes
Observations	67,696	67,515	22,201	22,135
Adjusted R^2	0.067	0.113	0.069	0.102

Appendix Table A.3 : Variation in Interest Rate Effect on Default by House Price Decline Variation within 3-digit Zip Codes

This table reports the results of regressions aimed at understanding how the effect of interest rate on default varies with the variation of house prices within 3-digit zip codes. Columns (1) through (4) report results for the 10% negative equity sample and results for the 30% negative equity sample are reported in columns (5) through (8). A 3-digit zip code is defined as 'p10<0.9', if the 10th percentile of the pairwise 5-digit zip code level HPI correlation between 2002 and 2008 is more than 0.9 and 'p25>0.9 & p10<=0.9' if 25th percentile is more than 0.9 but 10th percentile is less than 0.9 and so on. Regression statistics are reported for instrumental variables regressions where default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is used as the instrument. Loans are considered to have defaulted if the become at least 60-days delinquent within 1 year after reaching the specified level of equity. Standard errors are clustered at ZIP3 Code * Negative Equity Year level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10% NE				30% NE			
	p10>0.9	p25>0.9 & p10<=0.9	p50>0.9 & p25<=0.9	p50<=0.9	p10>0.9	p25>0.9 & p10<=0.9	p50>0.9 & p25<=0.9	p50<=0.9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Interest Rate	0.035***	0.020**	0.021***	0.009*	0.030***	0.011	0.009	0.015*
	(0.007)	(0.009)	(0.006)	(0.006)	(0.011)	(0.016)	(0.013)	(0.008)
FICO	-0.0003***	-0.0004***	-0.0004***	-0.0003***	-0.0004***	-0.0005***	-0.0004***	-0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.005***	0.005***	0.005***	0.004***	0.006***	0.005***	0.005***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
log(Loan amount)	0.003	0.003	-0.004	-0.005*	0.016**	0.003	-0.004	-0.011
	(0.004)	(0.005)	(0.003)	(0.003)	(0.007)	(0.007)	(0.008)	(0.007)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***	0.001**	0.0005**	0.0003*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,003	29,743	35,327	36,932	15,777	10,854	10,450	6,872
Adjusted R^2	0.062	0.09	0.072	0.138	0.065	0.098	0.065	0.151

Appendix Table A.4: Effect of Interest Rate on Default, Negative Equity Samples, IV, Quarterly Fixed Effects

This table examines the causal relationship between mortgage interest rate and default for negative equity loans. Regression statistics are reported for instrumental variables regressions where default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and benchmark 30-year mortgage rate is used as the instrument. Loans are considered to have defaulted if the become at least 60-days delinquent within 1 year after reaching the specified level of equity. Standard errors are clustered at ZIP3 Code * Negative Equity Quarter level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10% NE	20% NE	30% NE	40% NE	50% NE
	(1)	(2)	(3)	(4)	(5)
Interest Rate	0.012***	0.011***	0.005	0.006	0.001
	(0.002)	(0.003)	(0.005)	(0.006)	(0.007)
FICO	-0.0004***	-0.0004***	-0.0004***	-0.0004***	-0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.001***	0.0005***	0.0003	-0.0001	-0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log(Loan amount)	-0.004**	-0.001	-0.001	0.002	-0.003
	(0.002)	(0.002)	(0.003)	(0.005)	(0.006)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Quarter	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Quarter	Yes	Yes	Yes	Yes	Yes
Cond. F. Stat	9,468	3,823	1,361	663	909
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.05	0.044	0.042	0.053	0.045

Appendix Table A.5: Effect of Interest Rate on Default, Negative Equity Samples, IV (T-Bond Rate), Quarterly Fixed Effects

This table examines the causal relationship between mortgage interest rate and default for negative equity loans. Regression statistics are reported for instrumental variables regressions where default is the dependent variable, mortgage interest rate is the independent variable being instrumented for, and 10-year treasury rate is used as the instrument. Loans are considered to have defaulted if the become at least 60-days delinquent within 1 year after reaching the specified level of equity. Standard errors are clustered at ZIP3 Code * Negative Equity Quarter level and reported in parentheses below coefficient estimates. *,**, *** denote two-tailed significance at the 10%, 5%, and 1% levels, respectively.

	10% NE	20% NE	30% NE	40% NE	50% NE
	(1)	(2)	(3)	(4)	(5)
Interest Rate	0.013***	0.011**	0.017**	0.017*	-0.006
	(0.003)	(0.005)	(0.008)	(0.010)	(0.012)
FICO	-0.0004***	-0.0004***	-0.0004***	-0.0004***	-0.0003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan-to-value	0.001***	0.0005***	0.0002	-0.0002	-0.0003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log(Loan amount)	-0.003**	-0.001	0.001	0.004	-0.004
	(0.002)	(0.002)	(0.003)	(0.005)	(0.006)
Debt-to-income	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ZIP3 * Neg. Equity Quarter	Yes	Yes	Yes	Yes	Yes
ZIP3 * Origination Quarter	Yes	Yes	Yes	Yes	Yes
Cond. F. Stat	661	214	84	64	70
Observations	135,392	75,095	44,401	26,452	20,870
Adjusted R^2	0.05	0.044	0.042	0.053	0.043

3 Loan Stacking and Loan Performance

3.1 Introduction

An important advantage of online lending is that borrowers can receive multiple loan offers very quickly, reducing the time it takes to obtain a loan. Usually, borrowers apply to multiple lenders on the same day.[1] This makes it easy for borrowers to "stack" loans, to take out loans from multiple lenders in quick succession. In some cases, borrowers stack loans and the lenders are not aware of each other until after they have provided loans to the same borrower. In other cases, lenders may observe these loans, but not until after they have already offered an interest rate.[2] In other cases, one of the loans will come after the other and the second lender will be aware of the first, but not vice versa.

