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ORIGINAL ARTICLE

CONTEMPORARY ECONOMIC POLICY

Do payday lending bans protect or constrain regional economies? Evidence from the Military Lending Act's final rule

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Abstract

The 2007 Military Lending Act attempted to ban high-interest loans to U.S. military members and the 2017 "Final Rule" further restricted access, causing regional shocks in payday lending exposure in counties with a military base. Difference-in-differences and dynamic estimators provide mixed evidence on the effect of this payday lending access shock on regional economic outcomes and local business outcomes. However, we consistently find statistically significant reduced entry and exit of firms. Given payday lenders congregate around low-income and minoritized populations analogously to how they congregated around military bases, these results provide policy implications for more general usury bans.

KEYWORDS

high-interest, payday loans, regional economic development

JEL CLASSIFICATION

R11, G28, G23, D53

1 | INTRODUCTION

The Military Lending Act (MLA) banned high-interest payday lenders from loaning to active-duty military members and their families. Military leaders testified that high-interest lenders were targeting military bases and that the concentrated availability of payday lenders around military bases increased the use of these loans. They further testified that these loans were exploitative and that they weakened morale and readiness. Research on individual-level effects on military members of the original MLA are positive (Carrell & Zinman, 2014) or null (Carter & Skimmyhorn, 2017) in terms of readiness and credit. However, the original MLA contained loopholes which were closed in 2017 when the "Final Rule" introduced amendments to the MLA that further restricted credit access. No work has examined the broader effects of the Final Rule—which we show caused declines in payday-lender exposure in some geographies—on regional economies.

This paper shows that, consistent with the testimony of military leadership and journalists, counties with a military base had substantially higher payday lenders access than other counties prior to the Final Rule. This paper then shows that the Final Rule caused shocks (reductions) to this access in military base counties. Standard two-way fixed effects (TWFE) difference-in-differences (DID) and dynamic estimates indicate mixed effects of the Final Rule (and reduced payday loan access) on county-level regional economic outcomes, with several of the outcomes not remaining significant under a variety of robustness checks. Specifically, due to the existence of pre-treatment trends for some outcomes, we provide estimates from multiple specifications that respectively test the inclusion linear difference trends, county-linear trends, linear-spline trends, and robustness to deviations from pre-treatment trends. We also test robustness to potential anticipation effects and alternative geographic aggregations (commuting zones). We then test potential regional entrepreneurial mechanisms like establishment birth rates and nonemployer receipts using the same methods. In sum, this paper will thus examine the research question: what was the effect of the Final Rule on regional economic outcomes?

In addition to contributing to research on the effect of the MLA, this article provides additional evidence to the effect of banning high-interest lending more generally, an ongoing policy consideration at the federal and state levels. (Some states have now limited usury lending more generally.) Evidence on the effects of banning payday loans is mixed (Ramirez, 2020). Some research indicates beneficial effects of banning payday lenders, like reducing personal bankruptcy rates (Skiba & Tobacman, 2019) and improved well-being (Dobridge, 2016), negative effects like increased credit delinquencies (Barth et al., 2020; Desai & Elliehausen, 2017; Morgan et al., 2012; Zinman, 2010), or null effects on financial well-being (Bhutta et al., 2015; Dasgupta & Mason, 2020).

The variety of effects may be due to heterogeneity exacerbated by smaller samples and geographically limited cross-border analyses (Bolen et al., 2020). This article has the advantage of examining effects across the United States in a wide variety of locations (Figure A1). Additionally, this article is the first to examine the broader regional economic effects of bans and the first to examine the effects of the Final Rule. Given payday lenders congregate around low-income and minoritized populations (Barth et al., 2015) analogously to how they congregated around military bases, these results provide important policy implications for more general usury bans.

