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The effect of interest rate caps on bankruptcy: Synthetic control evidence from recent payday lending bans

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ABSTRACT

Citing consumer protection concerns, several states have recently enacted interest rate caps on small loans. After cataloguing the history of such legislation, we test whether these laws caused a decrease in the number of payday-lending establishments and subsequently prompted variation on incidence of bankruptcy filings. To motivate a causal interpretation of our estimates, we create a synthetic control that serves as a counterfactual from which we estimate the aggregate treatment effect of these interest rate ceilings. Importantly, we estimate the treatment effect for each period after the imposition of the cap, yielding novel insights about the dynamic heterogeneity in the relationship between payday-loan access and bankruptcy. Our results show payday-lending establishments drop by approximately 100%–a banishment of the industry. We find no short-run or long-run effects of these bans on bankruptcy. The range of our estimates allows us to rule out magnitudes that were documented in several previous studies.

1. Introduction

Payday loans are unsecured short-term high-interest loans, with a maturity of about two weeks and a typical annualized percentage rate (APR) of 390%, possibly more if the loan is rolled over. To apply for a loan, a borrower must show a valid ID, a bank statement, and proof of stable income, e.g., several successive pay stubs of some kind. The potential borrower writes a check for the full amount (principal plus interest and fees), post-dated for the maturity date of the loan. The lender cashes the check on the loan due date, unless the borrower rolls over the loan for an additional fee, which happens approximately 80% of the time (Burke et al., 2014)

Since the mid-90s, payday lenders have been growing in number (Stegman, 2007), and now total more locations than McDonalds (Bennett, 2019). Citing consumer protection concerns, legislatures around the country have responded. At the federal level, in 2006, the federal government enacted the Military Lending Act (MLA), which capped unsecured loans to military members at 36% APR, effectively banning payday lending for military personnel.[3] The 2010 Dodd-Frank Act created the Consumer Financial Protection Bureau (CFPB), a bureau that polices the industry. We are unaware of any studies on the effects of the creation of the CFPB on consumer credit markets. At the state level, states have enacted various restrictions on payday lending, from limiting the number of rollovers, e.g., Washington, to capping the APR at 36%, e.g. New Hampshire.[4]

Restrictions on payday lending may have some merit. There is evidence that access to payday loans leads consumers to do the following: delay health care, have utilities cut off, pay their rent late, increase their reliance on food assistance programs, go delinquent on child support payments, lower job performance, file for

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[3] For a peer-reviewed study on the MLA, see Carter and Skimmyhorn (2017), who, in addition to analyzing its effects, argue that it was effective insofar as it insulated the military from payday lending. Hence, the interest rate cap effectively banned the US military from using payday loans since lenders cannot profitably operate at such a relatively low APR (Flannery and Samolyk, 2005). Therefore, following the norm in the literature, e.g., Bhutta et al. (2016), Dobridge (2018), and others, we use 'interest rate cap' in this payday lending context and 'ban' and 'effective ban' interchangeably throughout this paper.

[4] See Appendix A.6 for a history of payday lending legislation in each state and the District of Columbia.

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bankruptcy, consume more alcohol, commit more crime, and even contemplate suicide.[5]

Despite the merits of having some restrictions on payday lending, there may be some unintended consequences. Recent evidence has shown that payday loan restrictions have pushed consumers into less desirable forms of finance, including bank overdraft fees, pawnshop loans, and bill delinquency–the latter entailing the hassle of debt collectors.[6]

Some researchers have attempted to resolve these conflicting findings by drilling down into the heterogeneity of the populations of likely payday loan users. For example, Morse (2011) and Dobridge (2018) focus their analyses of payday loan availability on those populations who are hit by an extreme weather event. Carter and Skimmyhorn (2017) narrow part of their analysis to young military members with low AFQT scores.

Other researchers have narrowed their focus to applicants of payday loans, analyzing administrative panel data: the 'applicant' aspect of such data allow for a regression discontinuity design, while the panel nature of the data allow for an analysis of dynamic heterogeneity. Bhutta et al. (2015) find no effect on credit scores–short-term or long-term. Skiba and Tobacman (2019) find that "bankruptcies 2 years after the first payday loan are generally larger than the 1-year effects, statistically and economically." Using UK data, Gathergood et al. (2019) find that consumers who are "barely approved" for a payday loan see their financial situation deteriorate as time passes relative to the applicants who are "barely rejected," even up to two years out.[7]

The credibility of these most recent studies comes from their impeccable degree of internal validity. However, this credibility inevitably comes at the cost of external validity: what effects do payday loans have on those borrowers who are "far away" from the thresholds? A fuzzy regression discontinuity design is an instrumental variable identification strategy. As such, it estimates the local average treatment effect (LATE). Furthermore, by focusing on the usage of payday loans rather than the broader measure of access, there are potential causal channels that are missed. For instance, forward-looking consumers may take the prevalence (and convenience) of payday loans into account when formulating their optimal consumption plan. The removal of such loans would alter these consumers' behavior with respect to smoothing, debt, delinquency, and bankruptcy.

Our study fills a gap in the literature: we estimate the effect of state payday lending bans on bankruptcy–formal and informal–taking full account of the dynamic heterogeneity mentioned above. We answer the questions: what are the short-run statewide effects of payday lending bans? Do the short-run effects differ from the long-run effects? Our paper provides a state-level analogue to Gathergood et al. (2019) and Skiba and Tobacman (2019).

Between 2009 and 2011 four states–Arizona, Arkansas, Montana, and New Hampshire–enacted interest rate caps, e.g., small loans cannot exceed 36% APR. Presumably, the policy objective of such caps is consumer protection: payday loans can ensnare borrowers into a type of debt trap. The initial loan cannot be repaid and is therefore continually rolled over, incurring fees each time, ultimately leading to financial ruin. Capping interest rates will debilitate the industry, which, in turn, will protect consumers from falling into a debt trap, sparing them from bearing the real costs mentioned above.

Our research strategy in this paper is twofold. As a "first-stage" analysis, we examine whether interest rate caps do indeed function as an effective ban on payday lending. We estimate a difference-in-differences (DD) regression on the number of payday lending establishments. We then perform DD analyses on two separate, nationally-representative surveys that ask about payday lending usage. This first-stage exercise is not trivial: sometimes states pass heavily-restrictive legislation, only to have it circumvented shortly thereafter, e.g., Ohio. We supplement this statistical evidence with qualitative evidence. We find that payday loan usage drops across the board in the treated states.

As a "second stage," we test whether the caps have a causal effect on bankruptcy filings and delinquencies ("informal bankruptcy"). To do so, we exploit the spatio-temporal variation in payday lending bans. We perform an OLS-DD analysis, comparing the abovementioned treated states (as well as others) to several sets of control states. While the results of the DD analysis are statistically zero, our 95% confidence interval allows us to confirm the bankruptcy magnitudes found in Morgan et al. (2012), but rule out those found in Skiba and Tobacman (2019).

We then employ the synthetic control method (SCM) of Abadie et al. (2010), which relaxes the OLS-DD assumptions of parallel trends and equal weights across control units. The SCM lets the data choose the control unit as a weighted average from a pool of potential control states. The weights are chosen optimally with respect to minimizing the pre-treatment distance between the treated unit and the synthetically-created control unit.

Of the four treated states, we present New Hampshire as the treated unit for a detailed case study.[8] New Hampshire is noteworthy because during the study period (2001–2016), it was surrounded by perennially-banning states, meaning that the ban completely shut New Hampshirites out of the storefront payday lending market. This unique geo-legistlative position precludes any cross-state contamination of our results, see, e.g., Melzer (2011). New Hampshire also implemented its cap earliest of the four states mentioned above and therefore has the greatest number of post-treatment quarters available. The length of the post-treatment period allows us to best estimate the treatment effect as far as eight years out. We iteratively run the SCM for each post-implementation quarter, thereby giving us a full range of estimates with which we can assess the potential dynamic heterogeneity of the ban. After presenting the case of New Hampshire, we pool the four treated states using the algorithm developed by Galiani and Quistorff (2017).

Our SCM results indicate that payday lending bans, manifested as relatively low APR caps, do not cause statewide changes in bankruptcies, formal or informal, in the short-run or the long-run. The range of our post-treatment effects rules out the bankruptcy magnitudes found in both Morgan et al. (2012) and Skiba and Tobacman (2019). Moreover, visual plots of the treatment effect show no discernable pattern, contrary to the suggestive evidence of Gathergood et al. (2019) as applied to the US.

[5] For utilities, rent, and delayed healthcare see Melzer (2011), but cf. Dobridge (2018). For an increase of use of food assistance and going delinquent on child support payments see Melzer (2018). For lower job performance see Carrell and Zinman (2014), but cf. Carter and Skimmyhorn (2017). For bankruptcy see Morgan et al. (2012) and Skiba and Tobacman (2019). For alcohol expenditures, see Cuffe and Gibbs (2017). For crime see Xu (2016), but cf. Morse (2011). For suicide risk see Lee (2017).

[6] Edmiston (2011) shows that the typical bank overdraft fees (and credit card overcharges) are often higher than a payday loan. For evidence that payday lending restrictions lead to more involuntary bank closures–which are almost always the result of delinquent overdrafts–see Gathergood et al. (2019) and Bhutta et al. (2016), but cf. Campbell et al. (2012). For overdraft fees, see Stegman (2007), Melzer and Morgan (2015), Morgan et al. (2012), and Zinman (2010). For pawnshop loans, see Bhutta et al. (2016) and Ramirez (2017). For bill delinquency, see Desai and Elliehausen (2017); for the hassle of bill collectors, see Morgan et al. (2012)

[7] In the UK, online lenders dominate the market for payday loans. See Liberman et al. (2020) for a similar RDD-based study that details the UK payday lending market.

[8] The other states are each immediately available upon request. We also ran the analysis on South Dakota (SD), which banned in 2016. The visual results for SD are in the Appendix Figures A.1 and A.2.

Bankruptcy filings and delinquencies are imperfect proxies of the disutility that's potentially brought about by payday loans (or their banishment). Still, filing for bankruptcy protection represents the final stop that breaks the debt spiral—the culmination of the entrapment, as it were; delinquencies are perhaps a penultimate stop in the debt trap story. Bankruptcy and delinquency data have the added benefit of having a high degree of data quality and availability. Formal bankruptcy filings are collected from court records in a consistent and centralized manner; they're available at a quarterly frequency, which increases variation and the precision of our estimates.

That said, our data are highly aggregated and not administrative. This could account for the lack of congruency with some of the studies mentioned above. In particular, we measure payday loan access, proxied by state laws, rather than loan application or approval. To address the discrepancy of our findings with those mentioned above, we analyzed the first three waves of the National Financial Capability Survey (NFCS) administered by FINRA (and the US Department of the Treasury). The NFCS dataset is a nationally-representative survey that asks about payday loan usage as well as bankruptcy. To identify payday users, we filter out only those respondents who have taken out a payday loan in the past. We then perform DD regressions on bankruptcy and delinquency-related outcomes.

Our survey results show that while bankruptcy is unaffected by the bans mentioned above, credit card late payments increase, which corroborates some of the findings above, namely that payday loan bans push borrowers into alternative forms of finance. Thus, it's possible that payday borrowers near the loan-approval threshold see their credit score worsened, file for bankruptcy, etc. But away from the threshold, we see would-be borrowers substitute to alternative sources of finance, which could stave off going delinquent or filing for bankruptcy.

The paper proceeds as follows. The second section reviews the literature that most directly links payday lending to bankruptcies. The third section briefly describes the state-level regulatory framework in the United States. The fourth section describes the variables, data, and the empirical methodology. The fifth section describes the results. The sixth section concludes and presents avenues of future research.

2. Previous literature

There is a lot of research on the effects of payday lending. In this section we limit the scope of our review to those studies that are most closely related to ours, that is, only those studies that examine the link between payday loan access and bankruptcy, both formal (Chapters 7 and 13), informal (loan delinquencies), and credit scores, which can be thought of as a type of aggregation of the formal and informal measures.

Regarding formal bankruptcy, Lefgren and McIntyre (2009) analyze why bankruptcy filings differ across states since bankruptcy law is determined at the federal level. The authors find that demographic characteristics account for the bulk of the variation, leaving little to no explanatory room for strictness of payday legislation and usury laws. Stoianovici and Maloney (2010) exploit state variation in payday lending legislation between 1990 and 2006. They find no effect on bankruptcy rates, a result confirmed by Hynes (2012) for the period 1998–2009. Moreover, Stoianovici and Maloney perform Granger causality tests and find that the number of payday lending stores does not Granger cause bankruptcies.

The finding that payday lending restrictions do not affect bankruptcies is contrasted with the findings of Morgan et al. (2012). These authors exploit state-time variation in payday lending legislation and find that payday-banning states see a decrease in Chapter 13 bankruptcy rates (for some specifications) relative to states that did not ban. Their findings suggest that the ban was helpful to consumers: bankruptcies were high; legislators passed the interest rate cap; bankruptcies fell. However, the authors caution against this knee-jerk interpretation of the results. They also find that overdraft fees and complaints against debt collectors also increase in the payday banning states. Therefore, the welfare effects of the payday ban are unclear.

Carter and Skimmyhorn (2017) exploit the exogenous variation created by random military assignment and the imposition of the 36% APR cap of the MLA. They find that bankruptcy rates are mostly unchanged for military members who are randomly assigned to 'payday banning' states relative to those who are assigned to 'payday permissive' states. They find a negative effect on formal bankruptcy for some specifications, which corroborates Hynes (2012), who finds a negative relationship in counties with large military communities. Regarding credit scores, these authors find positive effects of access in their diffence-in-differences identification strategy (null for others). These results withstand a battery of testing on the subpopulations who are most likely to use payday loans. These authors ultimately suggest that the concerns about payday lending are "much ado about nothing."

Regarding informal bankruptcy, Zinman (2010) finds that Oregon's 2007 interest rate cap led to consumers stating that they plan to pay their bills late relative to consumers in Washington, which is a 'permissive' payday lending state. Morse (2011) compares home foreclosure rates within the same state, California, after particular areas are hit by a natural disaster. She finds that access to a payday lender mitigates home foreclosure rates.

Melzer (2011) exploits the spatio-temporal variation in delinquencies (and credit scores) at the zip-code level, which controls for state-level shocks. He compares the real financial hardship, e.g., having utilities cut off, trouble paying rent, etc., of residents who are in the zip code of a banning state but bordering a 'payday permissive' state to zip codes in the same state that are far from the border. He shows evidence that borrowers can and do cross state lines to obtain a payday loan. Melzer (2018) also examines hardship, including child support delinquency. Both of Melzer's studies show that access to payday loans increase informal delinquency and real costs.

Bhutta (2014) uses the zip-code identification strategy above and finds that delinquencies and credit scores are unaffected by proximity to payday access. Desai and Elliehausen (2017) examine credit delinquencies in the counties of the banning states of Georgia, North Carolina, and Oregon before and after each state's respective ban to neighboring counties that are located in states that did not ban. They generally find no effect, with Georgia's ban being an exception: revolving credit (e.g., credit cards), delinquencies increase, but installment credit (e.g., auto loans), delinquencies decrease. Carter and Skimmyhorn (2017) find no effect on 'balance in collection status' and major derogatory payments (60 days past due).