Loan stacking potentially poses a large threat to online lenders. Individuals can borrow funds from two or more lenders at rates and amounts that do not reflect the existence of multiple loans. There are two problems that may arise when this is possible.

The first potential problem is fraud. Consumers can borrow large amounts from many different lenders on the same day—either in their own name or using a stolen identity[3]—and then never repay these loans. The media, along with online lenders and credit bureaus, have promoted the hypothesis that loan stacking is a threat to online lenders because in enables

[1] According to our data.

[2] Most lenders cannot change interest rates after the borrower has accepted the rate. If they find out about an additional loan, they usually must either honor the offered interest rate or reject the borrower outright.

[3] Note that both identity theft and borrowing in one's own name with no intention to repay are fraud. The key feature of fraud is that the borrower never intends to repay.

fraud[4]. There are also multiple industry services intended to address loan stacking, all of which market themselves as anti-fraud services. TransUnion runs a "Fraud Protection Exchange," a real-time data exchange for online lenders, to combat the threat of loan stacking. ID Analytics provides a competing service, which reports loan application to the members of the "ID Network." As the names of these services imply, the original intent of these services was to combat fraud, especially identify fraud.

Another potential threat posed by loan stacking is that borrowers can take on larger debt burdens (or perhaps larger total debt burdens, conditional on interest rate) by stacking loans than they can by taking out a loan from a single lender or from multiple lenders sequentially. Perhaps borrowers are not blatantly fraudulent, but are just exploiting the deficiencies of the reporting system to their advantage. Loan amount, and the related ratios, debt-to-income[5] and loan-to-income[6], are among the most important variables used by lenders when determining the availability and price of credit. By stacking loans, borrowers can assume much more debt than would be possible otherwise. They also may be able to reduce the average interest rate they pay by taking out two smaller loans rather than one larger loan in situations in which they would have been eligible for a larger loan. If the debt burden explanation is correct, we would expect stacked loans to perform worse than other loans only when the information on the other loan is omited. If the borrower's information is adequately updated to reflect the debt from the stacked loan, there will be no additional effect of loan stacking.

[4]Media references in include the Wall Street Journal and Reuters. Two of the three leading credit bureaus in the U.S. (Transunion and Experian) have issued statements about loan stacking and fraud. Both of the lenders in our sample have also publicly addressed the issue of loans stacking and fraud.

[5]Debt-to-income is a standard variable in consumer lending. A borrower's debt-to-income is defined as the sum of all his monthly debt obligations (including the loan being applied for), divided by his gross monthly income.

[6]We define loan-to-income to be the total loan amount divided by annual income. Not that that loan-to-income is not analogous to debt-to-income. Debt-to-income is based on monthly payment, whereas our definition is loan-to-income is based on loan amount.

Currently, there is no academic literature on loan stacking or on the reporting problems faced by credit bureaus in the online era. Our study will thus attempt to answer the most basic questions related to loan stacking and whether it poses a significant threat to online lenders. The main questions we attempt to answer or whether stacked loans perform poorly and, if they do, why they perform poorly.

To address these questions, we use a probabilistic matching procedure to identify which borrowers received loans from both of the two largest online lenders in the U.S. at approximately the same time. We identify a pair of loans as stacked when two loans—one from each lender in our sample—are accepted by the lenders in the same month for a borrower from the same city with similar credit and employment statistics. Using placebo tests, we show that these matches are at least 80% accurate.[7] Loan stacking has become increasingly common over time and is associated with a 50% increase in default probability. Taken together, these two facts suggest that loan stacking is an important issue for lenders to address.

Contrary to anecdotal reports, however, we find no evidence that loan stacking is associated with fraud. All of the loan stackers in our sample make at least one payment, and loan stackers do not systematically default earlier than non-stackers. We do, however, find suggestive evidence that the poor performance of stacked loans is driven by the size of the other loan, and its absence in the credit report used to set the interest rate. If we adjust loan amount to include the combined loan amounts as a single amount, loan stacking is not significantly related to default. We also find the the size of the competitor loan is strongly related to default, with loan stackers with very small competitor loans actually defaulting less frequently than

[7]More formally, the rate of false positives (sometimes called the false discovery rate) is less than 20%. In other words at least 80% of borrowers we mark as loan stackers actually are

non loan stackers.