This article is organized as follows. First, Section 2 provides additional background on the MLA and its subsequent amendments, including the Final Rule. Then Section 3.1 details our data and documents the differential in payday lender access in counties with a military base relative to counties without a military base. We show that the MLA and its subsequent amendment years coincide with shocks to payday lender access on base counties. Next, Section 3.3 discusses our estimation strategies and robustness checks. Section 4 provides estimates of the effect on regional economic outcomes and then tests entrepreneurial outcomes as potential mechanisms. Finally, Section 5 summarizes and concludes with policy implications. The appendix provides robustness checks for anticipation effects and alternative geographic aggregations (commuting zones).

2 | BACKGROUND: THE MILITARY LENDING ACT

The MLA was enacted in 2006 (and became effective October 2007) with the stated goal of protecting active duty military members, their spouses, and their dependents from certain lending practices. In 2013, the MLA was amended such that enforcement became federal. In July 2015 (to become effective October 2016), the Department of Defense (DoD) issued a another amendment (the 'Final Rule') that extended the protections of the MLA to a broader range of credit products, including credit cards. Summarizing, the amended MLA imposes a 36% rate cap, bans mandatory arbitration, imposes other restrictions, and requires disclosures for "consumer credit," as enforced by the Federal Trade Commission (Federal Reserve Board, 2016). Figure 1 provides a timeline and specific details related to the MLA and its amendments.

The MLA was enacted after the DoD and popular media reported that active duty military member and their families were being targeted by payday lenders. Specifically, in 2006, the DoD released a "Report On Predatory Lending Practices Directed at Members of the Armed Forces and Their Dependents" in which it reported "...predatory lending undermines military readiness, harms the morale of troops and their families, and adds to the cost of fielding an all-

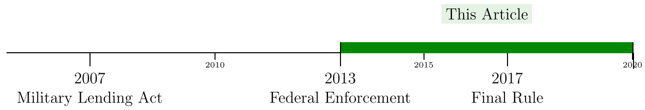

FIGURE 1 Timeline of Military Lending Act (MLA) and its amendments. The 2007 MLA (enacted in 2006 and effective October 2007) only applied to narrowly defined payday loans, motor vehicle title loans, and tax refund anticipation loans with particular terms. The first amendment changed enforcement authority to the Federal Trade Commission, the Federal Deposit Insurance Corporation, Board of Governors of the Federal Reserve System, the Consumer Financial Protection Bureau, the National Credit Union Administration, and the Office of the Comptroller of the Currency. The 2017 final amendment (enacted in July 2015 to become effective October 2016) is broader in its definition of payday loans, and further includes credit cards, deposit advance products, overdraft lines of credit, and installment loans for which the purchased good is not itself collateral (Federal Reserve Board, 2016).

volunteer fighting force" (Department of Defense, 2006). The DoD reported that payday lenders target military personnel "through proximity and prevalence around military installations" and their "ubiquitous presence around military installations" and provides maps of the numerous high-interest lenders specifically surrounding large military bases. Indeed, the report repeatedly emphasizes the importance of proximity to military bases by, for example, writing, "Payday lenders are heavily concentrated around military bases" and that military bases are "among the most heavily targeted communities in their respective states" (Department of Defense, 2006). The report concludes by also documenting the deceptive practices of some lenders targeting military bases, including "official looking seals" and promises like "If you're serving...you're pre-approved!" (Department of Defense, 2006).

Active Duty military members are younger on average, so they have less financial experience, less savings, and lower incomes, making them more likely to need high-interest loans. But they also have a consistent paycheck, making them ideal targets for payday lenders (Hartman, 2013; Lawrence & Elliehausen, 2008). Resultantly, military bases make ideal locations for high-interest lenders to locate and *the sheer availability of payday loans near bases increases use* (Silver-Greenberg & Eavis, 2013). Sections 3.1 and 3.2 empirically verify that counties with a military base had substantially more payday lender establishments and payday lender employment than counties without military bases, and that the Final Rule year coincides with a negative shock to the number of payday lenders in counties with a military base.