The two most recent papers on the measures relevant for our paper, bankruptcy and deliquency, are Skiba and Tobacman (2019) and Gathergood et al. (2019), respectively. Skiba and Tobacman use a proprietary dataset from a payday lender in Texas and link the data to bankruptcy filings. These authors compare the bankruptcy outcomes of payday borrowers who were 'barely' approved for a payday loan to those who were 'barely' rejected. They find that being approved for a loan leads to a higher rate of bankruptcy filing. Interestingly, they find that bankruptcy rates are statistically higher two years after an initial payday loan application than they are only one year after. The authors attribute this dynamic heterogeneity to payday loan approval causing illiquidity issues for households in the short-run, which leads to insolvency in the longer-run.

Table 1
Summary of relevant literature.

Author(s)	Year	Measure of Payday Loan Access	Correlation of Access and Distress
Panel a: Bankruptcy Filings			
Lefgren and McIntyre	2009	Legal restrictions	None
Stoianovici and Maloney	2010	Number of lenders	None
Hynes	2012	Legal restrictions	Mixed
Morgan, Strain, Seblani	2012	Legal restrictions	Mixed
Carter and Skimmyhorn	2017	Legal restrictions	Mixed
Skiba and Tobacman	2019	Loan approval	Positive
Panel b: Delinquencies and Defaults			
Zinman	2010	Legal restrictions	Negative
Morse	2011	Number of lenders	Negative
Bhutta	2014	Legal restrictions	None
Carter and Skimmyhorn	2017	Legal restrictions	None
Desai and Elliehausen	2017	Legal restrictions	Mixed
Melzer	2018	Legal restrictions	Positive
Gathergood et al	2019	Loan approval	Mixed
Panel c: Credit Scores			
Bhutta	2014	Legal restrictions	None
Bhutta et al	2015	Loan approval	None
Carter and Skimmyhorn	2017	Legal restrictions	Mixed
Liberman et al	2020	Loan application, approval	Mixed

Notes: Higher bankruptcy and delinquency is more distress, while higher credit score is less distress. Mixed results and qualifications are explained in this note. Stoianovici and Maloney (2010) also examine legal restrictions and find no effect. Skiba and Tobacman (2019) is extended by Bhutta et al. (2015) by examining credit scores. Hynes (2012) shows that the correlation is negative in counties with large military communities, a finding corroborated by Carter and Skimmyhorn (2017) for some specifications (null for others). Morgan et al. (2012) find a positive effect without state trends—null when they're included. Zinman (2010) finds evidence that banned payday borrowers pay their bills late, which, we argue, is correlated with bill delinquency. Desai and Elliehausen (2017) find that there may be some mixed evidence for the state of Georgia. Gathergood et al. (2019) is for the UK. Regarding credit scores, Carter and Skimmyhorn (2017) find either null effects or a positive relation. Liberman et al. (2020) show with UK data that credit score drops with a loan application, but remains unchanged with a loan approval.

Gathergood et al. (2019) provide strong evidence on the effect of payday lending on delinquencies. These authors obtained payday loan data that includes virtually every (online) payday loan approval in the entire United Kingdom for the years 2012 and 2013. The authors link this payday data to personal credit data. Exploiting the exogenous variation created by a threshold in payday loan approval, the authors find that delinquencies decline immediately after the loan is approved. But, in the longer-run the situation is reversed: payday lending approval causes an increase in credit delinquencies. Liberman et al. (2020) use UK data from a storefront payday lender in a regression discontinuity design. They find that applying for a payday loan creates a documentable stigma, which lowers the applicant's credit score. However, being approved doesn't further worsen the credit score. It is worth noting that in the UK, the payday lending market is dominated by online lenders. Table 1 summarizes our review of the most pertinent literature.

Our paper makes several contributions to this divided literature. Our findings complement the findings of Skiba and Tobacman (2019) and Gathergood et al. (2019) with respect to external validity. The Skiba and Tobacman (2019) data come from one payday lender in one state, which leaves open the possibility that their results hold only for a particular population, e.g., maybe the payday lender attracts applicants that differ systematically from the rest of the population of payday borrowers. While the Gathergood et al. (2019) data come from virtually *all* payday lenders, we don't know the effect of payday lending on those payday borrowers who are far away from the loan-approval threshold. In contrast to both of these studies, state-level payday bans apply to all payday applicants, regardless of their proximity to the loan-approval threshold. Furthermore, analysis at the state level would capture any effects of payday lending outside of actually applying for a loan.[9]

Like several previous studies mentioned above, we employ difference-in-differences as an identification strategy. One concern with this strategy is that there may some unobserved heterogeneity between the treated units and the control units, ultimately confounding the causal effect. There is an additional concern that control units recieve equal weights—a consequence of OLS estimation. In this paper, we address these concerns with a data-driven method to choose an optimal control unit as a weighted average from a donor pool of untreated states. We do so by employing the synthetic control method, which lets the data choose how to weight the control states with respect to the minimization of pre-treatment fit, rather than the minimization of ordinary least squares.[10]

What's more, our paper incorporates an analysis of states–Arizona, Arkansas, Montana, and New Hampshire (and South Dakota in the Appendix)–that were hitherto not investigated because samples end in 2006, e.g., Bhutta (2014). Additionally, we analyze and quantify the effect that state-level bans have on usage using several data sources. Furthermore, we analyze a nationally-representative survey of self-reported payday loan users. Finally, we catalogue (see our appendix) the history of payday legislation in each state, providing a central source for future researchers.

3. Regulatory framework

Before describing the empirical framework, we first discuss the basis on which we select the donor pool, that is, the set of states from which we draw to create the synthetic control unit. Table 2 summarizes the underlying reasons for the classification of all 50 states and Washington DC into control, treated, and excluded categories. The control states include 30 states where payday lending is permissible during the period 2001–2016 (hereafter referred to

[9] Of course, there is a "price" to pay for this increase in external validity and scope of causality, i.e., identifying the causal effect at the aggregate level is much more difficult.

[10] This, too, comes at a cost: data-identified control units run the risk of pre-treatment overfitting. In our appendix we address this using the artificial counterfactual (ArCo) method of Carvalho et al. (2018).

Table 2
Treated states and selection of control units.

Category	States	Reason for Categorization
Control States (30)	Alabama (AL)	Permitted; authorized and regulated in 2003
	California (CA)	Permitted; authorized and regulated in 1996
	Delaware (DE)	Permitted; authorized and regulated since 1987
	Florida (FL)	Permitted; regulated in 2001
	Hawaii (HI)	Permitted; regulated in 1999
	Idaho (ID)	Permitted; regulated in 2003
	Illinois (IL)	Permitted; regulated in 2005
	Indiana (IN)	Permitted; authorized and regulated in 2002
	Iowa (IA)	Permitted; authorized in 1995
	Kansas (KS)	Permitted; authorized and regulated in 1993
	Kentucky (KY)	Permitted; authorized and regulated in 1998
	Louisiana (LA)	Permitted; authorized and regulated in 2000
	Michigan (MI)	Permitted; authorized and regulated in 2005
	Minnesota (MN)	Permitted; authorized and regulated in 1995
	Mississippi (MS)	Permitted; authorized and regulated in 1998
	Missouri (MO)	Permitted; authorized and regulated in 1991
	Nebraska (NE)	Permitted; authorized and regulated in 1994
	Nevada (NV)	Permitted; regulated in 2005
	New Mexico (NM)	Permitted; regulated in 1955
	North Dakota (ND)	Permitted; authorized and regulated in 2001
	Ohio (OH)	Permitted; authorized and regulated in 1995
	Oklahoma (OK)	Permitted; authorized and regulated in 2003
	Rhode Island (RI)	Permitted; authorized and regulated in 2001
	South Carolina (SC)	Permitted; authorized and regulated in 1998
	Tennessee (TN)	Permitted; authorized and regulated in 1997
	Texas (TX)	Permitted; authorized and regulated in 1997
	Utah (UT)	Permitted; regulated in 1999
	Washington (WA)	Permitted; authorized and regulated in 1995
	Wisconsin (WI)	Permitted; regulated in 2010
	Wyoming (WY)	Permitted; regulated in 1996
Treated States (4)	Arizona (AZ)	Restricted in 2010
	Arkansas (AR)	Restricted in 2011
	Montana (MT)	Restricted in 2010
	New Hampshire (NH)	Restricted in 2009
Excluded States (17)	Alaska (AK)	Payday lending was illegal until 2005
	Colorado (CO)	Imposed partial restrictions in 2010
	Connecticut (CT)	Restricted since 1949
	District of Columbia (DC)	Restricted in 2007; non-availability of data
	Georgia (GA)	Restricted in 2004
	Maine (ME)	Permitted but restricted
	Maryland (MD)	Restricted under usury law
	Massachusetts (MA)	Restricted under small loan act
	New Jersey (NJ)	Restricted under consumer loan act
	New York (NY)	Restricted under state banking law
	North Carolina (NC)	Restricted in 2001; banned in 2005
	Oregon (OR)	Restricted partially in 2007
	Pennsylvania (PA)	Restricted under discount company act
	South Dakota (SD)	Restricted in 2016
	Vermont (VT)	Restricted under small loan law
	Virginia (VA)	Permitted; unauthorized until 2002
	West Virginia	Restricted under small loan act

Notes: In Appendix Table A.6 we give a highly-detailed review of payday restrictions in each state.

as 'period of interest' or 'study period'). The treated states are: Arizona, Arkansas, Montana, and New Hampshire. South Dakota also passed a 36% APR cap in 2016, but since this falls outside the study period, we do not include it in the main analysis. In the appendix we include a synthetic control analysis of South Dakota. The list of excluded states includes the remaining 17 states, which were removed from our analysis mainly due to the existence of payday lending restrictions during the study period (either fully or partially).

We substantiate Table 2 by providing additional state-specific details on the history of payday lending regulations in an Table A.6 in the appendix. The state-specific payday lending information has been collected from a thorough and comprehensive review of annual state legislations in the HeinOnline and Lexis Advance databases in combination with the existing payday literature and online media evidence. To the best of our knowledge, this is the first study to provide a detailed overview of the history of payday lending regulations for all 50 states (plus DC) throughout time going as far back as the early 1990s, and sometimes earlier.

Fig. 1 provides the US map illustrating the status of payday lending regulation across all states as of the end of our study period. While 16 states completely restrict payday lending activities, three states (Oregon, Colorado, and Maine) allow the operation of the business under certain restrictions. The remaining 32 states (shaded in green) permit payday lending.

Fig. 2 provides a cartographic representation of the Table 1 information.[11] The treated states are shaded in dots, the control states are shaded in green, and the excluded states are shaded in purple. The control group includes the 30 states where payday lending is permissible, at least since the first year of our study

[11] Our Fig. 2 is an updated version of Bhutta et al. (2015)'s map (see their Figure 1, pg. 232). See also Bhutta (2014)'s Figure 1 on pg. 232 and his online appendix. See also Dobridge (2018), whose classification of 'always banned', 'always legal', 'banned', and 'legalized' is similar to ours.

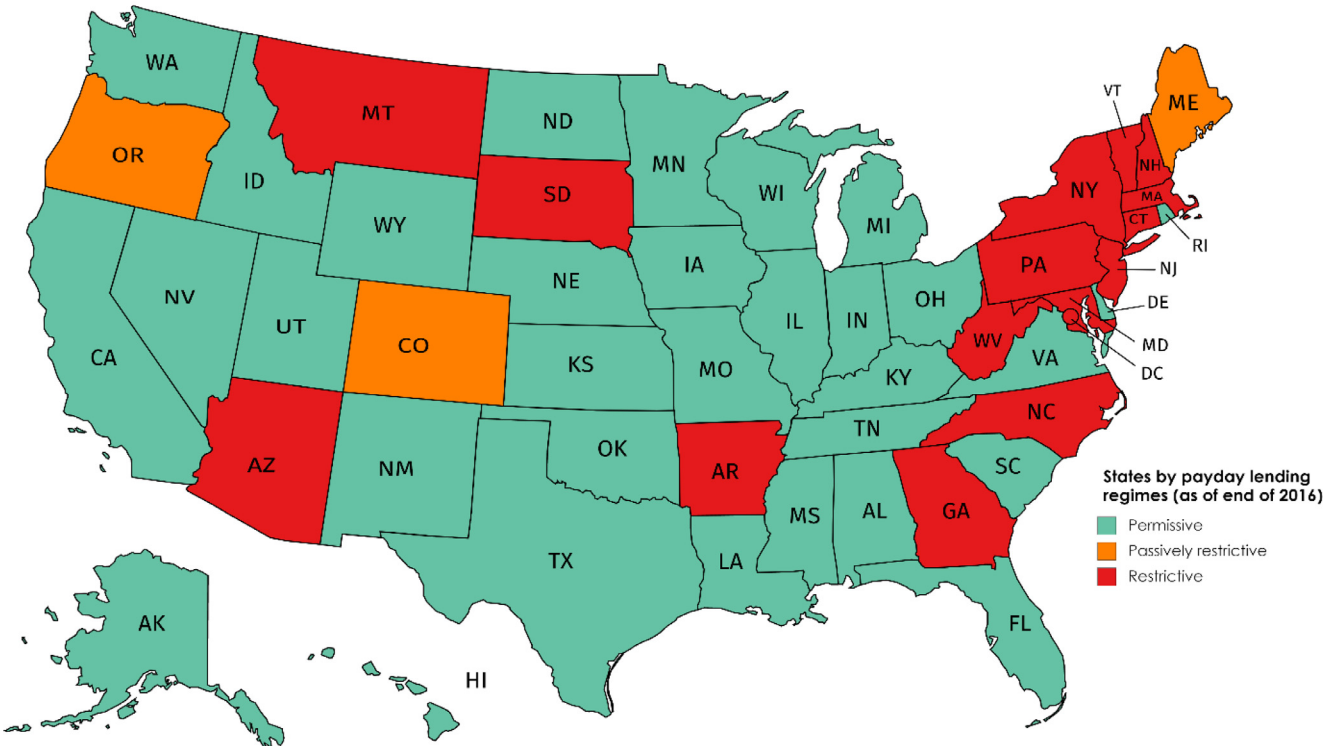

Fig. 1. Classification of US States by Payday Lending as of the end of 2016.