The remainder of our paper is organized as follows. In Section 1, we will discuss the existing literatures of fraud and online lending, as well as our hypotheses. In Section 2, we will discuss the data, the identification of stacked loans, and summary statistics. In Section 3, we will show the main results on the relationship between loan stacking and default. In Section 4, we will show supplementary results, in attempt to discover the mechanism behind the relationship between stacking and default. In Section 5, we conclude.

3.2 Literature Review

Consumer lending fraud is common and is, unsurprisingly, associated with poor loan performance. Ben-David (2011) finds that a large number of home buyers and sellers collude to misreport transaction prices. Garmaise (2015) finds that borrowers who misreport their assets on mortgage applications are significantly more likely to default. Agarwal, Ben-David, and Yao (2015) find that appraisals for refinancings are inflated, especially for financially constrained borrowers, suggesting that borrowers manipulate appraisals. Jiang, Nelson, and Vytlacil (2014) find that income is often misreported, and that this is associated with poor loan performance. Mian and Sufi (2017) find that risker borrowers are much more likely to misreport and that this was a major cause of the subprime crisis. Carrillo (2013) finds evidence of fraud for profit schemes involving inflated house prices. Griffin and Maturana (2016b) find that 30% of loans have misreported borrower income, transaction prices, or owner occupancy status, that these loans default 50% more frequently, and that originators are largely complicit in the misreporting. Griffin and Maturana (2016a) find that misreporting may actually have

driven the house price appreciation during the house bubble.

Both of our proposed hypotheses for why loan stacking might result in increased defaults fit into this literature. The first explanation is that stacked loans are blatantly fraudulent—the borrowers take out multiple loans, never intending to repay any of them. The second explanation, that stacked loans simply perform worse because relevant information (specifically information about the other loan) is missing from the borrowers credit report, is a form of misreporting. It's a relatively mild form of misreporting in that it does not require the borrower to explicitly lie, but is a form of misreporting nonetheless.

Loan stacking is an issue that is largely unique to unsecured lending. A somewhat similar phenomenon arises in corporate lending. Equity holders have an incentive to issue additional debt to dilute existing, although this only occurs if companies can issue additional debt with the same priority as outstanding debt. (Fama and Miller, 1972) Admati, DeMarzo, Hellwig, and Pfleiderer (2018) have recently suggested that requiring all additional debt to be junior is actually insufficient to avoid this incentive, since default and agency costs produce such an incentive.

In a traditional corporate finance setting, these incentive issues can be largely mitigated by covenants. In personal lending, however, covenants are infeasible. This is also true in other "fintech" lending markets such as online small business lending. In these markets, borrowers can often borrow smaller sums from multiple lenders for a lower average interest rate than they would receive from a single lender, even if these loans are taken out sequentially instead of all at the same time. The last lender would likely provide a loan at approximately the same interest rate as the borrower would receive if they took out one larger loan, but the earlier

smaller loans would be at lower interest rates—interest rates are almost always lower for smaller loan amounts in the personal lending market.

3.3 Data

3.3.1 Data Description

Our data are from the peer-to-peer (P2P) lending industry. We use data on all loans that have been issued by the two largest P2P lenders in the U.S., Lending Club and Prosper. For each lender, we have two sources of data, one of which is currently publicly available and one of which was publicly available in the past. Each lender currently publishes data on all loans issued through their platform, which gives us data for both lenders from 2007–2017.[8]

The data set includes all loans issued through March 31, 2017, with payment activity observations through June 12, 2017 for Prosper, and June 30, 2017 for Lending Club. The Prosper data were downloaded from Prosper.com and contain a series of loan files and a series of listing files. We merge these two files base on common variables (origination date, loan amount, prosper rating, interest rate, and loan term), discarding any duplicate matches. This ensures that our data are completely clean, although it does exclude a small number of loans from our analysis. Borrowers with extreme (either high or low) interest rates, Prosper ratings, and loan amounts are overrepresented in the sample, as are borrowers from earlier in the sample period and borrowers with 60 months loans. This is a mechanical effect–loans with more common characteristics are more likely to falsely match to listings.

For Lending Club, the data are downloaded from Lending Club's website as a series of

[8]Prosper data are availble from 2005, but given our intent to study loan stacking, data from before the inception of Lending Club are not useful for our analysis.

single files and no matching within the Lending Club data set is required. All loans issued by Lending Club are included in the sample, except those issued to borrowers in states that prohibited disclosure of borrower credit information.

The data sets from both lenders contain very detailed information from the borrowers' credit reports, as well as borrower-reported information on income, employment, and loan purpose. Month-by-month repayment data are not available, meaning that we do not observe when borrowers miss one or two payments and then recover. Data on the current status of loan and the date of the last payment are included. For our default analysis, we use a strict default definition, requiring that borrowers miss four monthly payments to be marked as in default. We must stick to this strict definition because we observe charge-offs, but do not observe missed payments except on loans that were late at the time of observation.

3.3.2 Which Loans Are Stacked?

The data include no personally identifiable fields, such as name or social security number. This means we must probabilistically match borrowers between the Lending Club and Prosper data sets. Probabilistic matching has been used previously in the literature to match borrowers across lenders (Agarwal, Amromin, Ben-David, and Evanoff, 2016; Carmichael, 2017).