3 | ESTIMATION

3.1 | Data

Our data sources are all publicly-available U.S. county-level measures including regional economic outcomes, employer outcomes, nonemployer outcomes, and demographic/economic control variables. We examine the year 2013-2020 because the amendment in 2013 may influence pre-treatment trends and 2020 were the most recent data available upon the writing of this article.[1] Sources include the U.S. Census Bureau's County Business Patterns using Bartik et al. (2017), U.S. Bureau of Labor Statistics, U.S. Small Area Income and Poverty Estimates, U.S. Nonemployer Statistics, and the U.S. American Community Survey and Decennial Census.[2] To create a list of counties that contain a military base, we overlaid a geospatial dataset that contain the authoritative boundaries of commonly known DoD installations, ranges, training areas, bases, forts, camps, armories, and centers, as compiled by the Defense Installation Spatial Data Infrastructure Program (Figure A1).[3] Following Carter and Skimmyhorn (2017), control variables included in robustness specifications are total population, share of the population over 25 with a bachelor's degree, and the median two-bedroom rent. Table 1 provides descriptive statistics by base and non-base counties and shows the large differential between payday lender exposure on and off-base. Specifically, base counties have 2.04 times as much payday lender employment and 1.3 times as many payday establishments per capita on average over our observation period. Figure 2 shows the total and detrended difference over time, with the shock coinciding with the Final Rule year.

3.2 | Military base status as payday lender access shock

Previous research on the effect of the MLA used individual-level data to look at direct effects of MLA on active duty military members, without attention to the potential for regionally variation in effects (Carrell & Zinman, 2014;

TABLE 1 Descriptive statistics.

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample		Base counties		Non-base counties	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Descriptive						
Payday employment	32.99	236.64	159.32	603.68	13.27	70.57
Payday establishments	5.40	23.22	21.55	57.21	2.88	8.08
Regional economic outcomes						
Income per capita	41,733.52	11,890.93	45,701.77	12,434.64	41,113.93	11,683.11
Average wage	40,446.22	8997.29	47,233.22	11,315.46	39,386.53	8080.24
Poverty rate	18.65	7.27	16.70	5.93	18.96	7.41
Employer outcomes						
Total employment	40,777.52	146,645.36	156,786.41	328,757.17	22,664.39	74,585.56
Total establishments	2590.43	8812.58	9452.59	20,135.15	1519.00	4243.27
Establishment entry rate	8.38	2.47	9.00	2.09	8.28	2.51
Establishment exit rate	8.24	2.15	8.42	1.54	8.22	2.23
Nonemployer outcomes						
Nonemployer establishments	8424.37	32,386.35	31,551.36	77,586.03	4813.42	13,285.69
Nonemployer receipts	401,443.92	1,664,184.43	1,528,826.31	3,951,950.28	225,419.27	731,257.01
Controls						
Population	108,513.03	338,757.27	388,481.10	788,725.95	64,800.02	146,371.10
Prevailing rent	762.51	202.25	914.93	288.17	738.71	173.6
Bachelor's degree share	20.86	9.1	26.89	9.99	19.92	8.58
Observations	24,712		3208		21,504	

Note: Each two columns contains the descriptive statistics for all counties, counties with a base, and counties without a base. Payday lender employment and establishment counts are provided descriptively, while the following italicized row subheadings organize our set of main outcomes and potential mechanism outcomes. Total employment and Total establishments exclude payday lenders here and throughout this article. Data sources are the U.S. Census Bureau's County Business Patterns using Bartik et al. (2017) for payday and employers, U.S. Bureau of Labor Statistics, U.S. Small Area Income and Poverty Estimates, U.S Department of Housing and Urban Development Fair Market Rents, U.S. Nonemployer Statistics, and the U.S. American Community Survey and Decennial Census. Years are 2013–2020 except for the Nonemployer Statistics, which were not available for 2020.

Carter & Skimmyhorn, 2017). But military leaders, popular media, and prior qualitative evidence (all described in Section 2) provide evidence payday lenders were *increasingly* targeting military bases in particular. Figure A1 provides a map of counties with a military base in the United States and highlights the regional diversity of locations included herein. It is sensible that it would be hard to target active duty military members more broadly and military base location would provide a heuristic for the locations of payday lenders target population.