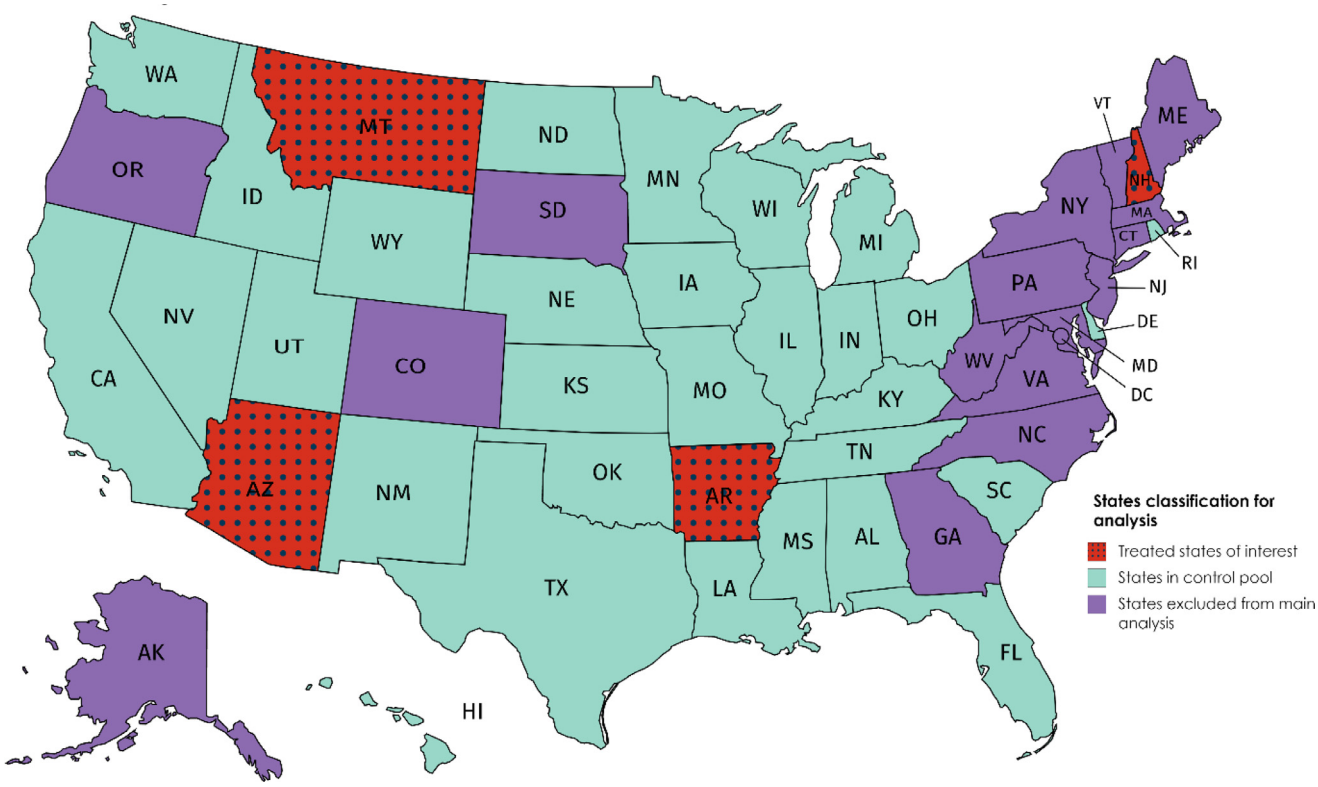

Fig. 2. Classification of US States into treatment-, control-, and excluded states.
Notes: Virginia legalized payday lending in 2002, which is only one year into our study period. Our main results are unchanged if we include Virginia as a control state and run the analysis for 2002–2016. We prefer eliminating Virginia and running the analysis from 2001 - 2016 to maximize the number of observations for other states. South Dakota (SD) passed an interest rate cap of 36% APR in 2016. Hence, because SD received the "treatment" we exclude them from the list of control states because it is conceptually possible that even the consideration of passing a payday lending ban could have altered consumer behavior. Nonetheless, for the sake of robustness, we checked whether the inclusion of SD affects our results; it does not. In the appendix we perform a synthetic control analysis of SD. Nominally, Maine is only "partially" restrictive, but the industry is effectively banned there, including online payday lending.

period, that is, 2001. It's important to note that some states in the control group may have authorized and/or initiated the regulation of payday lending activities during years within our study period. However, there is evidence that confirms the presence of payday lending business prior to the first year of our study period.[12] These states (along with effective authorization/ regulation years) include Alabama (2003), Florida (2001), Idaho (2003), Illinois (2005), Indiana (2002), Michigan (2005), Nevada (2005), North Dakota (2001), Oklahoma (2003), Rhode Island (2001), and Wisconsin (2010).

Some states in our control group have attempted to tighten their existing payday lending regulation in the recent past. A few examples include the states of Ohio and Washington. Ohio enacted the short-term lender law in 2008, which restricted lenders from making loans using the electronic media among other requirements. Nevertheless, the state's payday lenders were able to bypass the legislation by registering as credit service providers—entities that are eligible to operate payday lending under the state's Mortgage Lending Act and the Small Loan Act. Washington passed a bill in 2009 (to be effective in 2010) that limited the maximum loanable amount and imposed a cap on the number of loans an individual can borrow in a year, along with some other provisions (see Cuffe and Gibbs (2017) for details on Washington).[13] We broadly classify the control states as the 'permissible' group, that is, those states that have not completely restricted payday lending during the period of our interest.

With respect to the treated states, there are four: Arizona, Arkansas, Montana, and New Hampshire. Arizona imposed a ban on payday lending in 2010 by allowing a previously enacted law (effective in 2000) to expire. The law exempted payday lenders from the existing 36% APR cap on small loans. Arkansas restricted payday lending in 2011 by repealing its Check Cashers Act, which allowed payday lenders to operate within the state. Montana prohibited payday lending through a ballot initiative, which allowed voters to put a 36% APR cap on all loans. In January 2009, New Hampshire imposed an annual interest rate cap of 36% on all small loans, which includes payday loans.

We exclude 17 states from our analysis. We drop 14 states due to restrictions on payday lending activities that were imposed either prior to or within our study period. We drop the District of Columbia, Alaska, and Virginia for alternative reasons. Although the District of Columbia could have been a potentially treated state in our analysis, we could not include it due to missing data issues, especially with respect to the outcomes of interest such as bankruptcy filings. In Alaska and Virginia, payday lending was illegal until 2005 and 2002, respectively. We did not find any evidence on the presence of payday lending business in these two states prior to the respective authorization years. Nevertheless, to be sure, our results still hold if we include Virginia by restricting our study period to 2002–2016. However, we prefer using 2001–2016 as our study period to maximize the pre-treatment information.

[12] For a number of states, we treat the year that a state first enacted legislation to regulate the payday lending business as the authorization year. This is because regulating payday lending business appears to imply legalizing payday activities in addition to providing state-specific guidelines for payday services such as licensing requirements for lenders, maximum loanable amount, and other borrowing requirements, restriction on lenders' right to pursue criminal charges against defaulting borrowers. See Appendix Table A.6 for further details.

[13] Despite Washington's restrictions on loan rollovers, we keep it in the control sample for our analsyes because the loan-rollover legislation did not completely shut out the industry (nor was it intended to). Washington's payday lending industry, while weakened from the legislation, still operates a total of 79 storefront locations in the state as of 2018 (see: www.dfi.wa.gov). Bhutta (2014), Bhutta et al. (2015), and Dobridge (2018) exploit state-level legislation, all of which classify Washington in the control group, similar to us. Nonetheless, to be sure, we run our main analyses both with- and without Washington (and Ohio). These results are largely unchanged and are available upon request.

4. Empirical approach

As mentioned above, we pursue a sequence of research questions. First, we test whether payday lending decreased in the recently-banning states. Then, we test whether there was a change in bankruptcy rates, formal or informal. To implement this sequence of testing, we employ a twofold empirical strategy. First, we run difference-in-differences (DD) regressions. Second, we use the synthetic control method (SCM) for the continuous series, e.g., number of payday lending establishments, bankruptcies, and delinquency rates. In this section, we describe our variables and data, as well as a detailed explanation of the DD and SCM methodologies as applied to our research questions. Data summary statistics and sources are in Appendix Tables A.1 and A.2, respectively.

4.1. Variables and data sources

To examine whether interest rate caps are an effective payday ban, we analyze data from three sources. First, we use survey evidence from the FDIC's National Survey of Unbanked and Underbanked Households, which asks about payday loan usage and bankruptcy. This is a biennial (odd-numbered years, 2009 - 2017), nationally-representative survey that is administered as a CPS supplement. We do not use the 2017 survey since it is beyond the study period. Second, we use survey evidence from FINRA's National Financial Capability Study (NFCS), which is detailed below since it asks about usage, bankruptcy, and credit more generally. Finally, we look at the number of payday lenders as identified by the US Census Bureau's County Business Pattern data using the North American Industry Classification System (NAICS) codes. Payday lending is classified under NAICS code 522390, along with other non-bank credit intermediation services.

For formal bankruptcy, we collect quarterly non-business bankruptcy filings by state-year and state from the US Courts Caseload Statistics tables from 2001 to 2016. These are expressed as rates, per 100,000 people. We separately look at two different categories of personal (non-business) bankruptcy filings: Chapter 7 and Chapter 13; we also calculate the sum of the two ("overall" henceforth). Of the personal bankruptcy categories, Chapter 7 is the most common, through which an agent can liquidate a filer's assets to partially or fully repay the existing debts. In the event that the liquidation of assets fails to cover all debts, the remaining debt is discharged. Unlike Chapter 7, Chapter 13 filers (also known as the 'wage earner's plan') are not required to liquidate their assets. Instead, individuals need to have stable earnings in order to develop a proposal for a future repayment plan that ranges from 3 to 5 years. See Hynes (2012) for more details.

For informal bankruptcy, we use state-year percentages of delinquencies on student loans, auto, mortgage, and credit card debt. These estimates are obtained from the Center of Microeconomic Data (Federal Reserve Bank of New York), which provides annual state-level household debt statistics from the years 2003 until 2017.

We analyze FINRA's National Financial Capability Survey (NFCS), which is a nationally-representative, triennial survey that is run by FINRA in consultation with the US Department of the Treasury. The survey was administered three times: 2009, 2012, and 2015. The survey instruments that we are interested in are the following: bankruptcy, late fees on credit card bills, general difficulty paying bills, and overall financial condition. Across all three waves of the survey, there are a total of over 54,000 respondents. We use the survey in two ways. First, we use it, in part, to confirm that interest rate caps are an effective payday lending ban. Second, we use it to corroborate our bankruptcy findings from the state-level synthetic control evidence.

In many of our analyses, we consider a wide range of state-level controls to ensure the robustness of our estimates. With respect to demographics, we include state-year proportion of the population who are female (sex), White (race), Hispanic (ethnicity) and adult (18 years and above). The social and political characteristics of the state may affect consumers' financial condition. Accordingly, we control for the following: high-school graduation and college attendance; and a binary indicator for a Democrat governor.

To account for state-level economic conditions, we control for the following variables: seasonally-adjusted unemployment rate, real GDP per capita, poverty rate, Supplemental Nutrition Assistance Program (SNAP) take-up rate, and Medicaid recipience rate. We also include financial institution-based indicators, including the following: number of commercial banks (CB) and savings institutions (SI) per 100,000 population; total individual as well as credit card loans per capita (from CB and SI); total liabilities-assets ratio (in CB and SI); number of credit intermediation institutions; number of employed individuals in those establishments per 100,000 population. In Table A.1 of the appendix, we present pre-payday lending restriction sample mean (or proportion) of all variables used in our analysis for the control states and for each treated state: New Hampshire, Arizona, Arkansas, and Montana.

When analyzing survey data, we are able to include individual-level covariates such as race, gender, education, and age. When possible, we control for employment situation, e.g., employed, unemployed, and household size.

4.2. Difference-In-Differences estimation

For both of our research questions we employ difference-in-differences (DD) regressions. The control states in the DD regressions include all the states classified as control in Table 2. We also include all of the aforementioned state-level covariates when analyzing state-level bankruptcy and delinquency data. We include individual-level covariates when analyzing the survey data. More formally, we estimate the following:

$$Y_{ist} = \rho_0 + \rho_1 \text{BAN}_{st} + \rho_2 X_{ist} + \rho_3 Z_{st} + \gamma_s + \lambda_t + \epsilon_{ist} \tag{4.1}$$

where Y_{ist} represents the aforesaid binary outcomes (or bankruptcy or delinquency rates), and BAN_{st} is a binary indicator for having a payday lending restriction in state s at time t. X_{ist} and Z_{st} represent vectors of individual and state-level characteristics, respectively. State fixed effects are denoted by γ_s. Time fixed effects are denoted as λ_t. The parameter ρ_1 is the estimated measure of the relationship between payday restrictions and the outcomes of interest.[14] For binary outcomes, e.g., the FDIC and FINRA-NFCS survey instruments, the parameter ρ_1 has the interpretation of a marginal effect. For the sake of robustness, all binary outcomes are estimated by a LSDV-LPM model as well as a probit specification. We cluster all standard errors at the state-level.

4.3. Synthetic control method estimation

To assess the causal impact of payday lending restrictions on the incidence of personal bankruptcy filings and delinquencies, we employ the synthetic control method (SCM) formulated by Abadie et al. (2010); see also Abadie (Forthcoming) for a recent overview.[15] In the context of our study, the SCM implements a data-driven process to illustrate a counterfactual post-payday lending restriction path for the treated state(s), e.g., New Hampshire. More specifically, this counterfactual is represented by a 'synthetic' weighted average of control states, which is optimally generated from a pool of pre-specified 'donor pool' of control states, i.e., the "control states" in Table 2 above.

To illustrate the SCM strategy, let there be a sample of $I+1$ units (represented by the treated and control states) indexed by i, where unit $i = 0$ is the treated state and all other potential comparison states are denoted by $i = (1, \ldots, I)$. We define $Y0$ as a $(k \times 1)$ vector that incorporates pre-intervention values of the dependent variable and $(k-1)$ relevant covariates that are predictive of the outcome of interest. Let $Y1$ be a $(k \times I)$ matrix that includes the values of the same pre-intervention variables for the each ith unit in the donor pool. The SCM assigns non-negative weights to identify a convex combination of the I control states, which is expected to mimic the treated unit during the pre-intervention period. This is achieved by minimizing the difference between pre-treatment characteristics of the treated unit, e.g., New Hampshire, and the synthetic control unit. The weights w_i are selected such that $(\sum_{i=1}^{I} w_i = 1)$. Let μ be some non-negative scalar. Formally, then, the SCM selects the optimal of combination of w_i, represented by vector W^*, which solves the following constrained minimization problem:

$$W^* = \arg\min_{W} \ (\mathbf{Y_0} - \mu - \mathbf{Y_1 W})' \mathbf{V} (\mathbf{Y_0} - \mu - \mathbf{Y_1 W})$$

$$\text{s.t. } \mu = 0; \sum_{i=1}^{I} w_i = 1; w_i \ \geq 0 \ \forall \ i = (1, \ldots, I) \tag{4.2}$$

where $\mathbf{V}$ is a positive definite matrix.

The SCM allows us to compute a difference-in-differences (DD) estimate of the treatment effect between New Hampshire (NH) and synthetic New Hampshire (SNH). That is, we define $Depvar_{pre}^{NH}$ and $Depvar_{pre}^{SNH}$ to be the average pre-intervention values of the outcome variable for the treated state and its synthetic control, respectively. Further, we define $Depvar_{post}^{NH}$ and $Depvar_{post}^{SNH}$ as the post-payday lending ban average of the outcome variable for New Hampshire and its synthetic control, respectively. Using the defined values, the DD estimate for New Hampshire is:

$$DD_{NH} = (Depvar_{post}^{NH} - Depvar_{post}^{SNH}) - (Depvar_{pre}^{NH} - Depvar_{pre}^{SNH}) \tag{4.3}$$

which we iteratively calculate for each post-treatment period.