We simply match borrowers on a number of features that would often be the same for borrowers' records at Lending Club and Prosper. Since Lending Club and Prosper pulled credit reports from two different bureaus during our sample period, our matching mechanism will have many false negatives, that is, there will be many borrowers who are loan stacking that are not marked as such in our sample. We do this in an effort to keep the rate of false positives low. Given that loan stacking is a relatively rare event, keeping the rate of false

positive low is very important for reducing attenuation bias.

For the majority of our analysis, we use the following criteria for marking two loans—one at Lending Club and one at Prosper—as stacked:

- The loan applications were submitted in the same month.
- The borrowers are from the same 3-digit zip code.
- The difference in annual income is less than $100.
- The borrowers have the same home ownership status.
- The borrowers have the same number of open credit lines.
- The difference in the length of credit history is less than 10 months.
- The difference in employment length is less than 12 months.

3.3.3 Accuracy of the Match

The above matching criteria likely miss a number of genuine matches. We cannot estimate exactly how many, but provided the true number of matches is small, this will not significantly impact any of our results. What can impact our results, however is false matches—loans that we mark as stacked which actually are not. Although we cannot exactly estimate this number either, we can put an upper bound on it.

Restricting the sample to exclude the first 6 months and last six months of data, we find 596 stacked loans. By re-implementing the above matching strategy, but requiring that the Lending Club loan be issued exactly six months before the Prosper loan, we can create a placebo matched sample. This placebo match may still have some true positives (although it should have fewer since borrowers will be less likely to match on length of credit history and employment length if the observations are from different points in time). But the placebo match

should have the same number of false positives as the matching procedure requiring the same application month. The placebo match finds 126 matches. This serves as upper bound for the number of false positives in our main sample. 126 out of 596 is 21%, so the rate of false positives is no more than 21%.

3.3.4 Summary Statistics

Summary statistics for the sample are in Table 3.1. A typical borrower on these two platforms has a relatively high income at $76,000. The average loan size is $14,000. The loans are either 3 or 5 year loans. The average loan age in our sample is 25 months with a 10% default rate. Lifetime default rates for these loans are expected to be about 20%. FICO score is not reported in the table (it is not longer available from Lending Club). However, both lenders require a FICO score of 620 or more (with more stringent restrictions in the past). The mean FICO score of Lending Club and Prosper borrowers is in the 700–710 range, with the borrowers being tightly concentrated around this mean. Subprime borrowers are automatically rejected and borrowers with very high credit scores rarely apply.

We report sample means for Lending Club and Prosper loans in Table 3.2. There are some differences, with Lending Club catering to a slightly higher quality borrower at a slightly lower interest rate. The main reason for this is that for much of our sample period, Prosper had a higher maximum interest rate than Lending Club. Overall, the summary statistics support the notion that two lenders are direct competitors. The seem to be offering basically the same products to the same demographic.

Table 3.1: Summary Statistics for All Loans

Statistic	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)	N
income	76,400.840	477,915.800	45,000.000	65,000.000	90,000.000	1,800,264
credit history	203.699	94.515	139	186	253	1,689,270
debt-to-income	19.927	9.973	12.850	19.000	26.000	1,787,458
employment length	42.176	35.316	12	24	72	1,712,589
owns home?	0.567	0.495	0	1	1	1,799,541
income verified	0.754	0.431	1	1	1	1,800,268
recent inquiries	0.746	1.172	0	0	1	1,782,738
interest rate	0.137	0.051	0.100	0.130	0.166	1,800,270
loan amount	14,130.620	8,649.819	7,300.000	12,000.000	20,000.000	1,800,270
loan term	42.936	10.879	36	36	60	1,800,270
percent funded	0.999	0.013	1.000	1.000	1.000	1,800,268
loan age	25.800	20.596	12	21	34	1,800,270
default	0.098	0.297	0	0	0	1,800,270
credit lines	11.357	5.433	8	10	14	1,776,163

Notes to Table: Summary statistics for all loans, both Lending Club and Prosper. Income is annual income reported by borrower. Credit history is the length of credit history in months. Debt-to-income is the debt-to-income ratio, including the loan. Employment length is provided in years, rounded down (censored at 10 years). Owns home indicates home ownership, income verified indicates verification of self-reported income. Recent inquiries is the number of hard credit inquiries in the last 6 months. Interest rate, loan amount, and loan term are the terms of the loan, with loan term given in months. Loans are either 3 years or 5 years, so this is used in most other tables as a 5-year indicator. Percent funded is the fraction of the initially requested amount that is funded. Loan age is the months since issue. Default indicates whether the loan is charged-off or 121+ days past due. Credit lines is the number of open credit lines.

Table 3.2: Lender Subsample Means

	Lending Club	Prosper
income	76,909.080	74,532.320
credit history	199.830	217.616
debt-to-income	18.369	25.850
employment length	34.972	67.707
owns home?	0.603	0.436
income verified	0.696	0.968
recent inquiries	0.641	1.150
interest rate	0.132	0.156
loan amount	14,756.880	11,828.230
5-year loan?	42.796	43.452
percent funded	1.000	0.999
loan age	25.666	26.290
default	0.092	0.120
credit lines	11.666	10.147

Notes to Table: Subsample means for Lending Club and Prosper loans. This table shows the means for the same variables as Table 3.1, but for Lending Club and Prosper subsamples instead of the full sample.