Figure 2 empirically verifies that counties with a military base had more payday lender establishments and payday lender employment than counties without military bases and that a shock to the difference coincides with the Final Rule year.[4] More specifically, Figure 2a–c show an inflection point around the Final Rule year. Figure 2c in particular highlights increasing relative targeting of military bases counties prior to the Final Rule (in terms of number of payday lending establishments and payday lending employment), followed by a reduction and reversal of this trend between military and non-military base counties. This increase prior to the Final Rule is consistent with the testimony of military leaders and journalists that payday lenders were *increasingly* targeting military bases, which was motivational for adding additional restrictions to the MLA with the Final Rule.[5,6]

Figure 3a,b plot dynamic estimates of the final rule impacts on payday lender employment and establishments, respectively. We find statistically significant evidence of a decline in payday lender establishment exposure resulting from the final rule.

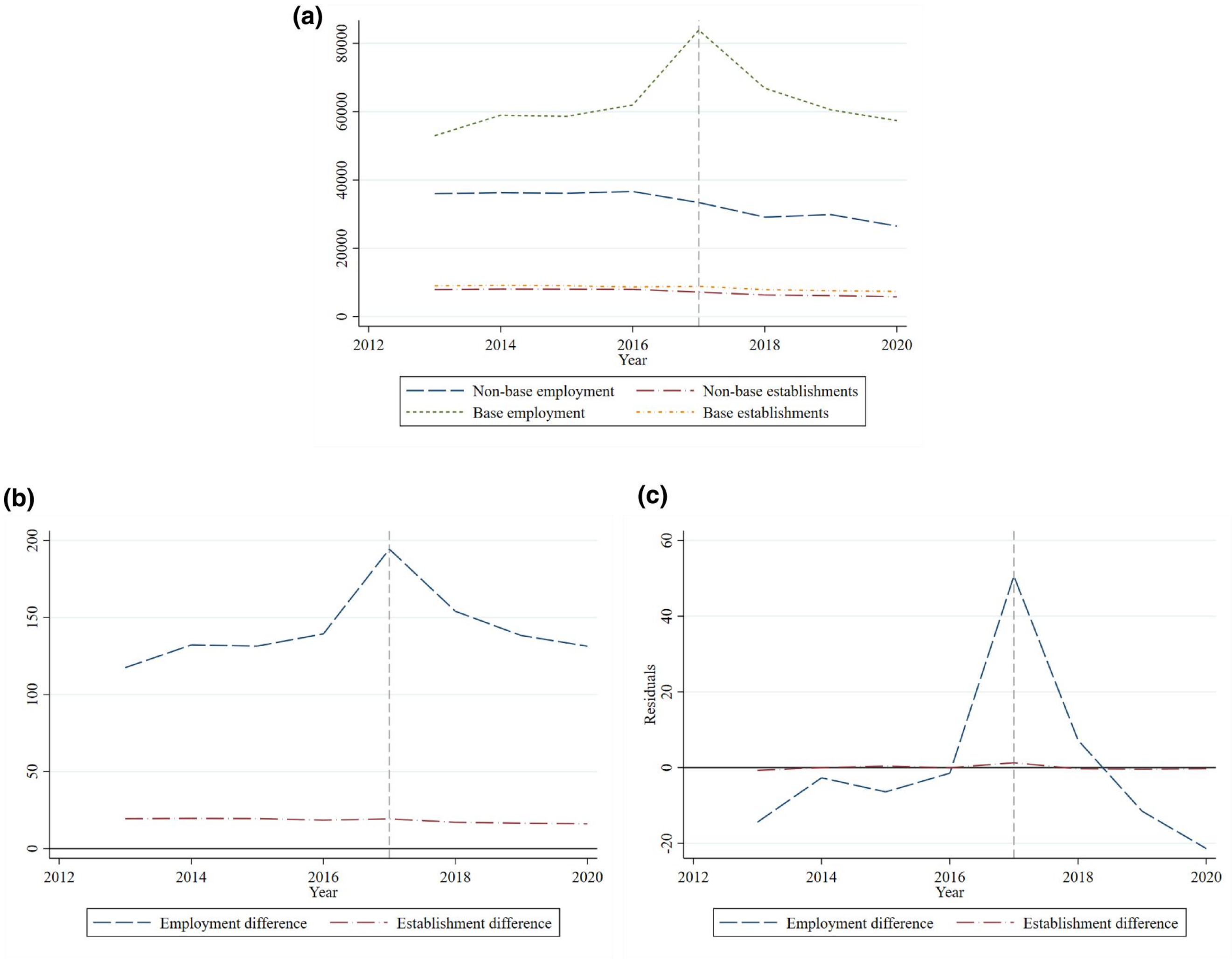

FIGURE 2 Trends in Base v. Non-Base Payday Lenders. (a) Trends. (b) Difference. (c) Difference detrended. Figure shows trends in payday lender employment and number of establishments in base and non-base counties following suggestions by Kahn-Lang and Lang (2020) (panel 2a), the differences between base and non-base counties (panel 2b), and in the detrended (and centered at zero) difference between base and non-base counties (panel 2c). Data is from Bartik et al. (2017).

3.3 | Empirical strategy

We begin with a standard TWFE DID estimator:

$$y_{ct} = \beta_1 Base_c \times Post2017_t + \alpha_c + \sigma_t + \epsilon_{ct} \quad (1)$$

where y_{ct} is outcome in county c in year t, $Base_c$ indicator if county c has a military base, Post 2017_t indicator if after 2017, and α_c and σ_t county and year fixed effects. β_1 is the effect estimates of the 2017 MLA Final Rule. In separate specifications, we also test the robustness of including county-year-level control variables ($\beta_2 X_{ct}$), following Carter and Skimmyhorn (2017),[7] a linear trend difference ($\theta Base_c \times \sigma_t$), county-linear trends ($\theta \alpha_c \times \sigma_t$), and linear spline trends following Bilinski and Hatfield (2019).

The timing of the treatment variation does not vary across the sample (i.e., all military base counties are the same across treatment years, and are all subject to the shock in 2017), so a standard TWFE DID estimator does not suffer from the methodological worries associated with heterogeneous treatment effects and treatment timing (Goodman-Bacon, 2021; De Chaisemartin & d'Haultfoeuille, 2020). However, there may still be concerns with pre-treatment trends and the underlying identification assumptions of difference-in-differences estimators, so we also test the robustness of

