Online Appendix

Targeted Debt Relief and the Origins of Financial Distress: Experimental Evidence from Distressed Credit Card Borrowers

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Appendix A: Additional Results

Appendix Table A1: Comparison of Recommended and Not Recommended Borrowers

	Approved Mean	Approved vs. Not Approved
Baseline Characteristics	(1)	(2)
Age	40.797	0.0001
		(0.0001)
Male	0.363	0.0008
		(0.0016)
White	0.639	-0.0050
		(0.0039)
Black	0.170	-0.0028
		(0.0046)
Hispanic	0.088	-0.0074
		(0.0041)
Number of Dependents	2.178	0.0032
		(0.0018)
Homeowner	0.419	-0.0055
		(0.0030)
Renter	0.435	-0.0016
		(0.0022)
Monthly Income (1,000s)	2.495	-0.0010
		(0.0009)
Debt in Repayment (1,000s)	18.558	0.0000
		(0.0001)
Percent with Exp. Creditors	0.446	0.0008
		(0.0032)
Baseline Outcomes		
Bankruptcy	0.003	-0.0095
		(0.0134)
Nonzero Collections Debt	0.248	-0.0013
		(0.0018)
Credit Score	586.355	0.0000
		(0.0000)
Employment	0.848	0.0047
		(0.0029)
Earnings (1,000s)	23.698	-0.0001
		(0.0000)
Data Quality		
Matched to SSA data	0.953	0.0319
		(0.0243)
Matched to TU Data	0.867	-0.0144
		(0.0096)
p-value from joint F-test		[0.0012]
Number of Observations	78,438	85,152

Notes: This table reports descriptive statistics for individuals recommended and not recommended for the repayment program. Column 1 reports the mean for the estimation sample recommended for the repayment program. Column 2 reports the difference between recommended and and not recommended individuals controlling for randomization strata fixed effects and clustering standard errors at the counselor level. The p-value is from an F-test of the joint significance of the variables listed.

Appendix Table A2: Intent-to-Treat Estimates in Different Samples

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Full-Sample Estimates							
Treat. Eligibility	0.019	0.010	-0.006	0.000	-0.324	-0.002	-0.074
	(0.004)	(0.003)	(0.002)	(0.003)	(0.530)	(0.002)	(0.112)
Control Group Mean	0.328	0.143	0.105	0.389	604.099	0.821	27.148
Number of Observations	78,438	78,438	78,438	68,000	67,705	74,738	74,738
Panel B: No Debt with Exp. Creditors							
Treat. Eligibility	0.009	-0.003	-0.005	-0.000	-0.628	0.001	0.384
	(0.008)	(0.004)	(0.006)	(0.009)	(1.469)	(0.005)	(0.309)
Control Group Mean	0.158	0.042	0.084	0.558	567.859	0.829	22.808
Number of Observations	18,582	18,582	18,582	16,122	16,022	17,742	17,742
Panel C: Nonzero Debt with Exp. Creditors							
Treat. Eligibility	0.022	0.014	-0.008	0.001	-0.429	-0.003	-0.146
	(0.005)	(0.003)	(0.003)	(0.004)	(0.674)	(0.002)	(0.140)
Control Group Mean	0.381	0.175	0.111	0.336	615.425	0.818	28.512
Number of Observations	59,856	59,856	59,856	51,878	51,778	56,996	56,996
Panel D: 1%-50% Debt with Exp. Creditors							
Treat. Eligibility	0.013	0.016	-0.014	0.001	-0.014	-0.000	0.196
	(0.009)	(0.007)	(0.005)	(0.007)	(1.161)	(0.004)	(0.267)
Control Group Mean	0.354	0.129	0.124	0.414	599.096	0.822	27.264
Number of Observations	23,914	23,914	23,914	20,798	20,698	22,719	22,719
Panel E: 51%-100% Debt with Exp. Creditors							
Treat. Eligibility	0.029	0.014	-0.004	0.003	-1.048	-0.003	-0.172
	(0.007)	(0.006)	(0.004)	(0.005)	(0.894)	(0.003)	(0.203)
Control Group Mean	0.407	0.209	0.102	0.282	626.787	0.815	29.347
Number of Observations	35,216	35,216	35,216	30,429	30,424	33,599	33,599

Notes: This table reports intent-to-treat estimates of the impact of treatment eligibility in different samples. Panel A reports the full-sample estimates from Tables 4-7. Panel B restricts the sample to individuals with no experimental debt. Panel C restricts the sample to individuals with experimental debt. Panel D restricts the sample to individuals with 1%-50% experimental debt. Panel E restricts the sample to individuals with 51%-100% experimental debt. All specifications control for potential treatment intensity, the baseline controls in Table 2, and randomization strata fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A3: Creditor Concessions and Dates of Participation

	Interest Rates		Minimum Payments		
Creditor	Treatment	Control	Treatment	Control	Dates of Participation
1	1.00%	7.30%	2.00%	2.00%	Jan. 2005 to Aug. 2006
2	0.00%	9.90%	1.80%	2.20%	Jan. 2005 to Aug. 2006
3	0.00%	9.00%	1.80%	2.00%	Jan. 2005 to Aug. 2006
4	0.00%	8.00%	2.44%	2.44%	Feb. 2005 to Aug. 2006
5	2.00%	6.00%	1.80%	2.30%	Jan. 2005 to Aug. 2006
6	0.00%	9.90%	2.25%	2.25%	Apr. 2005 to Aug. 2006
7	1.00%	10.00%	1.80%	2.00%	May 2005 to Oct. 2005
8	2.00%	6.00%	1.80%	2.30%	Sept. 2005 to Aug. 2006
9	0.00%	9.90%	1.80%	2.20%	Jan. 2005 to Aug. 2006
10	0.00%	9.90%	1.80%	2.20%	Jan. 2005 to Aug. 2006
11	0.00%	9.90%	1.80%	2.20%	Jan. 2005 to Aug. 2006

Notes: This table details the terms offered to the treatment and control groups by the 11 creditors participating in the randomized trial. Minimum monthly payments are a percentage of the total debt enrolled. See the text for additional details.