We formally test the statistical significance of the DD estimate by performing the permutation test recommended by Abadie et al. (2010). This is accomplished using a placebo analysis. The intervention is the period of the payday lending ban, e.g., 2009q1. We then identify synthetic comparisons for each state included in the donor pool to calculate placebo DD estimates of the untreated states. In other words, the program iteratively runs the synthetic control method on each of the control states in the donor pool, each time excluding New Hampshire. This will generate a sampling distribution for DD_{NH}, from which we are able to calculate a p-value for each post-treatment period (and jointly). See Figs. 6 and 8 in the next section.

Typically the SCM is run on one treated unit, e.g., New Hampshire, but it is possible to "pool" with several treated units, e.g., all four payday-banning states. Building on the traditional SCM, Galiani and Quistorff (2017) developed a "pooled" synthetic control method, which allows for an evaluation of the treatment effect

[14] We include a wide-range of relevant state-level demographic, social (crime, political, substance use), economic, and banking sector related covariates included in our regression to minimize omitted variable biases arising from unobserved heterogeneities.

[15] We use Stata version of the 'Synth' command developed by Abadie et al. (2010) to estimate SCM (information accessed from http://fmwww.bc.edu/RePEc/bocode/s/synth.html; retrieved on March 20, 2018). For robustness, we also estimate fully nested optimization models (using the nested and allopt option), which computes the optimal convex combination of the control units from all possible values of V and W. The results are markedly similar to the default estimates reported in our main analysis.

where the treatment is assigned to multiple units across different points in time. More specifically, for each treated unit denoted as $g \in \{1, \ldots, G\}$, Galiani and Quistorff (2017) show that it is possible to estimate the post-treatment effect, $\hat{\alpha}_g$, using the standard SCM approach such that the average treatment effect can be computed as

$$\bar{\alpha} = G^{-1} \sum_{g=1}^{G} \hat{\alpha}_g \tag{4.4}$$

Furthermore, based on the concepts developed in the standard SCM of Abadie et al. (2010), a corresponding set of control states, denoted by $\hat{\alpha}_{PL}$ is estimated for each treated unit, g. Similar to the estimation of $\bar{\alpha}$, the "pooled synth" algorithm generates an average placebo effect, $\bar{\alpha}_{PL}$ by averaging over the placebo effects, $\hat{\alpha}_{PL}$. Statistical inference comes from a comparison of the average of treatment- and placebo effects.[16]

4.4. Comparing difference-in-Differences to the synthetic control method

Doudchenko and Imbens (2016) show that the difference-in-differences (DD) method of identifying the counterfactual has a similar structure to the SCM identification strategy. Formally, within the framework of Eq. (4.2) above, the DD would be as follows:

$$W^* = \underset{W,\ \mu}{\arg\min}\ (\mathbf{Y_0} - \mu - \mathbf{Y_1 W})'\mathbf{V}(\mathbf{Y_0} - \mu - \mathbf{Y_1 W})$$

$$\text{s.t. } \mu > 0;\ \sum_{i=1}^{I} w_i = 1; w_i\ \geq 0\ ; w_i = \bar{w},\ \forall\ i = (1, \ldots, I) \tag{4.5}$$

where $\mathbf{V}$ is a positive definite matrix.[17] The μ term is constant and strictly positive in a DD identification strategy—the familiar parallel trends assumption. If it's true that $\mu > 0$, then it follows that $w_i = \bar{w}$ and hence it's possible to estimate a DD model with OLS. However, if μ is zero or not constant, then (pre-treatment) trends are not parallel and the results will be biased.

Empirically, it is often possible to test if the trends are parallel, e.g., in an OLS-regression-adjusted event study (Autor, 2003). If the test fails, i.e., μ is not constant, then the SCM presents a potential alternative if the assumptions are met, namely the constraints in Eq. (4.2) above. The $\mu = 0$ assumption rules out the possibility that there is a systematically-large difference between treatment and control in the pre-treatment time period. The assumption that the weights sum to one means that the strategy will not properly identify the average treatment effect if the treated unit is an outlier. For example, if, say, New Hampshire was rife with bankruptcies, then no weighting of the other non-banning states (the donor pool) could equal New Hampshire's bankruptcy values unless the assumption were relaxed and weights could sum to a value greater than one. Finally, the assumption of non-negative weights ensures a unique weighting solution, which makes the ultimate weighting scheme interpretable.[18]

The DD method is intuitive and simple to estimate. Moreover, it can yield results with as few as two time periods. Because of its congruity with OLS, inference is well established. On the other hand, while DD can accomodate unobservable heterogeneity that is constant (through state-specific linear time trends), it fundamentally cannot account for unobservable non-liner heterogeneity; if $\mu_t \neq \mu$, then there is not much a researcher can do. In Section 5.2 below, we test whether $\mu_t \neq \mu$ using the method of Autor (2003). An additional assumption of DD is the imposition of equal weights, $w_i = \bar{w}$. A researcher can manually change this through some alternative weighting scheme, e.g., weighted least squares (WLS), propensity scores (PSM), etc. But such weighting schemes are either arbitrary (WLS) or could inhibit a causal interpretation (PSM) of the results (Wooldridge, 2005).

The synthetic control method can accommodate the lack of parallel trends; indeed it fundamentally imposes it, i.e., it sets $\mu = 0$. The manifestation of this assumption can be seen in our SCM results; see Figs. 3 and 5 below and note the close overlap of both series throughout the pre-treatment periods. One way to improve the fit even more is to relax the weight constraints in Eq. (4.2), i.e., that they be positive and sum to one. Carvalho et al. (2018) develop an estimation procedure—artificial counterfactual (ArCo)—to do just that. We estimate the ArCo and our results are unchanged; see the appendix for details. Unlike DD, the SCM does not impose an equality of weights across treated units, a fact evident in Table 6 below.

That said, one criticism with the SCM weights is that they are somewhat of a blackbox. They can, at times, be non-intuitive, i.e., the weighting algorithm assigns a high weight to a state that is different in geography, culture, etc., from the treated state. For instance, in our New Hampshire case study, Hawaii receives positive weight (Table 6). To some extent, this is the "cost" of the SCM as an identification strategy. Nonetheless, we address this critique by manually assigning weights of 100% on the nearby state of Rhode Island, and then again with 100% on Delaware. Our overall results are robust to these estimations.

Another potential issue with SCM is the potential for overfitting. It's possible to get a near-perfect fit in the pre-treatment period, but some of that may be noise. We don't view this as a problem since our data are relatively low frequency and highly aggregated, which should smooth out any noise. In any case, the ArCo estimator is robust to overfitting.

Finally, as mentioned above, SCM cannot fundamentally handle outliers—particularly extreme ones (but cf. Powell (2018)). None of our treated states are outliers in any of the variables that we consider. If any of our states were outliers along any dimension, it would be evident in the SCM visual plots (Figs. 3 and 4) as well as a mismatch in the treated mean and synthetic control means (Table 6).

5. Research questions and results

In this section we present the results of our analyses. First, we test whether interest rate caps limit payday lending. This is important because there have been instances when states were able to circumvent the legislation, e.g., Ohio, North Carolina (see Bhutta (2014)). Then, we test whether the bans affect bankruptcy or delinquency.

5.1. Are interest rate caps an effective payday lending ban?

Rather than an outright ban on payday loans, many states have opted to impose an interest rate ceiling, frequently set at 36% APR. The rationale is that if payday lenders can't afford to make loans at that price, then perhaps they shouldn't be making loans at all. To give a rough idea, payday loans are typically two weeks in duration, and priced at $15 for every $100 borrowed, which translates to an APR of 390%. At 36% APR, the same payday loan would need to be priced at $1.39 for every $100 borrowed.

[16] For a recent application using state-level data see Chu and Townsend (2019).

[17] The matrix $\mathbf{V}$ is equal to the identity matrix if the only variables are lagged dependent variables. In the context of the SCM, this would entail a non-nested minimization problem to solve for the vector W^*. If the pre-treatment fit between treatment and control is minimized according to covariates, then the optimization problem is nested. Our results below are robust to both optimization procedures.

[18] Carvalho et al. (2018) point out that the assumption of non-negative weights is too restrictive in terms of fit. They recommend relaxing this assumption if certain conditions are met. See our discussion, tests, and visual plots using their ArCo method in the appendix.

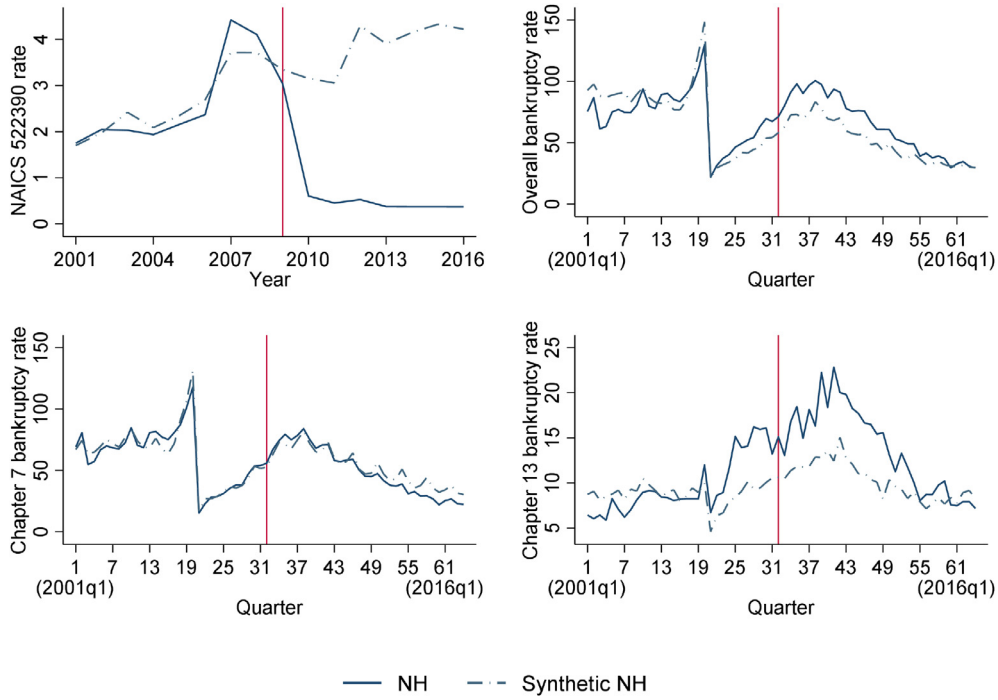

Fig. 3. Visual synthetic control results: New Hampshire formal bankruptcy.

Table 3
OLS DID analysis of payday loan establishments and usage.

	NAICS 522390 Establishments	FDIC Payday Loan Use	FINRA-NFCS Payday Loan Use
Study period	2001 - 2016	2009 - 2015	2009 - 2015
Sample mean	6.075	0.015	0.125
Payday loan restriction	−2.779***	−0.010*	−0.043**
	(0.781)	(0.006)	(0.009)
State fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
State characteristics	✓	✓	
Individual characteristics		✓	✓
Sample size	529	137,593	54,571

Notes: * is 10%; ** is 5%; *** is 1% significance. The NAICS unit is the number of establishments per 100,000 residents. The FDIC units is the percentage of people who responded "yes" to the question 'in the past 12 months, did you or anyone in your household have a payday loan or payday advance at a place other than a bank?' The FINRA units is the percentage of people who responded "yes" to the question 'in the past five years, how many times have to taken out a short term "payday loan?" We collapse the variable to a binary measure. The FDIC survey is biennial, while FINRA is triennial. Estimates in the table are interpreted as marginal effects. All standard errors are cluster-robust, being clustered at the state level.

A report by the FDIC (Flannery and Samolyk, 2005) shows that such high APRs, e.g., 390%, are indeed justified by the costs of the industry (but cf. a report from the Federal Reserve Bank of Kansas City by DeYoung and Phillips (2009)). Part of the reason for the high APR is the necessary cross-subsidization in a market plagued by asymmetric information. Dobbie and Skiba (2013) find that 19% of initial payday loans go into default. The adverse selection that Dobbie and Skiba find in their data is even worse in the online payday lending market: Li et al. (2012) find that the default rate from an online lender was 28%; Rigbi (2013) finds a default rate of 19.4%.

Given such high rates of default, payday lenders cannot profitably stay in business. From the supply side, we test whether the number of payday lenders decreases as a result of the payday loan restrictions. Our measure of the number of payday lending storefronts is the number of establishments of NAICS 522390, 'other activities related to credit intermediation'. This isn't a perfect measure since it also includes other non-bank credit intermediation services. Table 3 shows that establishments significantly decrease as a result of the interest rate cap. This finding corroborates Bhutta (2014), who finds that the concentration of payday lenders is higher in states that have less stringent payday lending restrictions.

To further solidify these supply-side results, we retrieved (from EDGAR) the 10-K reports from several payday lenders. In the 2011 10-K from Advance America, Cash Advance Centers, Inc., the nationwide market leader in the payday lending industry, it states (pg. 18) "[l]egislation was adopted in New Hampshire in 2008 that effectively prohibits us from offering cash advances to consumers in that state." It goes on to say that all operations were shut down in New Hampshire in February of 2009. Furthermore, it mentions that they ceased operations in Arkansas, Arizona, and Montana as a result of the passage of these states' respective regulations.