3.4 Loan Stacking and Default

We hypothesize that stacked loans should be riskier. In other words stacked loans should default more frequently than non-stacked loans. There are two primary reasons for believing that stacked loans should default more frequently. The first is that stacked loans may be fraudulent. Borrowers may take out multiple loans with the intention of never repaying any of them. The second reason is that stacked loans have negative borrower credit characteristics that are not reported on the credit report. By stacking loans, borrowers avoid having the hard credit inquiry, the recently opened account, and the outstanding debt that are associated with the other loan appear on their credit report. In the next section, we will attempt to uncover the mechanism through which loan stacking is associated with default, but our hypothesis is that stacked loans should default more frequently.

Hypothesis 1 (H1): *Stacked loans default more frequently than other similar loans.*

3.4.1 Empirical Results

We test this hypothesis using a Cox proportional hazards model. We model the hazard rate, or instantaneous default probability as a function of borrower and loan characteristics, as well as of an indicator variable which takes a value of "1" if the loan is stacked. The hazard rate for loan i at time t is modeled as

$$\lambda_{it} = \lambda_0(t) \exp(\beta_1' X_i + \beta_2' Y_i + \beta_3' Z_i + \gamma Stacked_i)$$

where λ_0 is the baseline hazard, X_i is a vector of borrower characteristics (annual income, credit inquiries in the last 6 months, etc.), Y_i is a vector of loan characteristics (interest rate, loan amount, and loan term), and Z_i is a vector of fixed effect indicators. β_1, β_2, β_3, and γ are coefficient (vectors) to be estimated. Our hypothesis that stacked loans default more frequently may be written as $\gamma > 0$.

We report regression statistics for the proportional hazards model in Table 3.3. In column 1, we report regression statistics for all loans. In columns 2 and 3, we report regression statistics for Prosper and Lending Club loans, respectively. The specifications used in the three columns are identical, except that column 1 includes an indicator variable which takes a value of "1" if the lender is Lending Club.

The "stacked" coefficient is statistically significant at the 5% level for all loans and for Prosper loans, and is economically large in all three regressions. The hazard rate increase associated with stacking ranges is 34% for Lending Club loans and 43% for Prosper loans. For

Table 3.3: Loan Stacking and Default: Cox Proportional Hazards

	Dependent variable:		
	default		
	All	Prosper	Lending Club
loan stacker?	0.344***	0.355**	0.289*
	(0.105)	(0.143)	(0.154)
interest rate	8.315***	5.495***	10.888***
	(0.050)	(0.083)	(0.070)
debt-to-income	0.009***	0.006***	0.010***
	(0.0002)	(0.0004)	(0.0004)
loan-to-income	−0.00003	0.00004	0.917***
	(0.0001)	(0.0001)	(0.034)
credit lines	0.007***	0.007***	0.006***
	(0.001)	(0.001)	(0.001)
income	−0.00000***	−0.00000***	−0.00000***
	(0.00000)	(0.00000)	(0.00000)
credit history	−0.001***	−0.0001	−0.001***
	(0.00003)	(0.0001)	(0.00004)
employment length	0.0005***	−0.0001	0.0004***
	(0.0001)	(0.0001)	(0.0001)
owns home?	−0.214***	−0.244***	−0.196***
	(0.005)	(0.011)	(0.006)
loan amount	0.00001***	0.00004***	−0.00000***
	(0.00000)	(0.00000)	(0.00000)
income verified	0.117***	1.313**	0.087***
	(0.007)	(0.594)	(0.007)
5-year loan?	0.111***	0.225***	−0.018**
	(0.006)	(0.012)	(0.007)
recent inquiries	0.059***	0.050***	0.100***
	(0.001)	(0.002)	(0.003)
Lending Club	0.104***		
	(0.008)		
state fixed effects?	Yes	Yes	Yes
Observations	1,595,941	347,528	1,248,413
R^2	0.036	0.033	0.039

Notes to Table: Regression statistics for a Cox Proportional hazard model of default. "Loan stacker?" is an indicator taking a value of 1 if the borrower is identified as a loan stacker. Column 1 includes all loans and a lender indicator. Columns 2 and 3 only include Prosper and Lending Club loans, respectively. All regressions include state of residence fixed effects. *p<0.1; **p<0.05; ***p<0.01

all loans, we estimate that loan stacking is associated with and 41% increase in the hazard rate. This is a large effect. The change in default rate associated with loan stacking is approximately the same as that associated with a 5 p.p. change in interest rate.

3.4.2 Empirical Results: Propensity Score Matching

Stacked loans are somewhat different than other kinds of loans. This could potentially affect our results. To address this concern we use propensity score matching. We create a propensity-score matched sample and employ a Cox proportional hazards model on this sample instead of on the full sample. We report the results below in Table 3.4.