Appendix Table A4: Correlates of Creditor Holdings

	Creditor 1	Creditor 2	Creditor 3	Creditor 4	Creditor 5	Creditor 6	Creditor 7	Creditor 8	Creditor 9	Creditor 10	Creditor 11
Baseline Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Age	-0.0004	-0.0010	0.0017	0.0000	0.0005	-0.0003	-0.0009	-0.0001	0.0022	-0.0006	-0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0001)
Male	-0.0028	0.0015	-0.0046	-0.0005	0.0022	-0.0238	0.0058	-0.0046	-0.0235	-0.0039	0.0088
	(0.0022)	(0.0032)	(0.0033)	(0.0002)	(0.0022)	(0.0040)	(0.0025)	(0.0036)	(0.0038)	(0.0034)	(0.0019)
White	0.0021	0.0067	-0.0046	0.0002	0.0015	-0.0361	0.0087	-0.0146	0.0196	-0.0187	-0.0190
	(0.0046)	(0.0064)	(0.0069)	(0.0004)	(0.0039)	(0.0083)	(0.0056)	(0.0078)	(0.0073)	(0.0068)	(0.0043)
Black	0.0061	-0.0052	-0.0023	0.0007	-0.0054	-0.0284	-0.0054	-0.0497	-0.0452	-0.0578	-0.0212
	(0.0049)	(0.0072)	(0.0076)	(0.0005)	(0.0045)	(0.0090)	(0.0056)	(0.0082)	(0.0088)	(0.0072)	(0.0048)
Hispanic	0.0045	0.0021	0.0207	-0.0002	-0.0082	0.0048	-0.0148	-0.0069	-0.0129	-0.0353	-0.0227
	(0.0062)	(0.0074)	(0.0085)	(0.0005)	(0.0047)	(0.0101)	(0.0060)	(0.0090)	(0.0094)	(0.0081)	(0.0046)
Number of Dependents	-0.0002	-0.0041	0.0020	-0.0001	0.0006	-0.0028	-0.0019	-0.0046	0.0020	-0.0057	-0.0057
	(0.0008)	(0.0015)	(0.0013)	(0.0001)	(0.0008)	(0.0015)	(0.0010)	(0.0014)	(0.0016)	(0.0012)	(0.0008)
Homeowner	0.0227	-0.0193	-0.0042	-0.0006	0.0016	0.0112	0.0113	-0.0034	-0.0135	0.0188	-0.0099
	(0.0036)	(0.0054)	(0.0055)	(0.0004)	(0.0033)	(0.0065)	(0.0043)	(0.0063)	(0.0061)	(0.0057)	(0.0029)
Renter	-0.0081	-0.0058	0.0018	-0.0004	-0.0021	-0.0097	0.0039	-0.0043	-0.0196	-0.0018	-0.0021
	(0.0030)	(0.0052)	(0.0051)	(0.0004)	(0.0028)	(0.0056)	(0.0035)	(0.0049)	(0.0056)	(0.0047)	(0.0028)
Monthly Income (1,000s)	0.0049	0.0039	0.0037	0.0002	-0.0023	0.0130	-0.0002	0.0014	0.0129	-0.0008	0.0094
	(0.0011)	(0.0015)	(0.0015)	(0.0001)	(0.0011)	(0.0018)	(0.0012)	(0.0014)	(0.0016)	(0.0015)	(0.0010)
Debt in Repayment (1,000s)	0.0009	0.0046	0.0001	0.0000	0.0018	0.0057	0.0047	0.0067	-0.0007	0.0080	0.0023
	(0.0001)	(0.0002)	(0.0001)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Percent with Exp. Creditors	0.0923	0.3496	0.0894	0.0002	0.0889	0.3841	0.1301	0.4588	-0.1543	-0.1275	-0.0346
	(0.0047)	(0.0075)	(0.0052)	(0.0003)	(0.0043)	(0.0060)	(0.0042)	(0.0070)	(0.0080)	(0.0043)	(0.0030)
Baseline Outcomes											
Bankruptcy	0.0072	0.0073	0.0372	-0.0005	0.0029	-0.0399	-0.0345	-0.0544	-0.0637	-0.0481	-0.0241
	(0.0165)	(0.0209)	(0.0261)	(0.0005)	(0.0112)	(0.0225)	(0.0129)	(0.0230)	(0.0299)	(0.0217)	(0.0096)
Nonzero Collections Debt	-0.0179	-0.0440	-0.0277	0.0001	-0.0047	-0.0197	0.0051	-0.0590	-0.0804	-0.0517	-0.0105
	(0.0031)	(0.0040)	(0.0041)	(0.0003)	(0.0024)	(0.0047)	(0.0027)	(0.0043)	(0.0051)	(0.0042)	(0.0023)
Credit Score	-0.0001	-0.0001	-0.0005	-0.0000	0.0001	-0.0002	0.0001	-0.0001	-0.0005	0.0002	0.0001
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Employment	0.0068	0.0091	0.0077	0.0004	0.0031	0.0200	-0.0023	0.0024	0.0351	0.0154	-0.0001
	(0.0039)	(0.0055)	(0.0057)	(0.0003)	(0.0040)	(0.0059)	(0.0041)	(0.0066)	(0.0066)	(0.0049)	(0.0035)
Earnings (1,000s)	0.0002	-0.0002	0.0004	-0.0000	-0.0001	0.0001	0.0002	-0.0003	0.0002	-0.0003	0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Data Quality											
Matched to SSA data	-0.0208	-0.0754	-0.0898	0.0004	0.0123	-0.0536	-0.1240	0.3314	0.1211	0.0017	-0.0378
	(0.1354)	(0.1219)	(0.1434)	(0.0007)	(0.0692)	(0.1828)	(0.0531)	(0.1566)	(0.1731)	(0.0701)	(0.0309)
Matched to TU Data	0.0731	0.0578	0.2689	0.0010	-0.0488	0.0830	-0.0509	0.0284	0.3140	-0.1145	-0.0738
	(0.0128)	(0.0178)	(0.0181)	(0.0011)	(0.0101)	(0.0202)	(0.0128)	(0.0192)	(0.0208)	(0.0170)	(0.0108)
Control Group Mean	0.073	0.215	0.165	0.001	0.056	0.320	0.100	0.381	0.248	0.189	0.049
Number of Observations	78,438	78,438	78,438	78,438	78,438	78,438	78,438	78,438	78,438	78,438	78,438

Notes: This table reports estimates from OLS regressions of creditor holdings on baseline characteristics. See the text for additional details.

Appendix Table A5: Descriptive Statistics by Treatment Intensity

	Zero Write-Down Zero Payment		Low Write-Down Low Payment		Low Write-Down High Payment		High Write-Down Low Payment		High Write-Down High Payment	
	Control	T vs. C	Control	T vs. C	Control	T vs. C	Control	T vs. C	Control	T vs. C
Baseline Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age	38.656	−0.0007	41.846	−0.0004	40.768	0.0001	44.416	−0.0010	41.668	0.0001
		(0.0005)		(0.0005)		(0.0008)		(0.0016)		(0.0005)
Male	0.356	−0.0087	0.359	−0.0099	0.367	0.0020	0.363	0.0295	0.383	0.0060
		(0.0089)		(0.0109)		(0.0205)		(0.0353)		(0.0135)
White	0.577	−0.0085	0.650	−0.0044	0.654	0.0012	0.721	0.0569	0.689	−0.0339
		(0.0178)		(0.0224)		(0.0370)		(0.0698)		(0.0247)
Black	0.233	−0.0017	0.151	−0.0220	0.137	−0.0277	0.121	0.0427	0.125	−0.0409
		(0.0198)		(0.0232)		(0.0456)		(0.0901)		(0.0303)
Hispanic	0.096	−0.0002	0.092	−0.0222	0.099	−0.0381	0.069	0.0346	0.080	−0.044
		(0.0236)		(0.0261)		(0.0427)		(0.0930)		(0.0309)
Number of Dependents	2.171	−0.0010	2.248	−0.0009	2.217	−0.0104	2.173	−0.0000	2.089	−0.0003
		(0.0029)		(0.0047)		(0.0079)		(0.0144)		(0.0051)
Homeowner	0.321	−0.0205	0.462	0.0164	0.476	0.0163	0.510	0.1038	0.449	0.0328
		(0.0161)		(0.0188)		(0.0358)		(0.0630)		(0.0245)
Renter	0.506	−0.0208	0.406	0.0020	0.391	0.0030	0.365	0.0558	0.411	0.0272
		(0.0122)		(0.0181)		(0.0309)		(0.0583)		(0.0226)
Monthly Income (1,000s)	2.116	−0.0004	2.678	−0.0005	2.715	−0.0008	2.729	−0.0192	2.653	0.0017
		(0.0045)		(0.0042)		(0.0074)		(0.0144)		(0.0051)
Debt in Repayment (1,000s)	11.041	0.0003	22.133	0.0001	21.561	0.0005	25.873	−0.0003	20.953	−0.0001
		(0.0004)		(0.0004)		(0.0006)		(0.0011)		(0.0005)
Percent with Exp. Creditors	0.154	−0.0366	0.349	−0.0018	0.697	−0.0554	0.591	−0.1075	0.807	−0.0429
		(0.0177)		(0.0256)		(0.0503)		(0.1054)		(0.036)
Baseline Outcomes										
Bankruptcy	0.007	−0.0399	0.003	0.0679	0.002	−0.0682	0.000	0.3807	0.001	0.0532
		(0.0533)		(0.0979)		(0.1864)		(0.3602)		(0.1459)
Nonzero Collections Debt	0.397	0.0105	0.216	0.0033	0.176	0.0057	0.128	0.0421	0.143	−0.0344
		(0.0109)		(0.0158)		(0.0292)		(0.0501)		(0.0202)
Credit Score	567.626	0.0000	583.136	−0.0000	596.860	−0.0002	609.825	0.0001	606.083	−0.0000
		(0.0001)		(0.0001)		(0.0002)		(0.0003)		(0.0001)
Employment	0.850	0.0119	0.850	0.0103	0.871	−0.0029	0.820	−0.0400	0.839	−0.0089
		(0.0163)		(0.0201)		(0.0330)		(0.0549)		(0.0224)
Earnings (1,000s)	20.586	−0.0000	25.222	−0.0002	26.215	−0.0004	24.605	−0.0003	24.828	−0.0001
		(0.0003)		(0.0003)		(0.0005)		(0.0010)		(0.0004)
Data Quality										
Matched to SSA data	0.953	−0.2159	0.948	−0.0099	0.953	0.1483	0.962	0.0687	0.953	0.0364
		(0.1893)		(0.3275)		(0.5624)		(0.1453)		(0.0541)
Matched to TU Data	0.872	−0.0272	0.870	−0.0047	0.864	0.1227	0.876	−0.0556	0.865	−0.0028
		(0.0439)		(0.0640)		(0.1053)		(0.1957)		(0.0766)
p-value from joint F-test		[0.2595]		[0.9949]		[0.7907]		[0.9067]		[0.6046]
Number of Observations	12,996	25,477	10,390	20,429	4,959	9,998	2,962	5,780	8,548	16,754

Notes: This table reports descriptive statistics and balance tests by potential treatment intensity. See the Table 2 notes for details.