From the demand side, we test whether payday loan usage dropped among consumers in the states that restricted payday lending relative to the states that did not. Our measure of payday

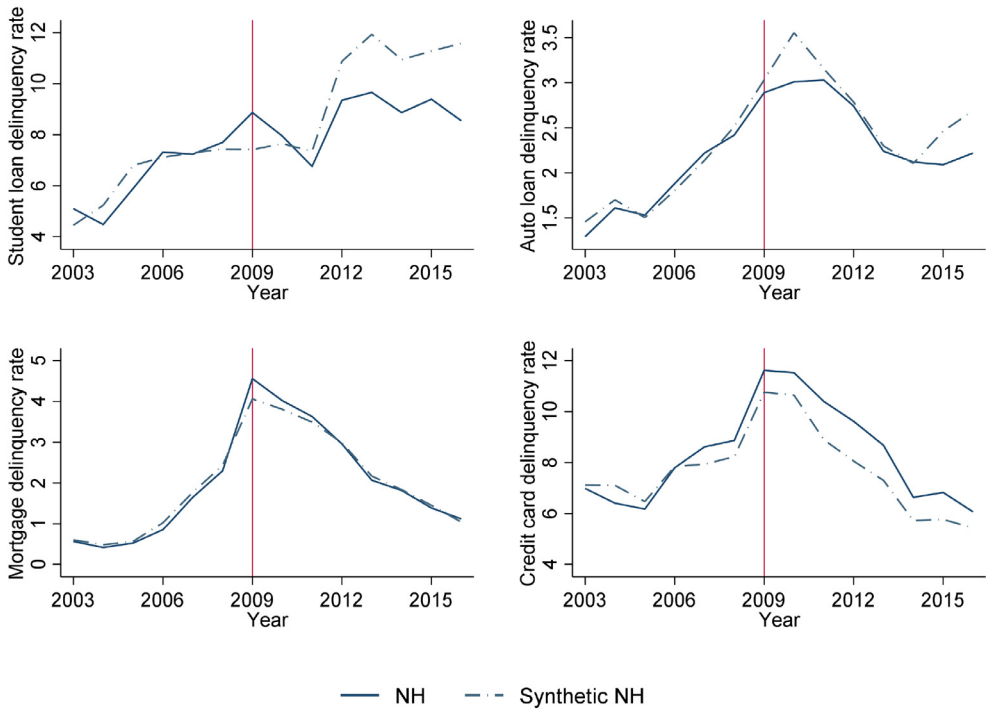

Fig. 4. Visual synthetic control results: New Hampshire informal bankruptcy.
Notes: In both figures above, the solid line represents the actual path of the measure throughout the study period. The dashed line represents the path of the synthetic control unit, which is calculated as the optimal weighting scheme that minimizes the distance between the two series (weights in Table 6). The vertical line is the legislative intervention period, which is 2009 for New Hampshire. Formal bankruptcies are quarterly rates per 100,000 persons; informal bankruptcies (delinquencies) are annual percentage points. The formal bankruptcy results could be confounded by the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCA), which was signed into law on 4/20/2005, and went into effect on 10/17/2005. We consider the effects of BAPCA in the pooled SCM results, Section 5.5. The Great Recession could also confound our results. We run the analysis on South Dakota as a robustness check against this possibility; see appendix Figures A.1 and A.2..

loan usage comes from two sources: the FDIC's National Survey of Unbanked and Underbanked Households, a biennial survey that is administered as a supplement to the Current Population Survey; and FINRA's National Financial Capability Survey, which is administered on a triennial basis.

Table 3 demonstrates that usage declined among the participants in both surveys, findings that corroborate Zinman (2010), who analyzes local survey data to show that payday loan usage fell in Oregon after it implemented its 36% cap. Of course, with survey data, there are selection concerns and measurement issues, so we note that our statistical findings are in line with anecdotal focus group evidence from a Pew Research report, Pew (2012), which shows that usage drops in restrictive states.

This demand side evidence is important because it shows that enacting an effective payday lending ban does indeed cut down on payday loan usage. Both the FDIC- and FINRA-NFCS surveys ask about general payday loan usage; neither makes a distinction between storefront and online loans. Therefore, since both surveys show a statistically significant decrease in payday lending, this is evidence that consumers did not, en masse, migrate to the online payday lending market. The abovementioned Pew report highlights that many would-be borrowers are apprehensive about giving their financial information online. That said, we can be reasonably confident that our bankruptcy findings below are reliable insofar as payday lenders aren't circumventing the cap and would-be borrowers are not getting their payday loans from elsewhere.

5.2. Do payday lending bans mitigate bankruptcy?

Strictly speaking, capping interest rates is not intended to eliminate bankruptcy filings *per se*. Rather, the goal of the cap is to eliminate the high APR of payday loans, which, in turn, is part of a broader desire to protect consumers from a so-called 'debt trap'. Consumers take out a high-APR loan, cannot repay it, roll it over, and eventually go delinquent on other debts. The cycle ultimately stops at dealing with bill collectors or filing for formal bankruptcy protection.

To be sure, there is overlap in the population of payday borrowers and the population of bankruptcy filers, homeowners, credit card holders, etc. Elliehausen and Lawrence (2008) find that 15% of payday borrowers have declared bankruptcy in the previous five years. Our analysis of the NFCS shows that 8.5% of payday borrowers have filed for bankruptcy in the past two years (see Table 10 below).

Regarding delinquency, the Bhutta et al. (2015) dataset shows that 59% of payday loan applicants have a general purpose credit card, with approximately 80% of those having little credit available on their credit cards or they are maxed out completely. Approximately 42% of borrowers who use Advance America, the market leader mentioned above, own their home; 32% have a mortgage; 53% have an auto loan; and 36% have at least some college.

In our first bankruptcy analysis, we use the states of Arizona, Arkansas, Montana, and New Hampshire, as quasi-experimental labs to test whether the bans affect bankruptcy filing rates and delinquencies. We run difference-in-differences (DD) regressions, the linchpin of which is the assumption of parallel trends. In order to estimate the true causal effect of payday loan bans on bankruptcy, the treatment and control states would need to differ pre-treatment by some constant amount. One potential reason for the failure of the parallel trends assumption is that the banning states experience significant variation in the outcomes of interest in anticipation of the interest rate cap.

We examine this possibility using the event study method, as done by Autor (2003) and others. Our event analysis incorporates

Table 4
Event analysis of formal and informal bankruptcy - dynamic impacts of payday bans.

	Formal Bankruptcy (Rates, per 100,000)				Informal Bankruptcy (Delinquencies, %)			
	NAICS 522390	Overall filings	Chapter 7 filings	Chapter 13 filings	Student loan	Auto loan	Mortgage loan	Credit card
Study period	2001 - 2016	2001 - 2016			2003 - 2016			
Sample mean	6.076	431.300	309.578	121.401	9.185	3.283	2.931	9.051
4 years prior	−1.455	−103.104**	−68.527**	−34.573	−0.157	0.064	−0.387	−0.313
	(1.203)	(41.975)	(31.089)	(27.205)	(0.860)	(0.272)	(0.339)	(0.208)
3 years prior	−2.033	8.240	10.936	−2.834	−0.088	−0.037	−0.191	−0.178
	(1.416)	(61.041)	(37.302)	(26.372)	(1.432)	(0.298)	(0.317)	(0.301)
2 years prior	−2.893	−38.657	−25.808	−12.896	0.137	0.137	0.111	0.187
	(1.799)	(52.636)	(35.090)	(22.755)	(1.695)	(0.184)	(0.579)	(0.470)
1 year prior	−3.161*	−44.533	−26.967	−17.690	−0.079	0.136	0.552	0.669
	(1.636)	(50.191)	(34.186)	(20.662)	(1.451)	(0.162)	(0.818)	(0.870)
Year of passage	−3.084*	−29.106	−2.264	−27.000	0.047	0.234	1.359	0.913
	(1.694)	(68.271)	(48.816)	(25.847)	(1.849)	(0.423)	(1.515)	(1.213)
1 year after	−3.508**	-0.579	7.681	−8.512	0.321	0.199	1.213	0.698
	(1.663)	(75.266)	(52.994)	(29.131)	(1.879)	(0.564)	(1.138)	(1.004)
2 years after	−4.525***	−13.226	−8.993	−4.470	0.021	0.456	0.583	0.685
	(1.280)	(80.270)	(58.920)	(27.416)	(1.635)	(0.570)	(0.909)	(0.895)
3 years after	−4.841***	−64.955	−35.072	−29.842	−0.015	0.578*	−0.193	0.164
	(1.244)	(57.670)	(39.818)	(28.762)	(1.048)	(0.329)	(0.663)	(0.715)
4+ years after	−4.675***	-92.484**	−52.263	−39.152	0.036	−0.014	−0.494	−0.295
	(1.316)	(41.991)	(32.227)	(26.704)	(1.189)	(0.387)	(0.854)	(0.589)
State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
State characteristics	✓	✓	✓	✓	✓	✓	✓	✓
State Linear Trends	✓	✓	✓	✓	✓	✓	✓	✓
Observations	529	529	529	529	462	462	462	462
R-squared	0.910	0.925	0.913	0.951	0.902	0.909	0.822	0.919
F-stat: sum of 4 leads = 0	2.54	0.92	1.02	0.51	0.00	0.16	0.00	0.05
P-value	0.12	0.35	0.32	0.48	0.97	0.69	0.96	0.82

Notes: * is 10%; ** is 5%; *** is 1% significance. The omitted, reference category is five years or more prior to passage of the interest rate cap. The above regressions are performed using a similar specification as Table 5 below. We report the anticipatory and post-treatment effects of imposition of a 36% interest cap on payday loans by creating binary indicators of leads (each for 1, 2, 3, and 4 years prior), passage year, and lags (each for 1, 2, 3, and 4+ years post) of the passage of state legislation. All standard errors are estimated by clustering at the state-level.

binary indicators for the following: each of the four years prior to the passage year of the interest rate cap, the passage year, and each of the four (or more) post-implementation years. The period representing five or more years prior to implementation is the omitted, reference category. Our results are in Table 4 below.

With respect to payday lending establishments, we find that relative to our omitted period all coefficients are negative and generally increasing in absolute value, indicating a progressive decline in payday firms over time. All of the post-implementation coefficients are statistically negative (relative to the omitted category). The magnitudes show that the ban was effective at reducing storefronts, even four years later. The year prior to passage is significant, which shows that there is at least some anticipation of the legislation. Such anticipation is to be expected from payday lenders since they monitor legislation closely through their trade associations. Of course, the reaction from the supply side of the market could alter the reaction from the demand side. For example, if lenders consolidate locations in anticipation of the upcoming ban, this could affect the price and quantity of payday loans, which would affect the demand side of the market.

Turning to bankruptcy filings, there is little evidence of any anticipatory effects. The sole exception is for overall- and Chapter 7 bankruptcies four years prior to the passage, both of which show a large, negative effect. However, these coefficients are relative to the omitted category of 5+ years prior—a time period that, for some treated states, overlaps with the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCA). BAPCA was, in part, a way to incentivize consumers to file for Chapter 13 rather than Chapter 7. This may explain why Chapter 7 filings are twice the magnitude of Chapter 13. We address the potential confounding nature of BAPCA in our synthetic control analysis below.

To summarize, for payday lending establishments, the presence of the anticipation effect casts some doubt on the parallel trends assumption. That is, the value of μ in Eq. (4.5) may not be constant. For bankruptcies and delinquencies, since there is virtually no evidence of anticipatory effects, we can trust the parallel trends assumption for these measures and hence proceed with a valid DD identification strategy.

We run a set of OLS difference-in-differences regressions on annual state-level bankruptcy and delinquency data (per 100,000 residents) for 34 states between 2001 - 2016. These data are annual so as to match the lowest frequency of our control variables.[19] There are four treated states: Arizona, Arkansas, Montana, and New Hampshire. We eliminate the twelve states that banned payday lending throughout the sample period, e.g., New York, because they do not add any variation. We further eliminated other states that banned in the sample period, but are not considered part of the analysis, e.g., Georgia. However, we also ran the DD analyses with the other states that banned during our sample period, i.e., adding Georgia to the treated group. We also estimated the DD models using the 'always-banning' states, e.g., New York, as an alternative control group. The results are robust to these alterations and are available upon request. All regressions were run with the inclusion of state-specific linear trends.

Table 5 presents the results of our DD analysis. The NAICS establishments estimate is essentially the same as the estimate from Table 3; standard errors are lower here as a result of the presence of control variables.

[19] In our SCM analysis below, we are able to use quarterly bankruptcy data when we run the non-nested version, i.e., when the minimization problem is with respect to lagged outcomes only.

Table 5
OLS DD analysis of formal- and informal bankruptcy.

	Formal Bankruptcy (Rates, per 100,000)				Informal Bankruptcy (Delinquencies, %)			
	NAICS 522390	Overall filings	Chapter 7 filings	Chapter 13 filings	Student loan	Auto loan	Mortgage loan	Credit card
Study period	2001 - 2016	2001 - 2016			2003 - 2016			
Sample mean	6.075	431.300	309.578	121.401	9.185	3.283	2.931	9.051
Control mean	6.388	437.472	311.937	125.228	9.113	3.310	2.974	8.997
Payday loan restriction	−2.811***	−33.529	−19.309	−14.209	0.096	0.121	−0.221	−0.115
	(0.489)	(29.071)	(24.414)	(13.798)	(0.612)	(0.191)	(0.588)	(0.306)
	[-3.80, -1.82]	[-92.67, 25.62]	[-68.98, 30.36]	[-42.28, 13.86]	[-1.15, 1.34]	[-0.27, 0.51]	[-1.42, 0.98]	[-0.80, 0.51]
State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
State characteristics	✓	✓	✓	✓	✓	✓	✓	✓
State Linear Trends	✓	✓	✓	✓	✓	✓	✓	✓
Observations	529	529	529	529	462	462	462	462
R-squared	0.904	0.924	0.911	0.950	0.902	0.908	0.818	0.916

Notes: * is 10%; ** is 5%; *** is 1% significance. NAICS 522,390 is the rate per 100,000. These DD regressions were run using annual data for each series. We ran the regressions including all states that banned during the noted time periods, e.g., Georgia in 2004; and we estimated the effect using only those states under main consideration, e.g., AR, AZ, NH, and MT. The results are robust to the inclusion of all banning states. States that banned throughout the entirety of the sample, e.g., NY, were thrown out because they do not add any variation. All variables are expressed as per 100,000 residents. Delinquency units are percentages. Standard errors in parentheses. All standard errors are cluster-robust, being clustered at the state level. 95% cofidence interval in brackets.

Despite the overall bankruptcy result being statistically null, we can nevertheless make some comparisons. For example, if the most extreme version of the debt trap hypothesis is true, then payday lending bans should reduce bankruptcies. But by how much? Our 95% confidence interval for overall bankruptcies shows that we can rule out a decline of 92 annual bankruptcy filings per 100,000. Putting this magnitude in context, Skiba and Tobacman (2019) find that payday lending access—measured as loan application—causes a statistically-significant increase of 2.35 bankruptcy filings (per 100 applicants) one year later. These authors use payday loan data from one lender in Texas from 2000 to 2004. Since these authors use bankruptcies per 100 applicants, we need to scale their results in order to make them comparable to ours, which is bankruptcies per 100,000 persons. To do that, we need an "application-to-household" conversion factor. We use Texas's application rate (11.37 applications per 100 persons) from the 2009 NFCS survey, which is the earliest year available. After scaling up again by 10 (to get per 100,000), we calculate their treatment effect to be approximately 280 bankruptcies per 100,000 persons. Appealing again to the 95% confidence interval, we can rule out a magnitude of this size.[20] Perhaps the average treatment effect is different at the threshold than it is in aggregate; or we haven't considered enough time periods for the effect to take hold, a possibility that we discuss in our synthetic control results below.

Regarding our Chapter 7 result, the effect is statistically zero, which is somewhat unsurprising since Chapter 7 represents a type of finality of debt relief: it offers full discharge, but it stays on the record longer, and typically requires a dramatic liquidation of assets. Our finding of null results corroborate Morgan et al. (2012). The 95% confidence interval of the result shows that we can rule out an effect size of fewer than 69 filings per 100,000 households. To give some context, Lefgren and McIntyre (2009) estimate that strictness of state wage garnishment restrictions is associated with a drop of 75 filings per 100,000 for mild restrictions and a drop of 192 filings per 100,000 for severe restrictions. With 95% confidence, we can rule out such effect sizes. Hence, payday loan bans don't reduce Chapter 7 bankruptcy filing rates nearly as much as wage garnishment restrictions do—a result that confirms Lefgren and McIntyre (2009), albeit from a different estimation method, updated time period, and alternative frequency of data.