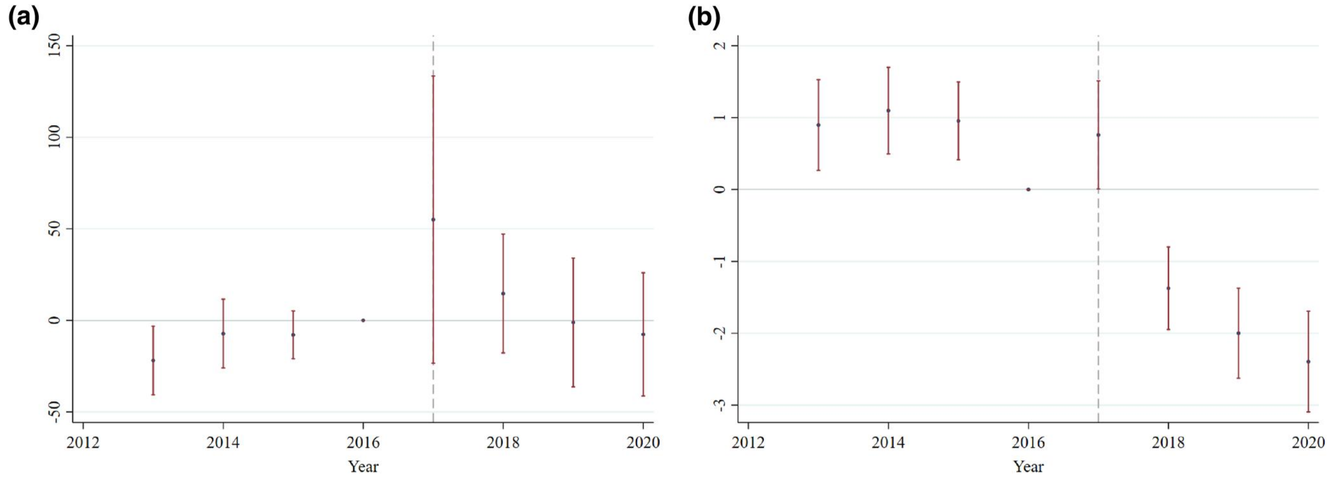

FIGURE 3 Impact of Military Lending Act (MLA) on Payday Lenders in Base Counties. (a) Employment. (b) Establishments. Figure shows specifications of Equation (2) with payday lender employment and number of establishments as the outcome variables. Data is from Bartik et al. (2017).

our results to the doubly robust difference-in-differences estimator (Callaway & Sant'Anna, 2021) and the synthetic difference-in-differences estimator (Arkhangelsky et al., 2021).[8]

We also estimate dynamic specifications, in which we measure the impact of military base county location a certain number of years after the year prior to treatment:

$$y_{ct} = \sum_{\tau} \beta_{\tau} 1(t = T + \tau; Base_c = 1) + \alpha_c + \sigma_t + \epsilon_{ct} \tag{2}$$

where variables are defined as in Equation (1). Consistent with the testimony of military leaders and journalists that payday lenders were *increasingly* targeting military bases (Section 2), the dynamic specifications provide statistically significant evidence of pre-treatment trends for some outcomes.

3.3.1 | The existence of pre-treatment trends

Pre-treatment trends in the availability of payday lenders in military base counties (relative to non-military base counties) is consistent with the testimony of military leaders and popular media reporting (described in Section 2), who argue that high-interest payday lenders were increasingly targeting military bases prior to the MLA. Additionally, given we also find evidence of pre-treatment trends in Figures 2 and 3, we test the robustness of our estimates to loosening the assumption of parallel trends in favor of the assumption of pre-treatment trends continuing in counterfactual post-treatment trends and test robustness to deviations from the pre-treatment trends following Rambachan and Roth (2023).

3.3.2 | Identification concerns and ex ante reasonableness

Ex ante, the timing of MLA Final Rule interacted with military base county seems to constitute quasi-random variation suitable for identifying causal effects. Candidates for a confounding labor market shock would have to share *both* the timing of MLA *and* target counties with a military base. The ex ante reasonableness of this identifying assumption follows the intuition that a confounding shock that cuts across the MLA Final Rule year and military counties is unlikely. Additionally, as we show in Appendix C, the results are generally robust to shifting the treatment a year earlier. These results also provide substantial evidence that anticipation effects (the MLA Final Rule was announced in July 2015 to begin enforcement in October 2016) are not a concern insofar as the conclusions of this article.

4 | MAIN RESULTS AND POTENTIAL MECHANISMS

4.1 | Impact of MLA on payday lenders

This section proceeds with the main estimates of the effect of the MLA Final Rule, which we show manifests as regional payday lending access shocks. Figure 3b provides evidence of a statistically significant effect of the MLA Final Rule on relative (base county to non-base counties) payday lender establishments. Figure 3a shows similar movement and base-county payday lender employment levels, though noisey and less pronounced.

4.2 | Regional economic outcomes

Figure 4a–c show the dynamic estimates from specifications of Equation (2) with different regional economic outcomes as the dependent variable. Each of the regional economic variables display some evidence of pre-trends prior to the MLA. There is some evidence of increases in average wage (panel 4a), income per capita (panel 4b), and decreases in