Appendix Table A6: Non-Parametric Estimates

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat. x No Write-Down	0.003	-0.003	-0.001	0.007	-1.404	0.001	0.085
x No Payment Reduction	(0.006)	(0.004)	(0.004)	(0.005)	(0.894)	(0.003)	(0.198)
Treat. x Low Write-Down	0.003	0.012	-0.012	-0.002	0.543	0.001	0.023
x Low Payment Reduction	(0.007)	(0.006)	(0.005)	(0.006)	(1.073)	(0.004)	(0.223)
Treat. x Low Write-Down	-0.003	-0.001	0.002	0.010	-2.780	0.002	0.105
x High Payment Reduction	(0.011)	(0.010)	(0.009)	(0.011)	(2.129)	(0.007)	(0.444)
Treat. x High Write-Down	0.025	0.016	-0.000	-0.002	0.130	0.004	-0.680
x Low Payment Reduction	(0.010)	(0.008)	(0.007)	(0.008)	(1.377)	(0.005)	(0.322)
Treat. x High Write-Down	0.035	0.016	-0.012	-0.003	0.031	-0.011	-0.171
x High Payment Reduction	(0.007)	(0.006)	(0.006)	(0.007)	(1.219)	(0.004)	(0.271)
Control Group Mean	0.328	0.143	0.105	0.389	604.099	0.821	27.148
Number of Observations	78,438	78,438	78,438	68,000	67,705	74,738	74,738

Notes: This table reports estimates separately by treatment intensity bin. We report coefficients on the interaction of treatment eligibility and an indicator for having potential treatment intensity in the indicated range. All specifications control for potential treatment intensity bins, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A7: Bankruptcy Regression Estimates by Year

	Year 1	Year 2	Year 3	Year 4	Year 5
	(1)	(2)	(3)	(4)	(5)
Treat. x Max Interest Write-Down	-0.014	-0.008	-0.008	0.001	-0.002
	(0.007)	(0.004)	(0.004)	(0.003)	(0.003)
Treat. x Max Payment Reduction	0.010	0.003	0.003	0.000	0.006
	(0.008)	(0.005)	(0.005)	(0.004)	(0.003)
Control Group Mean	0.058	0.018	0.014	0.009	0.006
Creditor Risk Sets	X	X	X	X	X
Number of Observations	78,438	78,438	78,438	78,438	78,438

Notes: This table reports reduced form regression estimates of the impact of targeted debt relief on bankruptcy filing by year. We report estimates for the interaction of treatment eligibility and the maximum potential interest write-down and maximum potential minimum payment reduction. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A8: Regression Estimates Pre- and Post-BAPCPA

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat. x Max Interest	0.029	0.017	-0.039	-0.001	0.565	0.011	-0.648
x Pre-BAPCPA	(0.016)	(0.013)	(0.011)	(0.015)	(2.494)	(0.008)	(0.560)
Treat. x Max Interest	0.064	0.052	-0.012	-0.017	4.152	-0.001	-0.914
x Post-BAPCPA	(0.022)	(0.018)	(0.014)	(0.017)	(3.316)	(0.010)	(0.672)
p-value on difference	[0.124]	[0.082]	[0.098]	[0.429]	[0.315]	[0.260]	[0.735]
Treat. x Max Payment	0.010	0.000	0.031	0.003	0.209	0.001	0.411
x Pre-BAPCPA	(0.021)	(0.017)	(0.013)	(0.020)	(3.060)	(0.011)	(0.788)
Treat. x Max Payment	0.008	-0.004	0.010	0.007	-4.653	-0.016	0.673
x Post-BAPCPA	(0.024)	(0.020)	(0.015)	(0.018)	(3.908)	(0.012)	(0.755)
p-value on difference	[0.931]	[0.847]	[0.232]	[0.871]	[0.258]	[0.197]	[0.767]

Notes: This table reports additional subsample regression estimates. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A9: Additional Subsample Estimates

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Estimates by Baseline Employment							
Treat. x Max Interest	0.051	0.032	-0.002	-0.011	7.426	-0.018	-2.250
x Nonemployed	(0.025)	(0.023)	(0.016)	(0.022)	(4.364)	(0.022)	(0.813)
Treat. x Max Interest	0.036	0.025	-0.035	-0.005	0.772	0.004	-0.494
x Employed	(0.016)	(0.012)	(0.010)	(0.013)	(2.469)	(0.007)	(0.505)
p-value on difference	[0.568]	[0.755]	[0.035]	[0.796]	[0.158]	[0.526]	[0.043]
Treat. x Max Payment	-0.005	-0.009	0.028	-0.003	-5.561	0.013	1.663
x Nonemployed	(0.030)	(0.024)	(0.018)	(0.024)	(4.571)	(0.022)	(0.899)
Treat. x Max Payment	0.019	0.006	0.019	0.005	-1.092	-0.012	0.301
x Employed	(0.020)	(0.015)	(0.012)	(0.016)	(2.964)	(0.010)	(0.678)
p-value on difference	[0.462]	[0.533]	[0.659]	[0.719]	[0.346]	[0.256]	[0.138]
Panel B: Estimates by Gender							
Treat. x Max Interest	0.014	-0.013	-0.012	0.005	0.544	0.008	-0.271
x Male	(0.021)	(0.016)	(0.016)	(0.018)	(3.334)	(0.009)	(0.649)
Treat. x Max Interest	0.049	0.047	-0.041	-0.013	2.565	0.005	-1.034
x Female	(0.017)	(0.015)	(0.010)	(0.014)	(2.604)	(0.009)	(0.531)
p-value on difference	[0.138]	[0.003]	[0.069]	[0.306]	[0.587]	[0.756]	[0.276]
Treat. x Max Payment	0.052	0.023	0.001	0.014	-4.723	-0.010	0.017
x Male	(0.025)	(0.020)	(0.018)	(0.023)	(4.090)	(0.012)	(0.829)
Treat. x Max Payment	-0.008	-0.003	0.037	-0.005	-0.184	-0.005	0.824
x Female	(0.021)	(0.018)	(0.012)	(0.018)	(3.244)	(0.010)	(0.675)
p-value on difference	[0.034]	[0.273]	[0.049]	[0.411]	[0.322]	[0.667]	[0.308]
Panel C: Estimates by Ethnicity							
Treat. x Max Interest	0.036	0.026	-0.024	-0.002	1.225	0.010	-0.856
x White	(0.017)	(0.013)	(0.012)	(0.014)	(2.662)	(0.008)	(0.534)
Treat. x Max Interest	0.039	0.021	-0.047	-0.012	2.802	-0.002	-0.517
x Non-White	(0.021)	(0.018)	(0.013)	(0.019)	(3.013)	(0.011)	(0.665)
p-value on difference	[0.887]	[0.773]	[0.116]	[0.638]	[0.651]	[0.323]	[0.643]
Treat. x Max Payment	0.017	0.002	0.014	0.006	-2.717	-0.006	0.599
x White	(0.019)	(0.016)	(0.013)	(0.017)	(3.014)	(0.011)	(0.703)
Treat. x Max Payment	0.006	0.015	0.050	-0.009	0.420	-0.009	0.375
x Non-White	(0.029)	(0.024)	(0.017)	(0.025)	(4.358)	(0.014)	(0.891)
p-value on difference	[0.692]	[0.606]	[0.051]	[0.530]	[0.476]	[0.826]	[0.812]

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel D: Estimates by Baseline Homeownership							
Treat. x Max Interest	0.033	0.018	-0.028	-0.015	2.107	0.010	-1.250
x Homeowner	(0.018)	(0.016)	(0.012)	(0.015)	(2.746)	(0.009)	(0.577)
Treat. x Max Interest	0.045	0.033	-0.031	0.003	1.614	0.003	-0.371
x Non-Owner	(0.017)	(0.014)	(0.012)	(0.014)	(2.570)	(0.009)	(0.586)
p-value on difference	[0.548]	[0.441]	[0.798]	[0.259]	[0.856]	[0.527]	[0.209]
Treat. x Max Payment	0.029	-0.000	0.016	0.015	-3.417	-0.007	0.696
x Homeowner	(0.021)	(0.019)	(0.014)	(0.018)	(3.198)	(0.011)	(0.768)
Treat. x Max Payment	-0.002	0.006	0.029	-0.009	-0.450	-0.007	0.401
x Non-Owner	(0.021)	(0.017)	(0.013)	(0.018)	(3.323)	(0.011)	(0.737)
p-value on difference	[0.178]	[0.782]	[0.444]	[0.186]	[0.393]	[0.977]	[0.722]

Notes: This table reports additional subsample regression estimates. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A10: Robustness Checks