Another notable finding in our DD analysis is that the sign and magnitude of our Chapter 13 estimate is in-line with those of Morgan et al. (2012), who do a DD analysis similar to ours. These authors use quarterly Chapter 13 filings per 10,000 persons. Scaling up, they estimate the treatment effect of bans to be -37.2 per 100,000 without state time trends, and -19.2 with state trends. These results are within our 95% confidence interval. Morevover, like us, the authors find a null effect on Chapter 7 filings (but don't report magnitudes). The upshot is that the null effects of payday lending bans on bankruptcy are constant over time, even in the face of significant alterations to bankruptcy law (BAPCA) as well as a deep recession (Great Recession), and even across a completely different set of treated states.

Our results for delinquencies have mixed signs, but they are all statistically insignificant, a finding that corroborates Bhutta (2014) and Carter and Skimmyhorn (2017), both of whom find no effect on delinquency (measured differently than ours). We also confirm the results of Desai and Elliehausen (2017) for North Carolina and Oregon but not for Georgia. Our results also seem to conflict with the findings of Gathergood et al. (2019), who find that payday lending access in the UK, measured as loan approval, increases delinquencies in the long-run. Our SCM results below allow for a fuller exposition of the long-run effect of payday loan access on delinquencies.

5.3. Synthetic control analysis of New Hampshire

Difference-in-differences imposes equal weights across the control units, as can be seen in the third constraint in the optimization problem of Eq. (4.5). This can be overcome with a manual weighting scheme, e.g., weighted least squares, but these weights would be arbitrary. In contrast, the synthetic control method lets the data choose the optimal weights. Moreover, the (non-nested) SCM also allows us to use quarterly data. The SCM further permits us to examine the treatment effect for each post-treatment period. We illustrate the traditional SCM of Abadie et al. (2010) with New Hampshire as the treated unit.

[20] In order to make this calculation, we make two assumptions. First, we are assuming that the Texas NFCS loan application rate equals the actual rate. However, even using the much-lower FDIC application rate of Texas of 4% as a conversion factor, we would still rule out the resultant effect size. The second assumption we make is that removing access to payday loans has the same effect—in absolute value—on bankruptcies as "granting" access through an application.

Table 6
Balance check for synthetic New Hampshire.

Dependent variables	Composition of ynthetic NH	Pre-treatment mean NH	Pre-treatment mean Synthetic NH	RMSPE
Payday lenders	HI 0.39; IN 0.13; RI 0.19; UT 0.041; WY 0.248	2.602	3.261	0.289
Overall bankruptcy	DE 0.01; HI 0.63; IA 0.19; NV 0.01; RI 0.07; UT 0.03; WY 0.05	71.767	75.529	0.253
Chapter 7 bankruptcy	DE 0.02; HI 0.46; RI 0.05; SC 0.05; TX 0.02; UT 0.03; WI 0.12;WY 0.27	61.899	67.719	0.259
Chapter 13 bankruptcy	DE 0.01; HI 0.30; IN 0.01; IA 0.03; RI 0.16; WI 0.21; WY 0.28	9.824	10.312	0.364
Student loan delinquency	HI 0.19;; IN 0.51 ND 0.08; RI 0.04 UT 0.03; WI 0.05; WY 0.10	6.288	6.362	0.094
Auto loan delinquency	RI 0.17; UT 0.01 WI 0.72; WY 0.11	1.825	1.970	0.255
Mortgage delinquency	DE 0.01; HI 0.95; UT 0.02; WY 0.02	1.048	1.136	0.242
Credit card delinquency	DE 0.01; HI 0.05; IN 0.614; UT 0.04; WI 0.12; WY 0.17	7.478	7.525	0.113

Notes: The SCM algorithm lets the data choose the optimal weights to minimize pre-treatment fit between the series. The weights are constrained to be positive and sum to one. The weights are chosen strictly according to fit and the data, not distance, similarity, etc. RMSPE is the root mean squared prediction error (divided by the pre-treatment NH mean), which measures fit, where lower is better.

There are two reasons why we single out New Hampshire as a case study.[21] First, of the four treated states, New Hampshire presents the strongest argument that there is no contamination from neighboring states. Throughout our study period, 2001–2016, all of New Hampshire's neighbors (and Quebec) had bans in place. Hence, New Hampshirites were completely shut out of the storefront payday loan market; they cannot cross state lines to access payday loans. This lack of contamination is important because it means that New Hampshire's ban will have its "true" effect for the state.[22] Other states' bans will not translate to their presumed reduction in bankruptcies because residents may still be getting access in neighboring states. Melzer (2011) documents this for Georgia.

The second reason New Hampshire is presented as a case study is that it was the first of the four treated states to implement its ban. This is relevant because it gives us the greatest number of post-treatment observations in order to assess the degree to which a payday ban affects consumers in the long-run.

Table 6 shows the balance of New Hampshire with its synthetic control unit. Before the intervention, the mean for New Hampshire was close to the mean of "synthetic New Hampshire." The near-equality of means is imposed by the convex hull assumption (treated unit is not an outlier) of the SCM optimization problem. The weights are also listed. One critique is that we are comparing New Hampshire to states that are thousands of miles away like Hawaii. This is true, but it is by design: demographically, New Hampshire may not be anything like Hawaii, but bankruptcy-filing-wise, it is. To be sure, we also do a "simple" DD putting 100% weight on nearby state Rhode Island, and then again by adding nearby Delaware; results are largely unchanged and are available upon request.

Figs. 3 and 4 plot the values of the outcome variable over time for New Hampshire (solid line) as well as the value of the synthetic New Hampshire (dashed line) for establishments & formal bankruptcy and delinquencies, respectively. In the pre-treatment period (vertical line), the plots provide a visual analogue to the balance from Table 6. In these visuals, the relaxation of the parallel trends assumption is immediately apparent. The difference in the pre-treatment period is the first term on the right-hand side of Eq. (4.3). In the post-treatment period, any divergence between the two series would be attributable to the policy intervention. The difference in the post-treatment period is the second term on the right-hand side of Eq. (4.3). We can then take the difference in these two differences as an estimate of the treatment effect. The results are in Table 7.

The results in Table 7 for establishments have the same sign and general magnitude as the OLS DD results from Tables 3 and 5. Regarding formal bankruptcy, the combined ("overall") measure has a positive sign throughout nearly all post-treatment periods. Disaggregating this measure, we see that Chapter 7 filings are also positive in the early post-treatment periods, but eventually decline and eventually turn negative 10 quarters later, which is qualitatively consistent with the findings of Skiba and Tobacman (2019), who find that the causal effect of payday loan access—measured as first loan application—on bankruptcy filings increases over time (their Fig. 8) up to 2.25 years out. In contrast, our lowest magnitude, regardless of measure, comes 26 quarters (6.5 years) later among Chapter 7 filers—approximately 15 per 100,000 per quarter, or 60 per 100,000 per year. Inference in SCM is placebo-based (see discussion below) so we don't have confidence intervals *per se*, but we can appeal to the range: we never observe a result as large (in absolute value) as Skiba and Tobacman (2019) for any measure of bankruptcy. And for Chapter 13 filings, we never observe an effect as large as Morgan et al. (2012).

Regarding delinquencies, student- and auto loans have a negative sign (in contrast to our DD results). The mortgage delinquency estimates are much lower than our DD estimates. The credit card delinquency estimates have a positive sign, which is inconsistent with the debt trap hypothesis, but is consistent with consumers substituting toward paying their credit cards late (see Section 5.5 and Table 10 for a discussion on this point).

Figs. 5 & 6 show the placebo-based inference results for formal bankruptcy; Figs. 7 & 8 show them for delinquencies. The dark line in Fig. 5 (and 7) is a plot across the sample period of the treatment effect, i.e., the values in Table 7, which is the difference between New Hampshire and synthetic New Hampshire. The lighter-shaded, gray lines are the results of the placebo tests—a plotting of the difference between each placebo and *its* synthetic control unit. Each placebo unit is a member of the donor pool, i.e., the 30 control states in Table 2. At each post-treatment period there is a distribution of the treatment effects: the true treatment effect of NH, and the 30 placebos. This empirical distribution allows us to calculate rank-based p-values. Fig. 6 (and 8) plot these p-values for each post-treatment period; the horizontal, dashed line is 10%.

In the pre-treatment periods, the dark line in Figs. 5 and 7 hovers about the zero axis, which is another manifestation of the minimization of the distance between the synthetic control unit and the treated unit. In the post-treatment periods, the plots of the p-values in Figs. 6 and 8 confirm that any post-treatment divergence is not statistically meaningful. The sole exceptions are the payday loan establishments—proxied by NAICS 522390—and student loan delinquency for a few years after. The number of payday lending

[21] We do the same analysis for the other treated states—AR, AZ, MT—and the results (available upon request) are largely the same. We also provide the visual analysis of South Dakota in the appendix, Figures A.1 and A.2.

[22] This lack of contamination means that our estimates will give us a lower bound on the total effect of the ban since neighboring residents who were coming into NH have now been cut off. We thank an anonymous referee for bringing this to our attention.

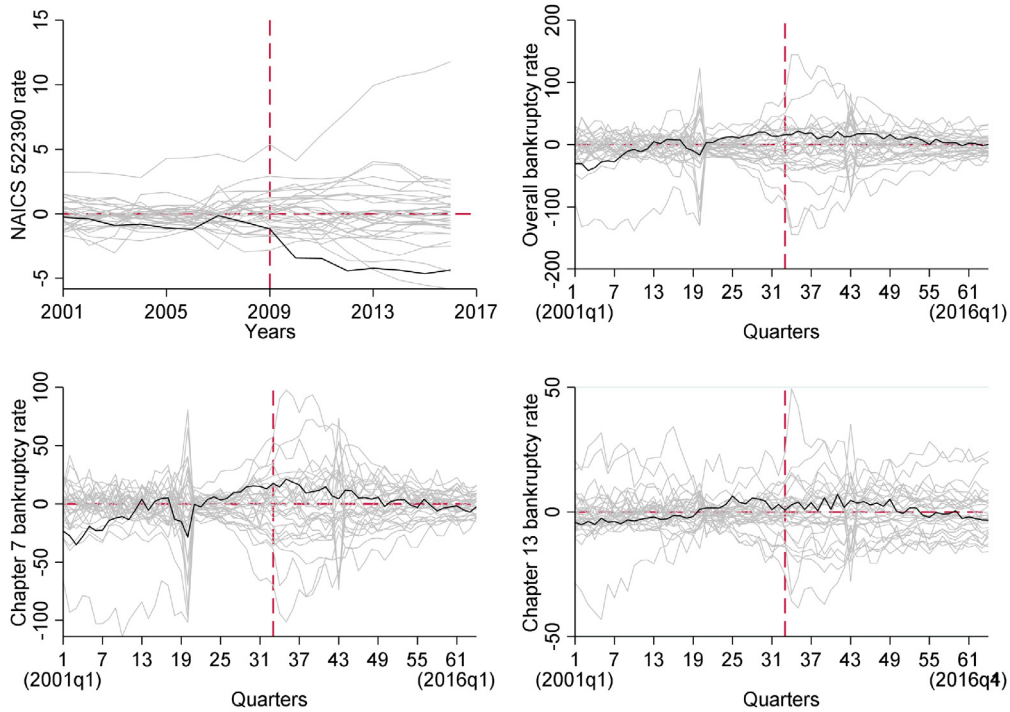

Fig. 5. Visual placebo tests: New Hampshire formal bankruptcy.

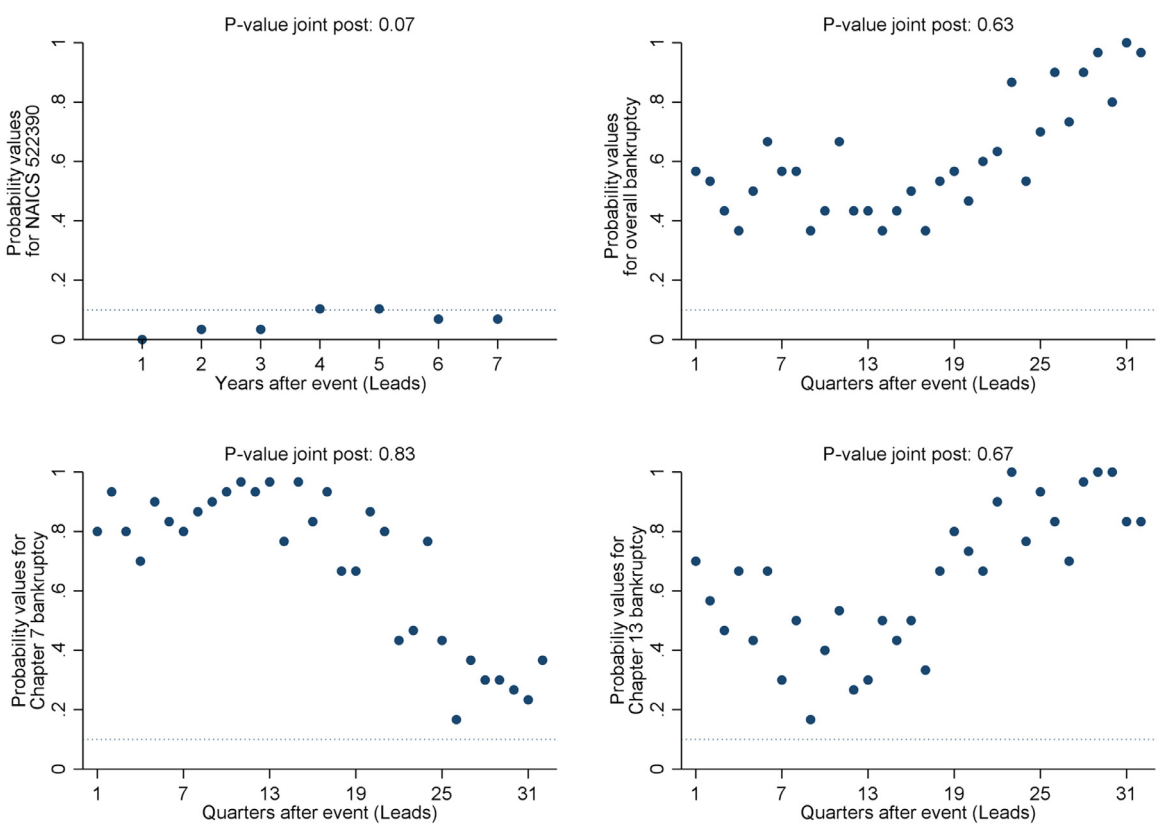

Fig. 6. Two-tailed p-values: New Hampshire formal bankruptcy.
Notes: Fig. 5: the bold line represents the difference between the actual series and the synthetic series over time. The faded gray lines represent the placebos; the program iteratively runs through each of the control states and estimates a synthetic control path. Fig. 6: the p-values are from two-tailed tests for each post-implementation period following the payday lending restriction. 'Joint post' represents the proportion of placebos with a post-treatment RMSPE at least as large as the average for the treated units..