The results are essentially the same as those using the full sample. The Lending-Club-only result becomes insignificant, but the magnitude only changes from 0.29 with the full sample to 0.27 with the matched sample. For the combined Lending Club and Prosper matched data set, and for the Prosper matched data set, the result is slightly stronger using Propensity score matching than using the full data set. Overall, the propensity score matching results are not qualitatively different from the full-sample results.

3.5 Why Stacked Loans are Riskier

Have established that stacked loans are much riskier than non-stacked loans, we now attempt to determine why they are riskier. The main hypothesis proposed by the media, credit bureaus, and online lenders is that stacked loans are more likely to be fraudulent. We classify a loan as fraudulent if the borrower defaults without ever making a payment. Early defaults have been used previously in the literature to consider mortgage fraud. First payment fails are also sometimes used in the banking industry to identify fraud.

Table 3.4: Loan Stacking and Default, Propensity Scoring

	Dependent variable:		
		default	
	All	Prosper	Lending Club
	(1)	(2)	(3)
loan stacker?	0.394**	0.519***	0.310*
	(0.165)	(0.189)	(0.186)
interest rate	8.125***	5.221***	13.132***
	(1.391)	(1.057)	(1.464)
debt-to-income	0.016*	0.020***	−0.002
	(0.008)	(0.008)	(0.009)
credit lines	0.019	0.063***	0.0003
	(0.018)	(0.017)	(0.016)
income	−0.00001***	−0.00002***	−0.00001***
	(0.00000)	(0.00000)	(0.00000)
credit history	−0.0003	−0.001	−0.00004
	(0.001)	(0.001)	(0.001)
employment length	0.003	−0.001	0.007***
	(0.002)	(0.002)	(0.002)
owns home?	−0.063	0.197	0.157
	(0.179)	(0.141)	(0.148)
loan amount	0.0001***	0.0001***	0.0001***
	(0.00001)	(0.00001)	(0.00001)
state	No	No	No
Observations	3,612	3,010	3,010
R^2	0.022	0.059	0.059

Notes to Table: Regression statistics for Cox proportional hazards model of default using propensity-score matched samples. "Loan stacker?" is an indicator taking a value of 1 if the borrower is identified as a loan stacker. Column 1 includes all loans and a lender indicator. Columns 2 and 3 only include Prosper and Lending Club loans, respectively. All regressions include state of residence fixed effects. *p<0.1; **p<0.05; ***p<0.01

The data, however, do not support the fraud hypothesis. Out of 604 stacked loans, none default on the first payment. There are also none that default on the second payment. There is thus no evidence that stacked loans are fraudulent. Borrowers do not seem to take out two loans on the same day with the intention of repaying neither.

[Include stuff on default timing here?]

3.5.1 Adjusting Loan Amount to Include Stacked Loans

If fraud is not the reason for default, then what is? One possible explanation is that that for stacked loans, the borrower's debt burden is not accurately reflected in the information available to the lender at the time of pricing. Perhaps a borrower who take out two loans, one for amount x and one for amount y is really more like a borrower taking out a loan for and amount $x + y$. In other words, we hypothesize that:

Hypothesis 2 (H2): *Stacked loans perform similarly to unstacked loans if the loan amount for the stacked loan is taken to be the sum of the amounts for the stacked loan pair.*

Of course, no statistical test can determine if stacked loans perform *exactly* the same as unstacked loans, if the loan amounts are adjusted. However, we can formally test whether the effect of stacking is less if we adjust for the amount of the stacked loan. We can also consider whether the largest effect size that lies within a confidence interval is economically significant. We adjust the loan-amount related variables from the default regressions in the previous section (loan-to-income, debt-to-income, and loan amount) to reflect the presence of the stacked loan. We then perform the same regressions as before, reconsidering the relationship between loan stacking and default.

Summary statistics for a proportional hazards model of default are reported in Table 3.5. These results use the 5-to-1 propensity score matched sample. We revise the following credit variables to reflect the presence of the competitor's loan: loan amount, debt-to-income, and loan-to-income. In Table 3.5, we include all three of these variables together in the regression in Column 1. We include them separately in Columns 2–4.

These results suggest that loan stacking is not significantly related to default, once the amount of the competitor's loan is considered. In these regressions, the Stacked coefficient is negative in 3 of the 4 specifications. In the fullest specification, the coefficient is actually significantly negative at the 10% level. If anything, these results actually suggest that, once their credit information is properly updated, loan stackers are better credit risks than other borrowers.

3.5.2 Competitor Loan Size and Default

We will also test whether the size of the stacked loan matters for explaining default. If the poor performance of stacked loans is a consequence of the additional, unobserved debt taken on by these borrowers, those with larger competitor loans will perform worse.