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Regression Estimates							
Treat. x Max Interest Write-Down	0.039	0.027	-0.030	-0.006	1.898	0.006	-0.749
	(0.014)	(0.011)	(0.009)	(0.012)	(2.272)	(0.007)	(0.468)
Treat. x Max Payment Reduction	0.013	0.003	0.023	0.002	-1.913	-0.007	0.526
	(0.018)	(0.014)	(0.011)	(0.016)	(2.766)	(0.009)	(0.629)
Panel B: Treatment x Demographic Effects							
Treat. x Max Interest Write-Down	0.042	0.028	-0.024	-0.013	3.871	0.010	-0.779
	(0.015)	(0.012)	(0.010)	(0.013)	(2.595)	(0.008)	(0.527)
Treat. x Max Payment Reduction	0.015	0.003	0.025	0.000	-1.382	-0.008	0.507
	(0.018)	(0.014)	(0.011)	(0.017)	(2.798)	(0.009)	(0.648)
p-value from joint F-test	[0.357]	[0.642]	[0.058]	[0.470]	[0.566]	[0.702]	[0.850]
Panel C: Treatment x Creditor Effects							
Treat. x Max Interest Write-Down	0.024	0.011	-0.022	0.008	-0.667	0.016	-1.210
	(0.018)	(0.016)	(0.014)	(0.018)	(3.191)	(0.011)	(0.671)
Treat. x Max Payment Reduction	0.017	0.008	0.014	-0.014	3.859	-0.004	0.559
	(0.022)	(0.018)	(0.015)	(0.021)	(3.460)	(0.012)	(0.757)
p-value from joint F-test	[0.207]	[0.319]	[0.977]	[0.202]	[0.075]	[0.727]	[0.417]
Panel D: Treatment x Demographic and Treatment x Creditor Effects							
Treat. x Max Interest Write-Down	0.028	0.016	-0.018	0.007	-0.189	0.017	-1.213
	(0.018)	(0.016)	(0.014)	(0.018)	(3.327)	(0.011)	(0.678)
Treat. x Max Payment Reduction	0.017	0.010	0.017	-0.014	4.051	-0.004	0.558
	(0.022)	(0.018)	(0.015)	(0.021)	(3.473)	(0.012)	(0.767)
p-value from joint F-test	[0.317]	[0.401]	[0.334]	[0.259]	[0.185]	[0.575]	[0.627]

Notes: This table reports robustness checks of our regression estimates. Panel A reports the regression estimates from Tables 4-7. Panel B adds treatment eligibility x demographic fixed effects for gender, race, homeownership, credit score, earnings, and debt-to-income. Panel C adds treatment eligibility x credit card issuer fixed effects. Panel D adds both treatment eligibility x credit card issuer and treatment eligibility x demographic fixed effects. We also report the p-value from an F-test that all of the indicated interactions are jointly equal to zero. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix Table A11: Estimates with p-values from Permutation Test

	Start Payment	Finish Payment	Bankrupt	Coll. Debt	Credit Score	Empl.	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat. x Max Interest Write-Down	0.039	0.026	-0.031	-0.015	2.045	0.006	-0.719
	[0.006]	[0.030]	[0.000]	[0.603]	[0.336]	[0.331]	[0.156]
Treat. x Max Payment Reduction	0.013	0.002	0.024	0.002	-1.987	-0.008	0.488
	[0.397]	[0.862]	[0.037]	[0.817]	[0.442]	[0.355]	[0.286]
Control Group Mean	0.328	0.143	0.105	0.389	604.099	0.821	27.148
Creditor Risk Sets	X	X	X	X	X	X	X
Number of Observations	78,438	78,438	78,438	68,000	67,705	74,738	74,738

Notes: This table reports reduced form regression estimates where the p-values are calculated using a non-parametric permutation test with 1,000 draws. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and creditor risk set fixed effects. See the text for additional details on the non-parametric permutation test.

Appendix Table A12: Comparison of Experimental Sample to Other Samples

	Experimental Sample	Credit User Sample	Default Sample	Bankruptcy Sample
	(1)	(2)	(3)	(4)
Age	40.85	48.55	41.87	44.84
Credit Score	586.4	739.5	572.3	580.8
Delinquency	0.323	0.148	0.634	0.678
Credit Card Balance (1,000s)	15.95	6.011	5.346	10.46
Credit Card Utilization	66.96	25.50	72.37	70.92
Any Auto Loan	0.467	0.283	0.420	0.473
Any Mortgage Loan	0.335	0.367	0.287	0.579
Number of Observations	68,000	3,308,824	61,079	56,906

Notes: This table reports descriptive statistics for different samples in the TransUnion credit data. Column 1 reports the mean for the estimation sample matched to the TransUnion data. Column 2 reports the mean for a random sample of all credit users from Dobbie et al. (2016). Column 3 reports the mean for credit users with a default in the next year from Dobbie et al. (2017). Column 4 reports the mean for credit users with a Chapter 13 bankruptcy in the next year from Dobbie et al. (2016).

Appendix Table A13: Characteristics of Borrowers Completing the Repayment Program

	Control	Independent Variable		
	Complier Mean	Treatment Eligibility	Treatment x Max Interest Write-Down	Treatment x Max Payment Reduction
Baseline Characteristics	(1)	(2)	(3)	(4)
Age	41.548	-0.0077	0.2775	-0.3839
		(0.2908)	(1.0292)	(1.3007)
Male	0.358	0.0064	-0.0069	0.0355
		(0.0080)	(0.0315)	(0.0403)
White	0.666	0.0058	0.0055	-0.0075
		(0.0090)	(0.0292)	(0.0363)
Black	0.136	0.0005	0.0114	-0.0061
		(0.0058)	(0.0191)	(0.0266)
Hispanic	0.091	-0.0084	-0.0203	0.0105
		(0.0058)	(0.0212)	(0.0256)
Number of Dependents	2.112	-0.0373	-0.0384	-0.0428
		(0.0244)	(0.0850)	(0.1129)
Homeowner	0.422	0.0017	-0.0374	0.0685
		(0.0088)	(0.0317)	(0.0420)
Renter	0.421	0.0082	0.0510	-0.0621
		(0.0090)	(0.0316)	(0.0406)
Monthly Income (1,000s)	2.691	0.0002	0.0493	-0.0729
		(0.0295)	(0.0992)	(0.1424)
Debt in Repayment (1,000s)	19.184	0.5993	1.9798	-1.2594
		(0.2841)	(0.9960)	(1.4747)
Percent with Exp. Creditors	0.532	0.0061	-0.0050	0.0005
		(0.0064)	(0.0090)	(0.0125)
Baseline Outcomes				
Bankruptcy	0.003	0.0006	0.0002	0.0010
		(0.0009)	(0.0029)	(0.0042)
Nonzero Collections Debt	0.163	0.0005	0.0234	-0.0427
		(0.0063)	(0.0221)	(0.0273)
Credit Score	596.163	-1.7605	1.1706	-6.2257
		(4.0306)	(12.9818)	(17.5044)
Employment	0.861	0.0084	-0.0003	0.0079
		(0.0067)	(0.0257)	(0.0332)
Earnings (1,000s)	25.944	0.3457	0.1835	0.6630
		(0.4001)	(1.4184)	(1.7806)
Data Quality				
Matched to SSA data	0.949	0.0025	-0.0033	0.0008
		(0.0038)	(0.0137)	(0.0170)
Matched to TU Data	0.833	-0.0031	-0.0004	-0.0094
		(0.0066)	(0.0207)	(0.0270)
Number of Observations	13,063	26,418	26,418	

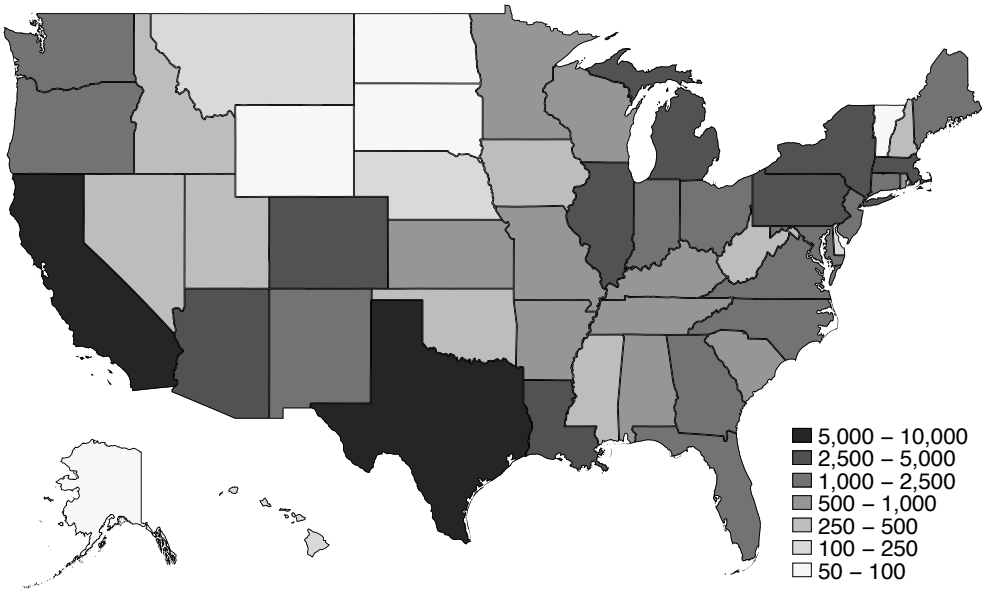
Notes: This table reports descriptive statistics for control and treatment compliers based on program completion. Column 1 reports means for the the control compliers. Column 2 reports estimates from a regression of the indicated variable on treatment eligibility and randomization strata fixed effects. Columns 3-4 reports estimates from a regression of the indicated variable on treatment eligibility interacted with potential treatment intensity and randomization strata fixed effects. All specifications cluster standard errors by counselor. See the text for additional details.