Table 7
Estimated treatment effects for New Hampshire.

Post- treatment period	Payday lenders	Overall filings	Chapter 7 filings	Chapter 13 filings	Student loan	Auto loan	Mortgage loan	Credit card
1	−2.546	17.255	3.938	2.573	0.300	−0.545	0.213	0.880
2	−2.601	18.448	1.514	5.417	−0.590	−0.124	0.137	1.503
3	−3.764	25.009	7.940	6.622	−1.527	−0.045	−0.028	1.558
4	−3.528	20.070	7.419	3.195	−2.276	−0.059	−0.102	1.373
5	−3.773	23.525	3.613	6.307	−2.074	0.016	−0.019	0.913
6	−3.949	17.081	2.195	3.440	−1.886	−0.372	−0.064	1.058
7	−3.846	19.750	3.333	9.426	−3.033	−0.470	0.076	0.631
8		17.183	3.030	4.787				
9		25.423	4.349	10.395				
10		21.007	−1.640	4.962				
11		18.196	−1.571	7.000				
12		18.273	1.977	6.186				
13		19.683	1.191	6.683				
14		18.545	−5.270	5.594				
15		18.448	−0.703	6.110				
16		11.496	−1.789	5.337				
17		17.299	−2.730	7.322				
18		11.218	−9.179	2.781				
19		10.026	−5.298	1.644				
20		13.435	−3.884	3.996				
21		11.448	−5.842	3.444				
22		7.901	−12.160	1.071				
23		2.468	−10.224	0.132				
24		9.134	−2.966	1.569				
25		4.882	−8.579	1.002				
26		4.233	−14.942	1.401				
27		5.014	−8.826	2.540				
28		−1.315	−10.213	−0.793				
29		2.373	−8.828	−0.292				
30		2.795	−11.107	−1.013				
31		0.521	−8.846	−1.218				
32		−0.157	−7.959	−1.139				
Observations	496	1984	1984	1984	434	434	434	434
Pre-treatment NH mean	2.602	71.766	61.899	9.820	6.288	1.825	1.048	7.478
Pre-treatment synthetic mean	3.261	75.529	67.719	10.312	6.362	1.970	1.136	7.525

Notes: The values in the table are the estimates of Eq. (4.3) for each post-treatment time period. Payday lenders (the number of establishments NAICS 522390) and the measures of loan delinquencies are at an annual frequency. The measures of formal bankruptcy are quarterly. "Overall" filings is the sum of Chapters 7 and 13 filings. All filings are at the state level per 100,000 people; delinquencies is percentage points of the population. The pre-treatment means demonstrate balance between New Hampshire and its synthetic control unit.

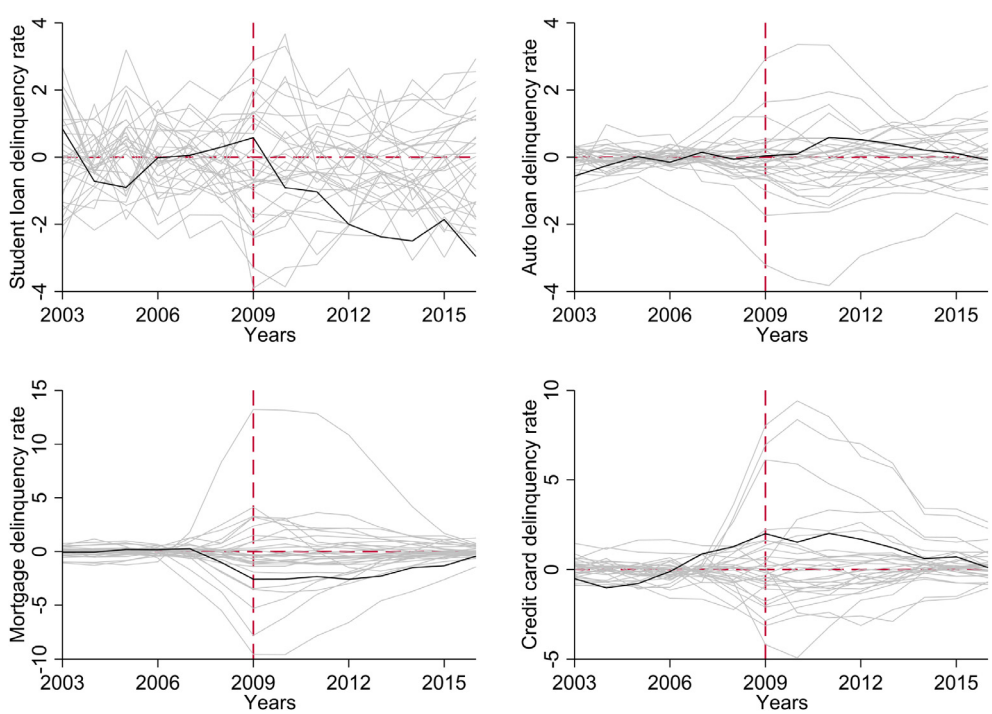

Fig. 7. Visual placebo tests: New Hampshire informal bankruptcy.

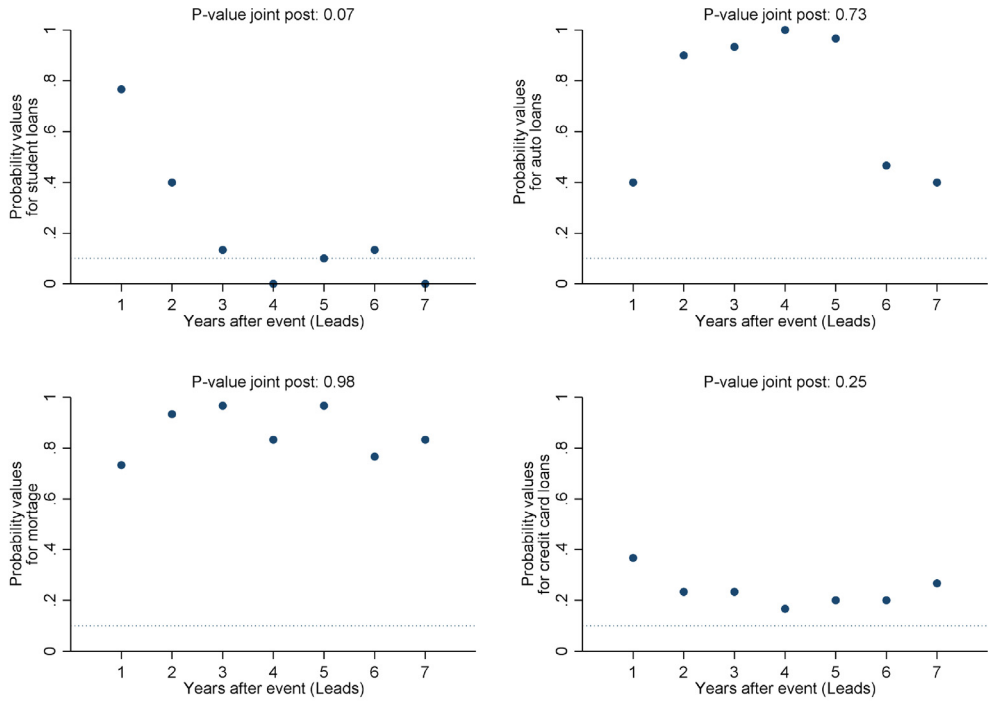

Fig. 8. Two-tailed p-values: New Hampshire informal bankruptcy.
Notes: Fig. 7: the bold line represents the difference between the actual series and the synthetic series over time. The faded gray lines represent the placebos; the program iteratively runs through each of the control states and estimates a synthetic control path. Fig. 8: the p-values are from two-tailed tests for each post-implementation period following the payday lending restriction. 'Joint post' represents the proportion of placebos with a post-treatment RMSPE at least as large as the average for the treated units..

establishments confirms the results from Section 5.1 that the interest rate caps did indeed serve as an effective ban.

The treatment effect for student loans is negative and statistically meaningful at about the 10% level between three years and seven years after. This finding is consistent with Agarwal et al. (2016), who find that between 2007 and 2013, the payday borrowing rates among those who have at least some college more than doubled, from 3.8% to 7.7%. It's possible that these borrowers have student loan debt (though we don't know what percentage of those with student loan debt take out payday loans). It's conceptually possible that New Hampshire's ban reduced student loan delinquency. Or, the effect is a statistical artifact, respresenting perhaps the large weight that Indiana has in the composition of synthetic New Hampshire (51%), a possibility that is consistent with the null findings of our DD analysis (Table 5). Our pooled SCM in the next section revisits this question.

The takeaway from this New Hampshire case study is that there is no statistically-meaningful effect on formal bankruptcies, neither in the short-term (three monthts later) nor the long-term (eight years later)—nor anytime inbetween. Moreover, we do not find any magnitude for formal bankruptcy in accordance with Morgan et al. (2012) or Skiba and Tobacman (2019). We also do not observe the latter's qualitative pattern of an increase in magnitude as time passes. None of the magnitudes that we estimate are statistically meaningful. Regarding informal bankruptcy, we do not observe the qualitative pattern of Gathergood et al. (2019). And here, too, we do not find our magnitudes to be statistically meaningful, with the possible exception of student loans.

The discrepancy between our findings cannot be attributed to payday lenders circumventing the ban, as we have shown—from both the supply and demand sides–that the ban was effective. And because we focus on New Hampshire, our null findings aren't contaminated by cross-state access. Our null findings could reflect the idiosyncracies of New Hampshire or its synthetic control unit, possibilities that we consider in the next section. Or they could reflect the differences in measuring 'payday loan access' by legal restrictions as opposed to measuring it with usage, a possibility that we consider in Section 5.6 below.

5.4. Pooled synthetic control analysis

Our pooled synthetic control results for payday establishments and formal bankruptcy rates are in Table 8. As is evident from Table 8 and subsequently in the visual representation provided in Fig. 9, we find a statistically-significant decline in the measure of payday establishments. This is confirmed by the negative estimate for each of the post-implementation years and p-values from our two-tailed tests. The joint p-value at the bottom of Table 8 indicates the proportion of effects from the control states (placebos) that have a post-implementation root mean squared prediction error at least as great as the treated states in our analysis. The statistical significance (at the 5 percent level) indicated by the joint p-value for our measure of payday establishments further substantiates the evidence of a significant negative impact that the state-level legislations had on payday lenders.

As a contrast, the formal bankruptcy results show no evidence of a statistically meaningful impact of the state-level payday interventions on any of the three measures. This finding holds across each of the post-implementation quarters. Moreover, there is no discernable pattern in the signs or magnitudes across the four (pooled) treated states. Looking at the range of estimates, we can rule out the estimated treatment effects of Morgan et al. (2012) and Skiba and Tobacman (2019), just as we did in our New Hampshire case study.

The statistically-null results also hold for our informal measures of bankruptcy, as the p-values in Table 9 show. The only result of any remote statistical significance is that the joint p-value on student loans is below 5%. Qualitatively, the signs and magnitudes

Table 8
Treatment effects based on pooled synthetic control estimation for payday establishments and formal bankruptcy filings.

Pre-Treatment Mean	NAICS 522390 4.584		Overall 109.490		Chapter 7 78.279		Chapter 13 31.135	
Post-implementation period	Estimate	2-tailed p-value	Estimate	2-tailed p-value	Estimate	2-tailed p-value	Estimate	2-tailed p-value
1	−3.114	0.004	6.937	0.724	8.753	0.569	3.690	0.533
2	−4.092	0.003	14.463	0.505	12.315	0.454	4.638	0.444
3	−4.699	0.003	11.080	0.586	12.900	0.422	0.004	0.999
4	−4.584	0.006	6.301	0.729	9.899	0.492	1.433	0.789
5	−4.311	0.009	3.557	0.842	8.496	0.555	0.336	0.950
6			2.087	0.902	4.950	0.714	0.983	0.859
7			5.722	0.721	9.110	0.481	0.211	0.966
8			9.642	0.575	12.250	0.378	1.437	0.781
9			−0.890	0.952	3.650	0.761	−0.485	0.916
10			−2.119	0.897	0.144	0.991	0.134	0.978
11			1.925	0.880	4.692	0.646	−0.931	0.833
12			2.447	0.838	4.357	0.647	0.600	0.886
13			−1.165	0.922	2.386	0.801	−1.278	0.774
14			−3.532	0.753	−0.898	0.918	−1.955	0.664
15			0.432	0.967	3.579	0.658	−1.543	0.725
16			−0.258	0.979	2.084	0.785	−1.036	0.812
17			−3.960	0.680	−0.312	0.966	−2.469	0.552
18			−5.130	0.587	−0.923	0.894	−3.448	0.432
19			2.118	0.815	4.017	0.545	−1.132	0.790
20			−0.201	0.982	2.153	0.737	−0.655	0.877
21			−3.382	0.702	0.490	0.938	−2.510	0.552
22			−5.723	0.512	−0.614	0.912	−4.032	0.373
23			−0.871	0.918	2.495	0.637	−1.828	0.685
Joint p-value post-treatment	0.015		0.854		0.910		0.977	
Observations	544		2176		2176		2176	

Notes: The above table presents treatment effects (difference between treated and synthetic units) for each post-implementation period estimated from a pooled synthetic control estimation that allows for multiple treated states receiving treatment (payday restriction) at different time points (see Galiani and Quistorff (2017) for details). NAICS 522,390 is the measure of the number of payday loan establishments. The treated states are New Hampshire (2009 January), Arizona (July 2010), Arkansas (Mach 2011), and Montana (January 2011). Based on the availability of information, we conduct a quarterly-level analysis for formal bankruptcy filings and annual-level analysis for NAICS 522,390 establishments. Inferences are provided by comparing the main treatment effects (reported in the table) to the distribution of placebo effects. The placebo effects are derived by synthetic control estimations for the same treatment period on all the states in the control (donor) pool. Joint p-value (post-treatment) represents the proportion of placebos with a post-treatment root mean squared percentage error at least as large as the average for the treated units. For sake of completeness, we also calculated one-tailed p-values and none of the results are significant at conventional levels.