Hypothesis 3 (H3): *Stacked loans perform worse when the size of the stacked loan is larger relative to the size of a lender's own loan.*

To test this hypothesis, we use a Cox proportional hazards model of default as before. We use four different variables of interest. The first variable of interest we will use is competitor loan-to-income (the ratio of the loan amount of the competitor loan to the borrower's annual income). Competitor loan-to-income, however is highly correlated with loan-to-income (cor-

Table 3.5: Loan Stacking and Default with Revised Credit Variables

	Dependent variable:			
	default			
	(1)	(2)	(3)	(4)
loan stacker?	−0.237	−0.102	0.158	−0.068
	(0.160)	(0.159)	(0.124)	(0.139)
revised debt-to-income	2.185***		3.017***	
	(0.553)		(0.471)	
revised loan-to-income	1.746***			1.852***
	(0.618)			(0.301)
revised credit lines	0.001			
	(0.011)			
revised loan amount	0.00000	0.00003***		
	(0.00001)	(0.00001)		
income	−0.00000	−0.00001***	−0.00000	−0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
credit history	−0.0004	−0.00004	−0.0004	−0.0002
	(0.001)	(0.001)	(0.001)	(0.001)
employment length	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
owns home?	−0.317***	−0.346***	−0.307***	−0.349***
	(0.111)	(0.111)	(0.111)	(0.110)
income verified?	0.582***	0.577***	0.656***	0.580***
	(0.198)	(0.192)	(0.197)	(0.192)
5-year loan?	0.285***	0.302***	0.423***	0.307***
	(0.106)	(0.105)	(0.101)	(0.102)
recent inquiries	0.181***	0.174***	0.173***	0.177***
	(0.032)	(0.032)	(0.031)	(0.032)
Lending Club?	0.081	−0.077	0.230**	−0.085
	(0.121)	(0.104)	(0.110)	(0.103)
state	Yes	Yes	Yes	Yes
Observations	3,612	3,612	3,612	3,612
R^2	0.062	0.059	0.059	0.058

Notes to Table: Regression statistics for Cox Proportional hazards regression of default. Explanatory variables include "revised" loan and credit variables. Revised credit variables represent revisions to reflect the precense of competing loan. The competitor's loan amount is added to loan amount, and to debt, and additional credit line is added, etc. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

relation coefficient of 81%). To ensure that we are isolating the relationship of the competitor loan amount and default, we orthoganalize loan-to-income and competitor loan-to-income. We run an OLS regression of competitor loan-to-income on loan-to-income and use the residuals of this regression, alongside loan-to-income, in a default regression. We will also repeat this analysis using competitor loan amount and residual competitor loan amount, instead of competitor loan-to-income and residual competitor loan-to-income. We report the regression statistics in Table 3.6.

In Column 1, the variable of interest is competitor loan-to-income. In Column 2, we use the orthogonalized, residual competitor loan-to-income. In both specifications (residual) competitor loan-to-income is positively and significantly related to default. Column 1 suggests that a 1 p.p. change in loan-to-income is associated with a 3% increase in default hazard; Column 2, a 4% increase in hazard. This is a very large effect, as 1 p.p. change in loan-to-income corresponds to a change in loan amount of only $736 for an average-income borrower.

Column 1 shows that individuals with very small competitor loans are actually as much as 45% *less* likely to default than individuals who do not loan stack. Individuals with competitor loan sizes that are 15% of there income, perform similarly to the non-stackers. Most individuals with stacked loan have loan-to-incomes greater than 15% and so perform worse than if they had had no competitor loan. In Column 2, residual competitor loan-to-income obviously has mean 0, meaning that in this case the coefficient on stacked can be interpreted as the average difference in default hazard.

Columns 3 and 4 use (residual) competitor loan amount instead of loan-to-income. These results confirm the results in the first two columns. The magnitudes are slightly smaller with

Table 3.6: Loan Stacking, Competitor Loan Size, and Default

	Dependent variable:			
	default			
	(1)	(2)	(3)	(4)
loan stacker?	-0.586^{**}	0.327^{***}	-0.147	0.364^{***}
	(0.266)	(0.124)	(0.230)	(0.122)
competitor loan-to-income	4.024^{***}			
	(0.908)			
residual competitor loan-to-income		3.058^{***}		
		(0.994)		
debt-to-income	0.021^{***}	0.021^{***}	0.021^{***}	0.021^{***}
	(0.006)	(0.006)	(0.006)	(0.006)
loan-to-income	1.364^{*}	1.597^{**}	1.637^{**}	1.563^{*}
	(0.792)	(0.792)	(0.795)	(0.800)
credit lines	0.002	0.002	0.002	0.002
	(0.011)	(0.011)	(0.011)	(0.011)
income	-0.00000	-0.00000	-0.00000	-0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
credit history	-0.0003	-0.0003	-0.0004	-0.0003
	(0.001)	(0.001)	(0.001)	(0.001)
employment length	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
owns home?	-0.320^{***}	-0.314^{***}	-0.317^{***}	-0.315^{***}
	(0.111)	(0.111)	(0.112)	(0.112)
loan amount	0.00000	0.00000	-0.00000	0.00000
	(0.00001)	(0.00001)	(0.00001)	(0.00001)
competitor loan amount			0.00003^{***}	
			(0.00001)	
residual competitor loan amount				0.00003^{**}
				(0.00001)
income verified	0.593^{***}	0.596^{***}	0.603^{***}	0.604^{***}
	(0.198)	(0.198)	(0.198)	(0.198)
5-year loan?	0.301^{***}	0.298^{***}	0.315^{***}	0.303^{***}
	(0.107)	(0.107)	(0.108)	(0.108)
recent inquiries	0.177^{***}	0.178^{***}	0.178^{***}	0.180^{***}
	(0.032)	(0.032)	(0.032)	(0.032)
Lending Club?	0.104	0.092	0.093	0.084
	(0.123)	(0.123)	(0.123)	(0.123)
state	Yes	Yes	Yes	Yes
Observations	3,612	3,612	3,612	3,612
R^2	0.062	0.059	0.059	0.058