Appendix Table A14: Regression Estimates for Additional Outcomes

	Percent Repaid	Serious Default	Card Balance	Card Util.	Any Auto	Any Mortgage	Nonzero 401k
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat. x Max Interest Write-Down	0.041	-0.012	0.159	-1.609	-0.014	0.016	0.003
	(0.012)	(0.013)	(0.390)	(1.219)	(0.014)	(0.012)	(0.012)
Treat. x Max Payment Reduction	0.003	0.010	0.279	1.032	0.003	-0.018	-0.005
	(0.015)	(0.017)	(0.526)	(1.393)	(0.018)	(0.016)	(0.015)
Control Group Mean	0.209	0.476	8.503	46.277	0.396	0.308	0.274
Creditor Risk Sets	X	X	X	X	X	X	X
Number of Observations	78,438	68,000	68,000	68,000	68,000	68,000	74,738

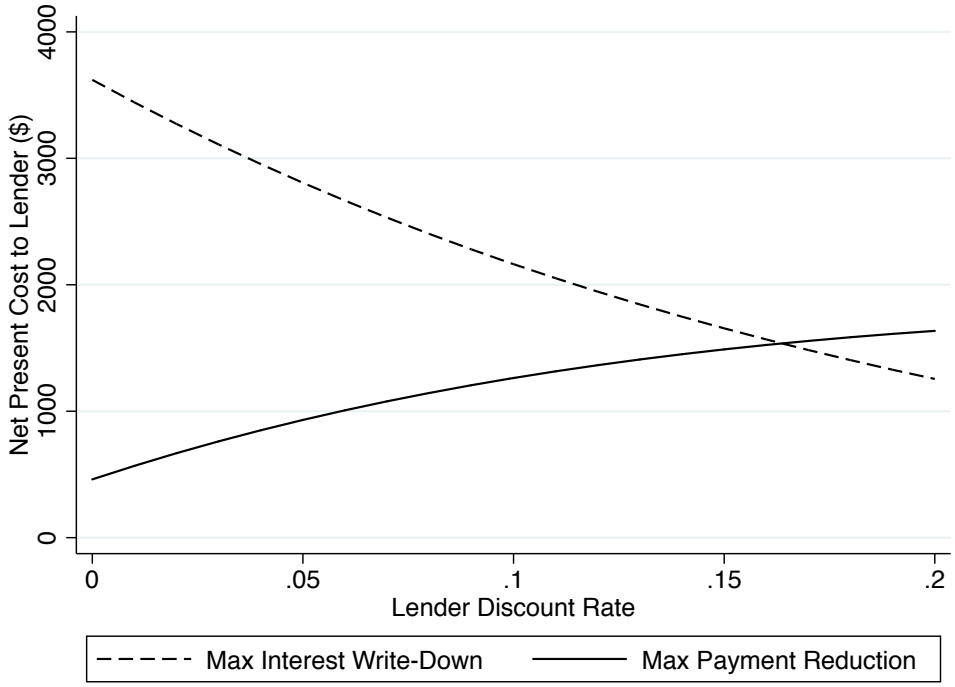
Notes: This table reports additional reduced form regression estimates of the impact of targeted debt relief. We report estimates for the interaction of treatment eligibility and the maximum potential interest write-down and maximum potential minimum payment reduction. All specifications control for potential treatment intensity, the baseline controls in Table 2, randomization strata fixed effects, and the creditor risk sets described in the text. Standard errors are clustered at the counselor level.

Appendix Figure A1: Geographic Distribution of the Experimental Sample



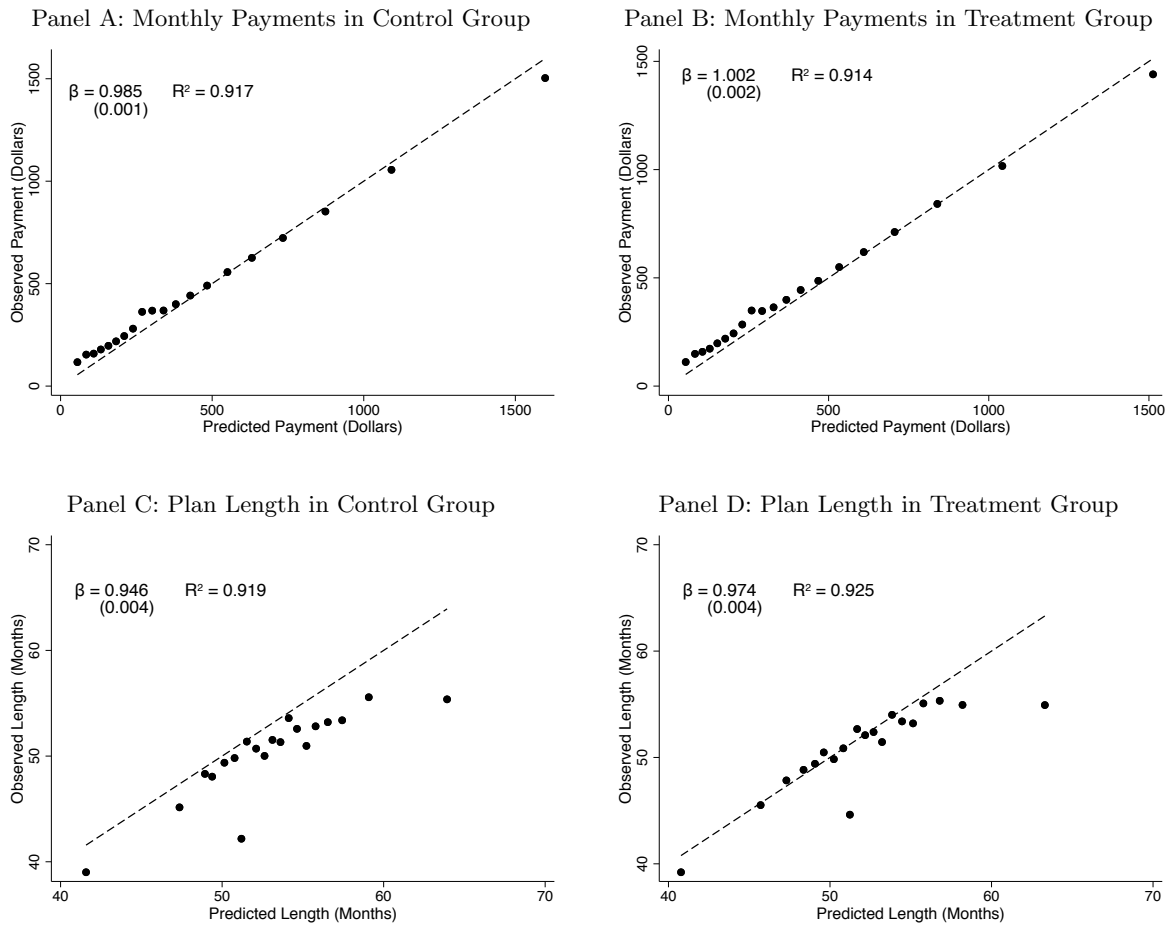
Notes: This figure plots the number of individuals in the experimental sample by state. See the text for additional details.