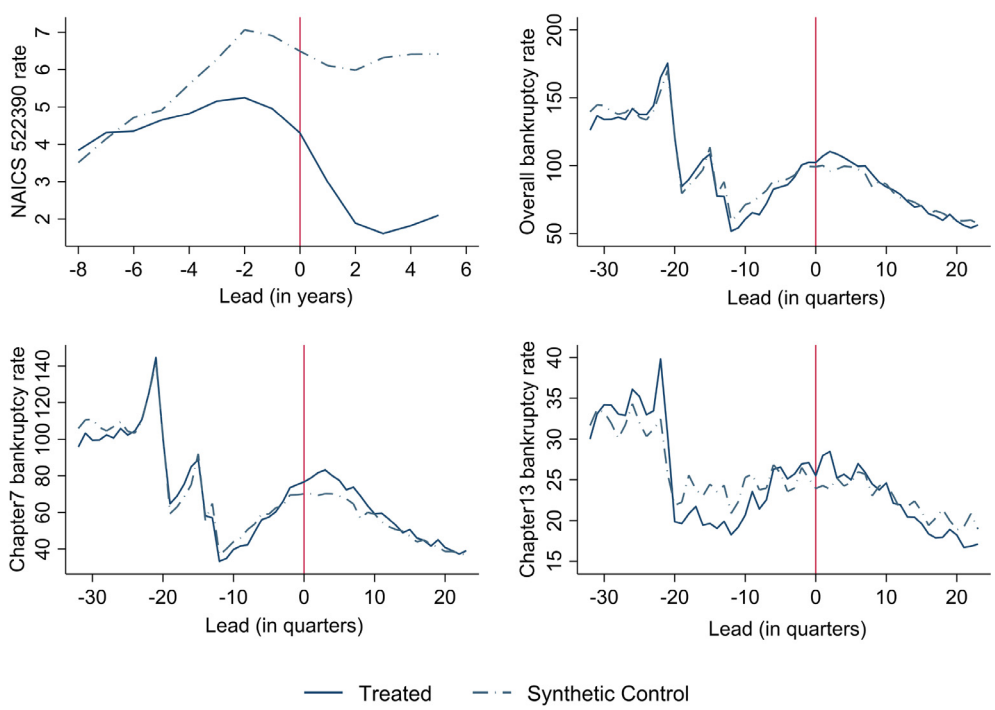

Fig. 9. Visual pooled synthetic control estimation for payday lenders and formal bankruptcy.

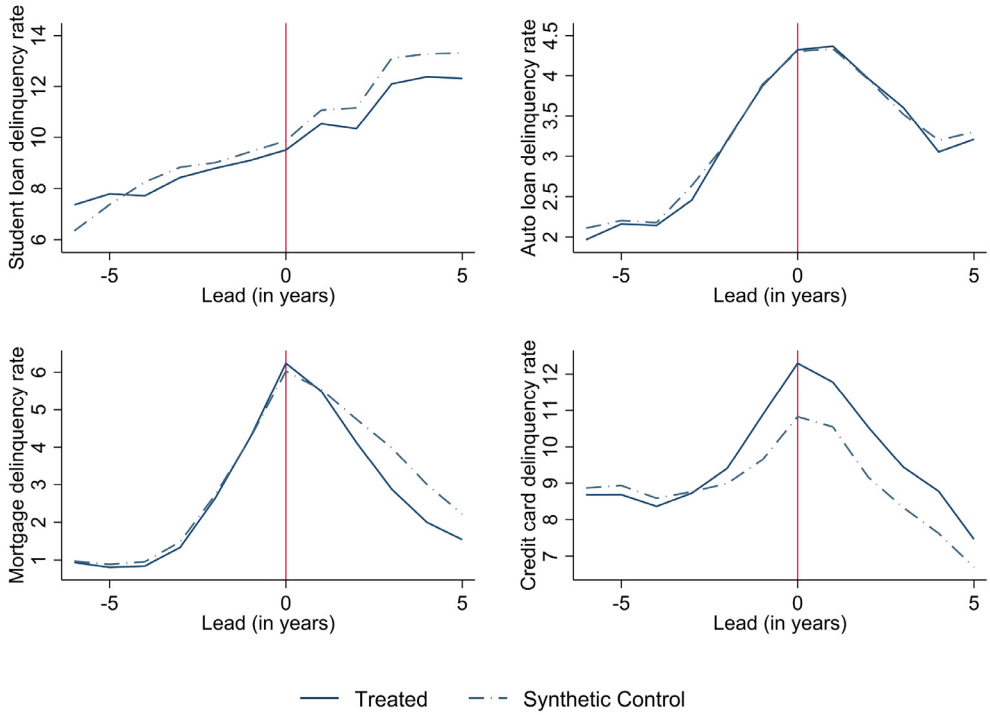

Fig. 10. Visual pooled synthetic control estimation for informal bankruptcy (delinquency rates).
Notes: In both figures above, the solid line represents the actual path of the measure throughout the study period. The dashed line represents the path of the synthetic control unit, which is calculated as the optimal weighting scheme that minimizes the distance between the two series. The vertical line is the centered legislative intervention period. Formal bankruptcies are quarterly rates per 100,000 persons; informal bankruptcies (delinquencies) are annual percentage points. The formal bankruptcy results could be confounded by the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCA). We re-ran the analysis without the BAPCA quarters and results are materially unaffected (available upon request). The Great Recession could also confound our results. We run the analysis on South Dakota as a robustness check against this possibility; see appendix Figures A.1 and A.2..

in Table 9 and Fig. 10 do not appear to exhibit any discernable pattern, e.g., short-term vs. long-term delinquencies as they do in Gathergood et al. (2019) for the UK.

A particularly noteworthy finding is that our mortgage delinquency results using the pooled synthetic control method are of a much greater magnitude than those from our New Hampshire case study (and our OLS-DD estimates). One potential reason is that the pooling includes Arizona, which is one of our four treated states. Arizona's housing market, of course, was hit hard by the Great Recession. Hence, it's possible that our results—mortgage and otherwise—are being confounded by this event, since the Great Recession happened close to when these bans were enacted. In fact, it's possible that all of our results are confounded by this event. We consider this possibility in the next section.

5.5. Potential confounders: BAPCA and the great recession

One potential problem with our analysis is the potential confounding effect of the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCA) that went into law officially in October, 2005. The effect of BAPCA can clearly be seen in Fig. 9 for the formal bankruptcy measures. Despite the SCM picking up this effect, it's possible that such a structural break could affect the optimal weighting, and hence the synthetic control unit.

To address this critique, we iteratively perform the structural break test of Zivot and Andrews (1992) for each state. This test allows the data to reveal the most likely date of a structural break, i.e., the date is endogenously determined (see Glynn et al. (2007) for a readable summary of structural break tests). The results and formal writeup are in the appendix, Table A.4. To summarize, 82% of the cases reject the null hypothesis of no structural break at the 10% level for Chapter 7—8% for Chapter 13. This makes sense since the intention of BAPCA was to push borrowers into filing Chapter 13 rather than Chapter 7 (see Lefgren and McIntyre (2009) for details on BAPCA). Foreseeing the law coming (it was signed in June of 2005), Chapter 7 bankruptcies were "moved up," leading to the data picking it up in the form of a structural break. The test reveals that for most of the cases, 2005q3 was the period that yielded the highest significance.

To accommodate this critique, we re-ran the pooled SCM omitting 2005q2-2006q4. Our finding of null results still holds. The signs of the coefficients are mostly negative a few years out, but in no period are any of formal bankruptcy measures statistically significant.

Another potential confounder is the Great Recession. Indeed, for some states, our structural break test chose this period as the most significant break point. The Great Recession was national, but there's a possibility that it affected bankruptcies differently, most notably home mortgage delinquencies. To accommodate this critique, we perform a synthetic control analysis on South Dakota, which imposed a 36% interest rate cap in 2016, well after the Great Recession and its lingering effects. We ran the South Dakota analysis with- and without the Great Recession quarters. The visual plots for South Dakota are Figures A.1 and A.2 in the appendix. Our findings of null results hold up: formal bankruptcies have a negative sign, but are statistically insignificant.

5.6. Usage vs access: Analysis on a population of payday borrowers

One critique of our analysis so far is that the population of people who file for bankruptcy may not overlap with the population of people who take out a payday loan. That is to say, perhaps

Table 9
Treatment effects based on pooled synthetic control estimation for payday establishments and informal bankruptcy.

	Student Loan		Auto Loan		Mortgage		Credit Card	
Pre-treatment mean	8.321		2.791		2.322		9.612	
Post-implementation period (years)	Estimate	2-tailed p-value	Estimate	2-tailed p-value	Estimate	2-tailed p-value	Estimate	2-tailed p-value
1	−0.542	0.489	0.033	0.940	−0.046	0.970	1.233	0.353
2	−0.825	0.212	0.010	0.981	−0.619	0.524	1.381	0.246
3	−1.005	0.133	0.083	0.808	−1.104	0.203	1.110	0.256
4	−0.899	0.167	−0.145	0.667	−1.004	0.133	1.151	0.171
5	−1.005	0.163	−0.089	0.800	−0.678	0.142	0.765	0.199
Joint p-value post-treatment	0.018		0.683		0.558		0.543	
Observations	476		476		476		476	

Notes: The above table presents treatment effects (difference between treated and synthetic units) for each post-implementation period estimated from a pooled synthetic control estimation that allows for multiple treated states receiving treatment (payday restriction) at different time points (see Galiani and Quistorff (2017) for details). The treated states are New Hampshire (2009, January), Arizona (July, 2010), Arkansas (Mach, 2011), and Montana (January, 2011). Based on the availability of information, annual-level analysis for loan delinquency. Inferences are provided by comparing the estimated main treatment effects to the distribution of placebo effects. The placebo effects are derived by synthetic control estimations for the same treatment period on all the states in the control (donor) pool. For sake of completeness, we also calculated one-tailed p-values and none of the results are significant at conventional levels.

Table 10
Conditional OLS DD analysis of bankruptcy and financial well-being.

Source	FINRA National Financial Capability Study			
	Bankruptcy	Late on credit card payment	Difficulty paying bills	Overall financial condition
Study period	2009, 2012, 2015			
Sample mean	0.085	0.474	1.89	4.333
Payday loan restriction	0.061	0.124**	−0.006	0.337
	(0.086)	(0.054)	(0.098)	(0.384)
State fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Individual characteristics	✓	✓	✓	✓
Sample size	6,821			

Notes: * is 10%; ** is 5%; *** is 1% significance. The data are filtered to those who answered "yes" if they took out a payday loan in the previous five years. The survey bankruptcy instrument is as follows: "have you declared bankruptcy in the last two years?" The credit card late payment instrument is as follows: "in the past 12 months, which of the following describes your experience with credit cards? In some months I was charged a late fee for late payment." The 'difficulty paying bills' instrument is as follows: "in a typical month, how difficult is it for you to cover your expenses and pay all your bills?" The scale is 1–3 with 1 being "very difficult" and 3 being "not at all difficult." The overall financial condition instrument is as follows: "how satisfied are you with your current personal financial condition?" The scale is 1–10 with 1 being "not at all satisfied" and 10 being "extremely satisfied." The reported results are from LSDV-LPM regressions. We also ran probit and logit (ordered probit for 'difficulty paying bills' and 'overall financial condition'), but the results are not materially affected. Individual characteristics include gender, age, and race. State-level characteristics are absent because of the phrasing of the preliminary payday-filtering question: 'in the past five years' would introduce arbitrariness into the estimation of the standard errors. In any case, the standard errors in the table, then, are a conservative estimate. All standard errors are cluster-robust, being clustered at the state level.

the reason that we find null effects is that our measure of payday loan access is availability rather than loan application or approval. To address this concern, we turn to the NFCS survey and condition the analysis on the population who have taken out a payday loan.[23]

Our results show that there is no statistically-meaningful effect of payday loan bans on bankruptcies, difficulty in paying bills, or overall financial condition. These results generally bolster our bankruptcy analyses above: there is no discernable effect on bankruptcies, both at the aggregate level as well as the disaggreated level, that is, conditioned on the population of people who have shown themselves willing to take out a payday loan. However, these findings seem to contrast with the survey-data analysis of Melzer (2011), who finds that general financial hardship—including difficulty paying bills—increases with payday loan access. One potential explanation for the difference in these findings is that Melzer's sample period is from 1996 to 2001, a time when the industry was relatively young. Hence, it's possible that as the industry grew and changed, so too did its customers.

A more recent survey-data analysis is Zinman (2010), who finds that Oregon's 2007 payday lending ban—implemented as a 36% APR interest rate cap—caused would-be borrowers to pay their bills later. Our findings in Table 10 corroborate Zinman's findings: payday lending bans led to an approximate 12% increase in late credit card payment. Our findings also bode well with a report by Pew Research, Pew (2012), which presents qualitative evidence that 62% of respondents said that they would "delay paying some bills" if payday loans were to become unavailable. Since the late fees are assessed in the subsequent month, they function as though they were payday loans, e.g., convenient, short-term, high-cost, etc. In fact, the substitutability across these financial assets could explain why we see no change in bankruptcies after the bans: people are substituting to other means of finance. See Bhutta et al. (2016) and

[23] The NFCS asks whether the respondent has taken out a payday loan in the past five years. We filter out all of the 'perennial banning' states, e.g., New York. We then filter the sample to only those who have taken out a payday loan in the past five years. We code the BAN variable (see Eq. 4.1) as 0 for all states for 2009, but a 1 for AR, AZ, NH, and MT for survey years 2012 and 2015. This is our treatment variable. The outcome variables are listed in Table 10. To be sure, the 'five years' will likely bias our estimate downwards as the result of measurement error (underreporting).

the sources therein regarding the substitutability of payday loans with alternative sources of finance.

6. Conclusion and future research

Payday loans have the potential to kick off a cycle of debt, where a high APR leads to difficulties in loan repayment, which necessitates a rolling over of another high APR loan, and so on. If consumers become trapped in this debt cycle it could ultimately lead to bankruptcy, either formal (Chapter 7, 13) or informal (loan delinquency). State legislatures have responded to the potential for this debt trap: recently, states passed legislation that caps the interest rate on small loans.

In this paper we show that interest rate caps are effective in banning the industry. Given that interest rate caps are effective, we should see a decrease in bankruptcy rates if the 'debt trap' hypothesis is as strong as many consumer advocates say it is. Using recent payday bans as quasi-experiments, we perform synthetic control analyses and show that, on the whole, bankruptcies and delinquencies are generally unchanged. These results seemingly conflict with a few recent studies. Gathergood et al. (2019) find that payday loan usage causes delinquencies to fall in the shorter-run, but rise in the longer-run. We do not observe this pattern with our data. Skiba and Tobacman (2019) find that usage has a higher causal effect on bankruptcy as time goes on. After scaling up their estimates, we find that we can rule out the magnitude of their treatment effect—even up to 32 quarters out.

There are several possibilities that reconcile our results to theirs. One is that we are using state payday bans as a proxy for access, while several studies use payday loan usage. We pursue this hypothesis by analyzing a national survey of self-reporting payday loan users. We still find that bankruptcies are unchanged.

Another possible reason for the discrepancy in findings is that the local average treatment effects are indeed very local–the findings hold only near the threshold. Alternatively, it could be that by studying access rather than usage, there is some causal channel that is offsetting the debt trap mechanism. In other words, maybe it's true that payday loans do indeed cause bankruptcy for some people, i.e., those near the threshold, but those who are away from the threshold substitute toward alternative financing, e.g., credit card late fees, which we find in our analysis, and others have found as well. Thus, a fruitful avenue of future research would be examining the heterogeneity of the populations who use these alternative financing channels.

Declaration of Competing Interest

The authors hereby acknowledge that the analysis does not involve any conflict of interest, as per the journal requirements.

CRediT authorship contribution statement

Kabir Dasgupta: Conceptualization, Data curation, Methodology, Formal analysis, Writing - review & editing. **Brenden J. Mason:** Conceptualization, Writing - original draft, Writing - review & editing, Formal analysis, Methodology.

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Supplementary material

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