Notes to Table: Regression statistics for Cox Proportional hazards regression of default. In addition to a "loan stacker" indicator variables, explanatory variables include variables related to the loan with the competitor: competitor loan-to-income which is the loan to income of the competitor loan, residual competitor loan-to-income, which is the residual of a regression of competitor loan-to-income on loan-to-income, as well as competitor loan amount and residual competitor loan amount, which are analogous variables using loan amount instead of loan-to-income. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

a change in loan size of $1,000 being associated with a 3% or 4% change in default rate, depending on the specification.

Our results suggest that loan stacking is not always a negative signal. It is the size of the competitor loan that is important. When the size of the competitor loan is included in loan amount related variables, loan stacking is not significantly related to default. When the size of the competitor loan is considered as a separate variable, we see that loan stackers with very small competitor loans perform better than comparable non loan stackers.

3.5.3 Loan Stacking and Loan Size

Since the higher default rates associated with loan stacking seem to be associated with the amount borrowed, we will now take a closer look at the size of stacked loans. At a minimum, we would expect stacked loan *pairs* to be for larger loans amounts than unstacked *loans*. There is no reason to expect stacked loans to be larger than unstacked loans. Stacked loans may perform poorly because loan stackers have more total debt and larger total monthly debt obligations, even if the individual loans are smaller. If stacked loans are larger than unstacked loans, this indicates that loan stackers are actually taking out double debt taken out by non-stackers.

Empirically, stacked loans are on average $943 larger than unstacked loans ($15,077 vs. $14,135). In Table 3.7, we present summary statistics for a regression of loan amount a stacked indicator and controls. The relationship remains, even with controls. These regressions are performed on a propensity-score matched data set, which is why the univariate regression implies a larger difference between stacked and unstacked loans than the $943 difference between stacked and unstacked loan amounts in the full sample.

Table 3.7: Loan Amount

	Dependent variable:			
	loan amount			
	(1)	(2)	(3)	(4)
loan stacker?	1,787.886***	1,787.886***	965.024***	954.974***
	(299.808)	(290.486)	(146.209)	(146.267)
debt-to-income			−38.874***	−35.969***
			(9.231)	(9.366)
loan-to-income			59,641.940***	59,560.920***
			(770.958)	(772.002)
credit lines			92.006***	88.253***
			(17.429)	(17.546)
income			0.103***	0.103***
			(0.002)	(0.002)
credit history			4.892***	4.735***
			(1.011)	(1.015)
employment length			−5.139**	−5.311**
			(2.354)	(2.355)
owns home?			1,342.427***	1,316.924***
			(167.957)	(168.491)
income verified			1,193.602***	1,249.380***
			(237.188)	(239.096)
5-year loan?			542.787***	643.764***
			(165.945)	(174.978)
recent inquiries			22.572	49.404
			(61.265)	(63.005)
interest rate				−2,831.593*
				(1,560.546)
Lending Club		2,865.280***	390.309**	402.089**
		(290.851)	(182.923)	(182.979)
Constant	13,289.290***	10,839.670***	−9,995.508***	−9,663.796***
	(211.996)	(1,471.687)	(807.859)	(828.033)
Observations	3,624	3,624	3,615	3,615
Adjusted R^2	0.009	0.070	0.769	0.769

Notes to Table: Regression statistics for linear regression of loan amount. *p<0.1; **p<0.05; ***p<0.01

Stacked loans are, on average, larger than unstacked loans to otherwise similar borrowers. This means that the average loan stacker assumes more than double the new debt as the average non-stacker. This substantial difference in new debt assumed by the borrowers is what we would expect. It is consistent with the results from the previous sections, which suggest that loan stacking is associated with increased risk because it is associate with greater new debt.

3.6 Conclusion

We show that stacked loans are, on average, larger and riskier than unstacked loans. We find no evidence that the increased default risk associated with stacked loans is driven by outright fraud. Larger loan amounts are associated with increased default risk, and the increased default risk associated with loan stacking is similar to this. Once the borrowers' loan and credit information is adjusted to reflect the presence of the stacked loan, there is no increased risk associated with loan stacking. In other words, a borrower who takes out a $\$10,000$ loan and simultaneously or shortly thereafter takes out another $\$10,000$ loan from another lender, performs very similarly to a borrower who takes out a $\$20,000$ loan.

A loan stacker who takes out a very large loan from a competitor is much more likely to default, whereas a loan stacker with a sufficiently small competitor loan is no more likely to default than any other borrower. There seems to be no increase in default risk associate with loan stacking per se, but the additional new debt that often does not appear on the borrower's credit report leads to loan stackers defaulting at significantly higher rates than expected.

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