Appendix Figure A2: Net Present Costs to the Lender



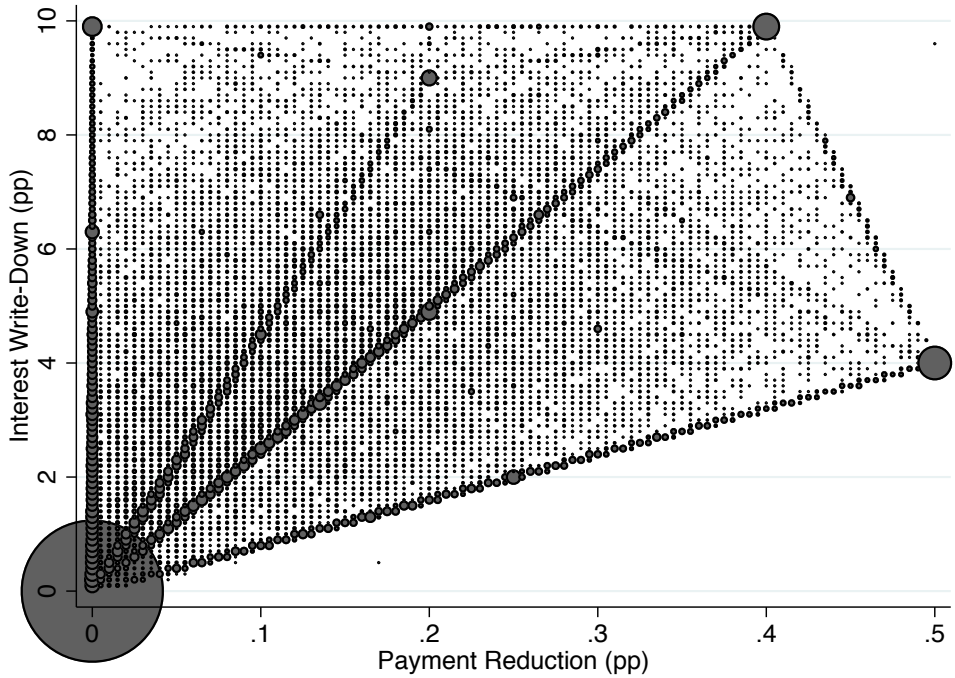
Notes: This figure plots the net present costs to the lender of providing each treatment. Lender costs (relative to the baseline case) are calculated using the control means for debt ($18,470), minimum payment (2.38% of debt), and monthly default rate during the repayment program (1.12%), and a baseline interest rate of 9.90%. The dashed line plots net present costs with the maximum 9.90 percentage point interest rate write-down. The solid line plots costs with the maximum 0.50 percentage point decrease in the required minimum payment. See the text for additional details.

Appendix Figure A3: Predicted and Actual Program Characteristics



Notes: These figures plot predicted repayment program characteristics against actual program characteristics for the control and treatment groups. Actual monthly payments are the maximum of the required minimum payment and the borrower's preferred minimum payment, and are available for all borrowers. Actual plan length is only available for borrowers completing the DMP, and is a function of the actual minimum payment, the actual interest rate, and any extra payments made by the borrower to shorten the repayment period (voluntary early repayment). In other words, the actual plan length should be weakly shorter than the predicted plan length. Actual interest rates are not recorded in the MMI data and are not included in this figure. The coefficient, standard error, and R^2 are estimated using OLS on the underlying micro data. The dashed line is the 45 degree line.

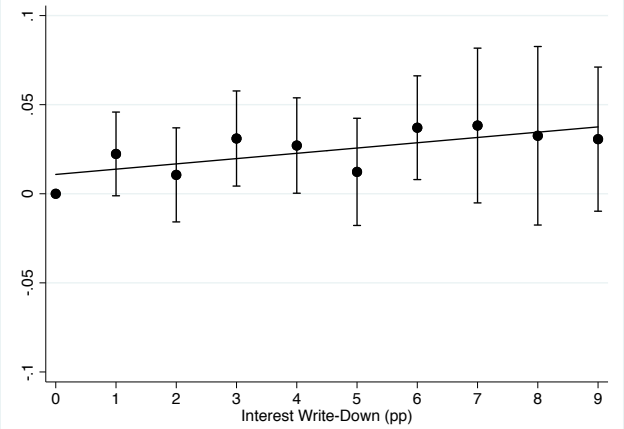
Appendix Figure A4: Distribution of Potential Treatment Intensity



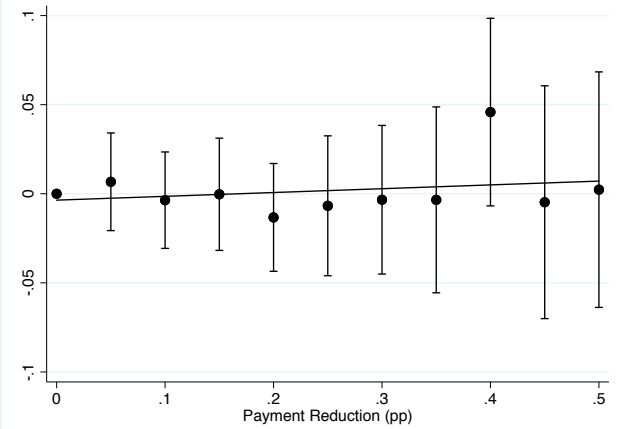
Notes: This figure plots the distribution of potential interest write-downs and potential minimum payment reductions in our estimation sample. Potential interest write-downs and potential minimum payment reductions are calculated using borrower-level data and the rules listed in Appendix Table A4. There is considerable bunching at the origin as approximately 25 percent of borrowers in our sample had no credit card debt with the card issuers participating in the experiment and were offered the "control" repayment program even when they were assigned to the treatment group. There are also four higher density "lines" that trace out the potential treatment intensities for individuals who have a mix of debt with one participating card issuer and one or more non-participating card issuers. For example, the vertical line running from the origin to the upper left corner consists of individuals holding debt with one or more of the card issuers offering a 9.9 percentage point write-down and 0.0 percentage point payment reduction and one or more non-participating card issuers. The greater the proportion of debt with the participating card issuer, the larger the hypothetical interest write-down the individual would receive if treated.

Appendix Figure A5: Non-Parametric Treatment Effects

Panel A: Interest Write-Down



Panel B: Minimum Payment Reduction



Notes: These figures report non-parametric treatment effects and associated 95 percent confidence intervals. All specifications control for potential treatment intensity, randomization strata fixed effects, and creditor risk set fixed effects. Standard errors are clustered at the counselor level.

Appendix B: Empirical Design Details

In this appendix, we formalize the assumptions under which we can identify the causal effects of the interest write-downs and minimum payment reductions using cross-sectional variation in potential treatment intensity.

A. Setup and Identifying Assumptions

Setup and Identifying Assumptions: We omit time subscripts and abstract away from baseline controls for notational simplicity. For each individual i, we observe a binary indicator for treatment eligibility Z_i, an outcome Y_i, and two continuously distributed variables X_{1i} and X_{2i} that determine the treatment intensity if in the treatment group. That is, individuals in the control group ($Z_i = 0$) receive no treatments, while individuals in the treatment group ($Z_i = 1$) receive X_{1i} and X_{2i}. The realized treatment variables can therefore be written as $Z_i X_{1i}$ and $Z_i X_{2i}$. In our context, these realized treatment variables $Z_i X_{1i}$ and $Z_i X_{2i}$ correspond to the $WriteDown_i$ and $Payment_i$ variables in Equation (1), respectively, while the continuous covariate variables X_{1i} and X_{2i} correspond to the potential treatment intensity variables described in the text. Note that we observe the potential treatment intensity variables X_{1i} and X_{2i} for everyone in the sample, regardless of treatment eligibility Z_i.

Causal effects are defined in terms of potential outcomes, where $Y_i(0)$ is the outcome if i is in the control group and $Y_i(1)$ is the outcome if i is in the treatment group. These latent variables are independent across individuals, satisfying a stable unit treatment value assumption (Rubin 1980). To this we add,

A1 *Additive Separability:* Potential outcomes can be written as

$$\begin{aligned} Y_i(1) &= Y_i(0) + \beta_{1i} X_{1i} + \beta_{2i} X_{2i} \\ Y_i(0) &= \mu(X_{1i}, X_{2i}) \\ &= \beta_{3i} X_{1i} + \beta_{4i} X_{2i} + \beta_{5i} \end{aligned}$$

Realized outcomes $Y_i = Y_i(Z_i)$ can therefore be written as

$$Y_i = \beta_{1i} Z_i X_{1i} + \beta_{2i} Z_i X_{2i} + \beta_{3i} X_{1i} + \beta_{4i} X_{2i} + \beta_{5i}.$$

A2 *Independence:* $(X_{1i}, X_{2i}, \beta_{1i}, \beta_{2i}, \beta_{3i}, \beta_{4i}, \beta_{5i}) \perp Z_i$.

Assumption A1 states that potential outcomes are a function of the potential treatment intensities X_{1i} and X_{2i}, thereby allowing for selection bias in non-experimental estimates. In our context, for example, this setup allows individuals holding debt with the credit card issuers offering relatively more generous interest write-downs and payment reductions to be unobservably different compared to individuals holding debt with the credit card issuers offering relatively less generous interest

write-downs and payment reductions. For this reason, OLS estimates of Y_i on $Z_i X_{1i}$ and $Z_i X_{2i}$ in non-experimental populations will be biased if we cannot also control for $\mu(X_{1i}, X_{2i})$, e.g., by using the randomly assigned control group.

In addition, Assumptions A1 and A2 state that $Z_i X_{1i}$ and $Z_i X_{2i}$ can be correlated with β_{1i} and β_{2i}, thereby allowing for Roy (1951)–type selection into the potential treatment intensities. In our context, for example, this setup allows for individuals holding debt with the credit card issuers offering relatively more generous interest write-downs and payment reductions to be more responsive to those interest write-downs and minimum payment reductions compared to individuals holding debt with the credit card issuers offering relatively less generous interest write-downs and payment reductions. For this reason, reduced form estimates of Y_i on $Z_i X_{1i}$ and $Z_i X_{2i}$ (also controlling for X_{1i} and X_{2i}) in our experimental population will also be biased, as variation in $Z_i X_{1i}$ and $Z_i X_{2i}$ is not only associated with change in the realized treatments, but also the gains to those realized treatments.

To address these issues, we experimentally compare individuals with the same set of credit cards where we might expect similar Roy (1951)–type selection, but with different proportions of debt on each credit card. Formally, let there be a set of covariates W_i with discrete support (e.g., the creditor risk set) such that the causal responses are independent from treatment intensity conditional on W_i:

A3 *Conditional Independence:* $(\beta_{1i}, \beta_{2i}, \beta_{3i}, \beta_{4i}, \beta_{5i}) \perp (X_{1i}, X_{2i}) | W_i$.

In our context, Assumption A3 rules out Roy (1951)–type selection within creditor risk sets, but not across risk sets. In other words, we require that the proportion of debt with each card issuer be as-good-as-randomly assigned with respect to the gains from treatment β_{1i} and β_{2i}, but not with respect to the initial choice of which credit cards to hold. We also require the conditional independence of β_{3i}, β_{4i}, and β_{5i} so that we can estimate the causal effects conditional on $W_i = w$ through a linear model of Y_i on $(Z_i X_{1i}, Z_i X_{2i}, X_{1i}, X_{2i}, 1)$, as described in greater detail below.

Given the conditional independence of treatment effects within creditor risk sets (Assumption A3), one might wonder whether we still need to be concerned about selection bias. In our framework, selection bias can still arise from the correlation between the potential treatment intensities X_{1i} and X_{2i} and potential outcomes within risk sets W_i. That is, we allow for the possibility that $\beta_{3i} \neq 0$ and $\beta_{4i} \neq 0$ within risk sets. We only rule out Roy (1951)–type selection on gains into the potential treatment intensities within risk sets W_i, not selection bias on levels within those risk sets. Building on our above example, our setup allows for individuals holding relatively more debt with the credit card issuers within a risk set offering relatively more generous interest write-downs and payment reductions to be unobservably different on levels (but not the gains) compared to individuals in the same risk set holding relatively less debt with those credit card issuers.

B. Estimating Equations

The goal of our empirical analysis is to estimate a weighted average of the risk set-specific causal effects of the realized treatment variables, β_1^λ and β_2^λ, for some weighting scheme λ. We begin by providing additional details on our two estimators, before formally proving that each estimator yields unbiased estimates of β_1^λ and β_2^λ for two different weighting schemes.

Matching Estimator: Our first set of estimates come from a matching estimator that allows us to impose our own weights on each risk-set specific estimate, but at the cost of statistical precision and feasibility in finite samples. We estimate these matching results using a two-step procedure. First, we estimate the effects of $Z_i X_{1i}$ and $Z_i X_{2i}$ on Y_i separately for each risk set $W_i = w$ using the following reduced form specification:

$$Y_i = \beta_{1,w} Z_i X_{1i} + \beta_{2,w} Z_i X_{2i} + \beta_{3,w} X_{1i} + \beta_{4,w} X_{2i} + \beta_{5,w} + \psi_i \tag{B.1}$$

Note that we do not require weighting superscripts in Equation (B.1) under Assumption A3. We can also residualize the included variables using additional baseline controls such as randomization strata fixed effects as needed. In our context, we residualize for both baseline covariates and randomization strata fixed effects. Using these first-step estimates, we can then construct our matching estimates using the weighted averages of the risk-set specific estimates $\beta_{1,w}$ and $\beta_{2,w}$:

$$\beta_1^M = \sum_w \beta_{1,w} \times Pr(W_i = w | Z_i = 1) \tag{B.2}$$

$$\beta_2^M = \sum_w \beta_{2,w} \times Pr(W_i = w | Z_i = 1) \tag{B.3}$$

where the weights M are equal to the fraction of treated individuals in each risk set. We calculate standard errors for β_1^M and β_2^M using the Bayesian bootstrap procedure described in the text.

Regression Estimator: Our second set of estimates comes from a regression estimator that is statistically precise and straightforward to implement in finite samples, but at the cost that the weighting scheme underlying the estimator may not be economically relevant. We estimate these regression results using the full experimental sample and the following reduced form specification:

$$Y_i = \beta_1^R Z_i X_{1i} + \beta_2^R Z_i X_{2i} + \beta_3^R X_{1i} + \beta_4^R X_{2i} + \beta_5^R + W_i' \gamma^R + \nu_i \tag{B.4}$$

where the weights R are described in greater detail below. We can also add additional baseline controls such as randomization strata fixed effects to the estimating equation as needed. In our context, we control for both baseline covariates and randomization strata fixed effects and cluster the standard errors at the examiner level.

We will now show that both the matching and regression estimators have a causal interpretation when Assumptions A1-A3 hold. We begin by showing that our matching estimator provides

unbiased estimates for the treatment effects of X_{1i} and X_{2i} within each risk set, which can then be aggregated using any researcher-imposed weighting scheme. We then show that, with an additional functional form assumption described below, our regression estimator provides unbiased estimates of a weighted average of risk set-specific treatment effects.

C. Proof of Matching Estimator

Proposition 1 Given Assumptions A1-A3, OLS estimates of Equation (B.1) provide unbiased estimates for the treatment effects of X_{1i} and X_{2i} within each risk set W_i. The constructed matching estimators in Equations (B.2) and (B.3) provide a weighted average of these risk set-specific treatment effects, where the weights are equal to the fraction of treated individuals in each risk set $Pr(W_i = w|Z_i = 1)$.

Proof of Proposition 1: Conditional on $W_i = w$, by Assumption A1 we have,

$$\begin{aligned}
E[Y_i|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{1i}Z_iX_{1i} + \beta_{2i}Z_iX_{2i} + \beta_{3i}X_{1i} + \beta_{4i}X_{2i} + \beta_{5i}|Z_i, X_{1i}, X_{2i}, W_i = w] \\
&= E[\beta_{1i}Z_iX_{1i}|Z_i, X_{1i}, X_{2i}, W_i = w] + E[\beta_{2i}Z_iX_{2i}|Z_i, X_{1i}, X_{2i}, W_i = w] \\
&+ E[\beta_{3i}X_{1i}|Z_i, X_{1i}, X_{2i}, W_i = w] + E[\beta_{4i}X_{2i}|Z_i, X_{1i}, X_{2i}, W_i = w] \\
&+ E[\beta_{5i}|Z_i, X_{1i}, X_{2i}, W_i = w]
\end{aligned}$$

By Assumptions A2 and A3,

$$\begin{aligned}
E[\beta_{1i}Z_iX_{1i}|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{1i}|Z_i, X_{1i}, X_{2i}, W_i = w]Z_iX_{1i} \\
&= E[\beta_{1i}|X_{1i}, X_{2i}, W_i = w]Z_iX_{1i} \\
&= E[\beta_{1i}|W_i = w]Z_iX_{1i}
\end{aligned}$$

Similarly, we can show

$$\begin{aligned}
E[\beta_{2i}Z_iX_{2i}|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{2i}|W_i = w]Z_iX_{2i} \\
E[\beta_{3i}X_{1i}|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{3i}|W_i = w]X_{1i} \\
E[\beta_{4i}X_{2i}|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{4i}|W_i = w]X_{2i} \\
E[\beta_{5i}|Z_i, X_{1i}, X_{2i}, W_i = w] &= E[\beta_{5i}|W_i = w]
\end{aligned}$$

Within each risk set $W_i = w$, we therefore have a linear conditional expectation model as follows:

$$E[Y_i|Z_i, X_{1i}, X_{2i}, W_i = w] = \beta_{1,w}Z_iX_{1i} + \beta_{2,w}Z_iX_{2i} + \beta_{3,w}X_{1i} + \beta_{4,w}X_{2i} + \beta_{5,w}$$

where the causal responses of interest are independent from (X_{1i}, X_{2i}) conditional on $W_i = w$:

$$\beta_{1,w} = E[\beta_{1i}|W_i = w]$$
$$\beta_{2,w} = E[\beta_{2i}|W_i = w]$$

Thus, an OLS regression of Y_i on $(Z_iX_{1i}, Z_iX_{2i}, X_{1i}, X_{2i}, 1)$ in Equation (B.1) yields unbiased estimates for $\beta_{1,w}$ and $\beta_{2,w}$. We refer to $\beta_{1,w}$ as the average causal response to X_{1i} within risk set $W_i = w$, and $\beta_{2,w}$ as the average causal response to X_{2i} within risk set $W_i = w$.

Finally, Equations (B.2) and (B.3) state that the matching estimators β_1^M and β_2^M are constructed as weighted averages of risk set-specific $\beta_{1,w}$ and $\beta_{2,w}$, respectively, where the weights are equal to the fraction of treated individuals in each risk set $W_i = w$.

D. Proof of Regression Estimator

In addition to our identifying assumptions A1-A3, we require the following functional form assumption to identify the causal effects of the treatments using our regression estimator:

A4 *Linear Relationship Between Covariates:*

$$E[Z_iX_{1i}|Z_iX_{2i}, X_{1i}, X_{2i}, W_i] = \pi_{2,1}Z_iX_{2i} + \pi_{3,1}X_{1i} + \pi_{4,1}X_{2i} + \pi_{5,1} + W_i'\Pi_1$$
$$E[Z_iX_{2i}|Z_iX_{1i}, X_{1i}, X_{2i}, W_i] = \pi_{1,2}Z_iX_{1i} + \pi_{3,2}X_{1i} + \pi_{4,2}X_{2i} + \pi_{5,2} + W_i'\Pi_2.$$

Assumption A4 ensures that the conditional expectation function $E[Z_iX_{1i}|X_{1i}, X_{2i}, Z_iX_{2i}, W_i]$ is linear in $(X_{1i}, X_{2i}, Z_iX_{2i}, 1, W_i)$ and the conditional expectation function $E[Z_iX_{2i}|X_{1i}, X_{2i}, Z_iX_{1i}, W_i]$ is linear in $(X_{1i}, X_{2i}, Z_iX_{1i}, 1, W_i)$. A4 would be violated if, for example, the correlation between the covariates X_{1i} and X_{2i} differs across risk sets W_i.

Proposition 2 Given Assumptions A1-A4, the regression estimators β_1^R and β_2^R in Equation (B.4) identify a weighted-average of the risk-set-specific effects of X_{1i} and X_{2i} on Y_i, where the weights are proportional to the variation in Z_iX_{1i} and Z_iX_{2i} in each risk set.

Proof of Proposition 2: By the Frisch-Waugh-Lovell theorem, the risk set-specific OLS estimates of Equation (B.1) are equal to

$$\beta_{1,w} = \frac{cov(Y_i, \widetilde{Z_iX_{1i}}|W_i = w)}{var(\widetilde{Z_iX_{1i}}|W_i = w)}$$
$$\beta_{2,w} = \frac{cov(Y_i, \widetilde{Z_iX_{2i}}|W_i = w)}{var(\widetilde{Z_iX_{2i}}|W_i = w)} \tag{B.5}$$

where the conditional $\widetilde{Z_iX_{1i}}|(W_i = w)$ denotes the residual of Z_iX_{1i} over the linear projection of Z_iX_{1i} on the space spanned by the covariates $X_{1i}, X_{2i}, Z_iX_{2i}$, and 1 <u>within</u> the risk set $W_i = w$, $Z_iX_{1i} - E^*[Z_iX_{1i}|X_{1i}, X_{2i}, Z_iX_{2i}, 1, W_i = w]$. $\widetilde{Z_iX_{2i}}|(W_i = w)$ similarly denotes the residual of

Z_iX_{2i} over the linear projection of Z_iX_{2i} on the space spanned by the covariates $X_{1i}, X_{2i}, Z_iX_{1i}$, and 1 within the risk set $W_i = w$, $Z_iX_{2i} - E^*[Z_iX_{2i}|X_{1i}, X_{2i}, Z_iX_{1i}, 1, W_i = w]$.

By the Frisch-Waugh-Lovell theorem, the full-sample OLS estimates of Equation (B.4) are equal to

$$\begin{aligned}\beta_1^R &= \frac{cov(Y_i, \widetilde{Z_iX_{1i}})}{var(\widetilde{Z_iX_{1i}})} \\ \beta_2^R &= \frac{cov(Y_i, \widetilde{Z_iX_{2i}})}{var(\widetilde{Z_iX_{2i}})}\end{aligned} \tag{B.6}$$

where (with some abuse of notation) the unconditional $\widetilde{Z_iX_{1i}}$ denotes the residual of Z_iX_{1i} over the linear projection of Z_iX_{1i} on the space spanned by the covariates $X_{1i}, X_{2i}, Z_iX_{2i}, 1$, and W_i, $Z_iX_{1i} - E^*[Z_iX_{1i}|X_{1i}, X_{2i}, Z_iX_{2i}, 1, W_i]$. $\widetilde{Z_iX_{2i}}$ similarly denotes the residual of Z_iX_{2i} over the linear projection of Z_iX_{2i} on the space spanned by the covariates $X_{1i}, X_{2i}, Z_iX_{1i}, 1$, and W_i, $Z_iX_{2i} - E^*[Z_iX_{2i}|X_{1i}, X_{2i}, Z_iX_{1i}, 1, W_i]$.

Conditional on $W_i = w$, by Assumption A4, the residuals $\widetilde{Z_iX_{1i}}$ and $\widetilde{Z_iX_{2i}}$ from Equation (B.6) are equal to the residuals $\widetilde{Z_iX_{1i}}|W_i = w$ and $\widetilde{Z_iX_{2i}}|W_i = w$ from Equation (B.6) for a given risk set $W_i = w$. To see why this equivalence holds, note that the residual $\widetilde{Z_iX_{1i}}$ from Equation (B.6) can be expressed as

$$\begin{aligned}\widetilde{Z_iX_{1i}} &= Z_iX_{1i} - E[Z_iX_{1i}|X_{1i}, X_{2i}, Z_iX_{2i}, W_i] \\ &= Z_iX_{1i} - (\pi_{2,1}Z_iX_{2i} + \pi_{3,1}X_{1i} + \pi_{4,1}X_{2i} + \pi_{5,1} + \underbrace{\Pi_{1,w}}_{\text{constant on } W_i=w}) \\ &= Z_iX_{1i} - E[Z_iX_{1i}|X_{1i}, X_{2i}, Z_iX_{2i}, 1, W_i = w] \\ &= \widetilde{Z_iX_{1i}}|(W_i = w)\end{aligned}$$

We can similarly show that the residual $\widetilde{Z_iX_{2i}}$ from Equation (B.6) can be expressed as

$$\widetilde{Z_iX_{2i}} = \widetilde{Z_iX_{2i}}|(W_i = w)$$

Finally, by the law of iterated expectations, the regression estimator β_1^R can be written as a

weighted average of risk set-specific effects of the treatment X_{1i}:

$$\begin{aligned}
\beta_1^R &= \frac{E[\widetilde{Z_i X_{1i}} Y_i]}{E[(\widetilde{Z_i X_{1i}})^2]} = \frac{E[E[\widetilde{Z_i X_{1i}} Y_i | W_i = w]]}{E[E[\widetilde{Z_i X_{1i}}^2 | W_i = w]]} \\
&= \frac{E_w[\beta_{1,w} var(\widetilde{Z_i X_{1i}} | W_i = w)]}{E_w[var(\widetilde{Z_i X_{1i}} | W_i = w)]} \\
&= E_w \left[\beta_{1,w} \times \left(\frac{var(\widetilde{Z_i X_{1i}} | W_i = w)}{E_w[var(\widetilde{Z_i X_{1i}} | W_i = w)]} \right) \right] \\
&= \sum_w \beta_{1,w} \times \underbrace{\left(\frac{var(\widetilde{Z_i X_{1i}} | W_i = w)}{E_w[var(\widetilde{Z_i X_{1i}} | W_i = w)]} \times Pr(W_i = w) \right)}_{\text{weight on } \beta_{1,w}}
\end{aligned}$$

Given the same assumptions, the regression estimator β_2^R can be similarly written as a weighted average of risk set-specific effects of the second treatment X_{2i}:

$$\beta_2^R = \sum_w \beta_{2,w} \times \underbrace{\left(\frac{var(\widetilde{Z_i X_{2i}} | W_i = w)}{E_w[var(\widetilde{Z_i X_{2i}} | W_i = w)]} \times Pr(W_i = w) \right)}_{\text{weight on } \beta_{2,w}}$$

□

Comment on the Weights for β_1^R and β_2^R: The covariate-adjusted weights underlying β_1^R and β_2^R are increasing in the variation in each treatment intensity in each risk set $W_i = w$ and the sample share of each risk set $W_i = w$. The covariate-adjusted weights underlying β_1^R and β_2^R are not necessarily the same, as the relative variation in each treatment intensity $var(\widetilde{Z_i X_{1i}} | W_i = w) / E_w[var(\widetilde{Z_i X_{1i}} | W_i = w)]$ and $var(\widetilde{Z_i X_{2i}} | W_i = w) / E_w[var(\widetilde{Z_i X_{2i}} | W_i = w)]$ may differ across risk sets W_i. This issue is problematic to the extent that the relative variation in each treatment intensity is correlated with the risk set-specific gains from treatment $\beta_{1,w}$ and $\beta_{2,w}$. In contrast, the weights underlying β_1^M and β_2^M can be chosen by the researcher to be identical, albeit at the possible cost of statistical precision in finite samples. We therefore present estimates from both our regression and matching estimators throughout.

Appendix B References

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