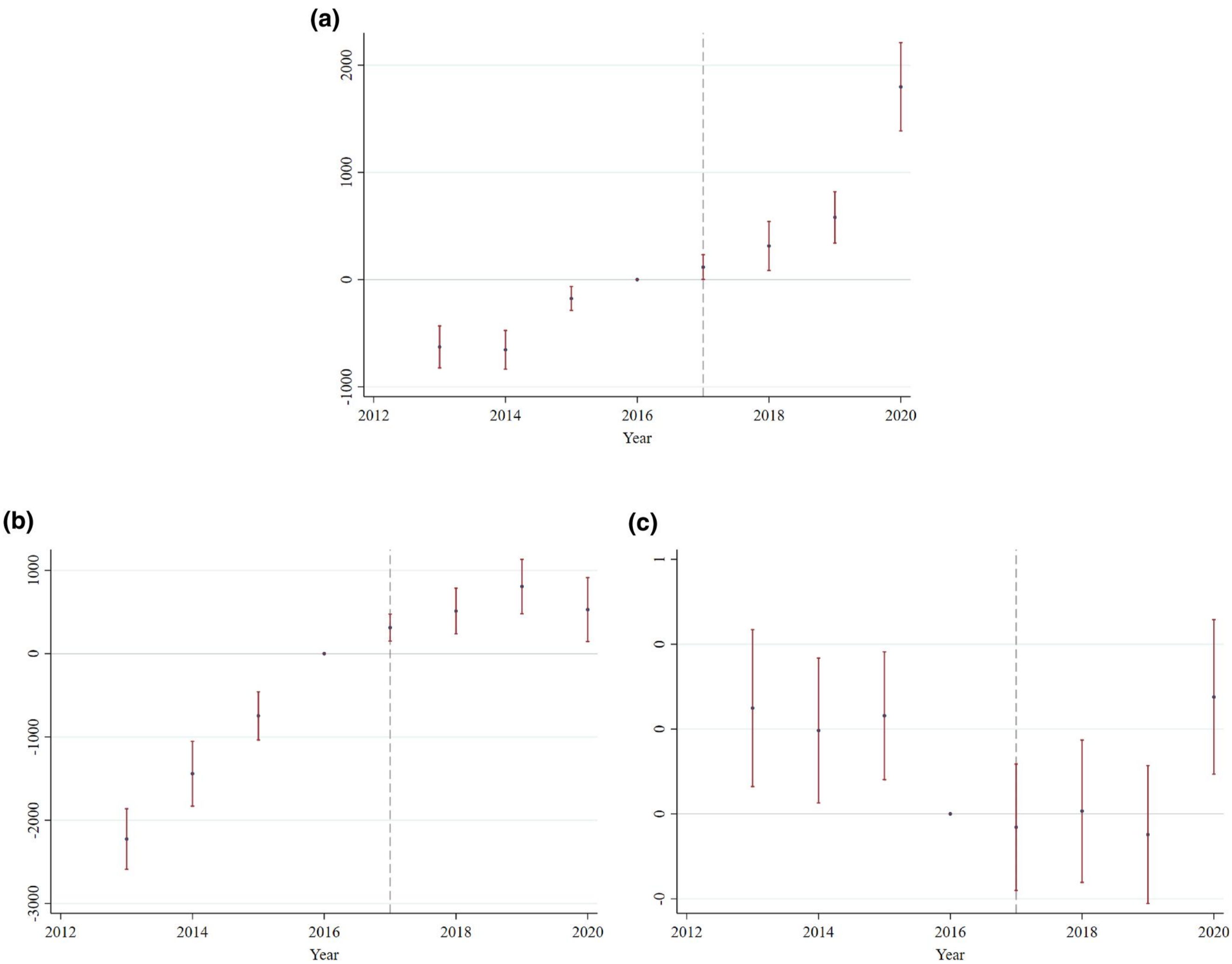

FIGURE 4 Regional Economic Outcome Plots. (a) Average wage. (b) Income per capita. (c) Poverty. Figure shows specifications of Equation (2) with regional economic outcomes as the dependent variables. Data is from U.S. Bureau of Labor Statistics and the U.S. Small Area Income and Poverty Estimates.

TABLE 2 Impact of MLA final rule: Regional economic outcomes.

	(1) Income per capita	(2) Income per capita	(3) Average wage	(4) Average wage	(5) Poverty rate	(6) Poverty rate
TWFE DID						
Base × Post 2017	1643.67***	1082.40***	1066.64***	583.09***	−0.12*	−0.09
	(205.71)	(186.36)	(140.56)	(119.08)	(0.07)	(0.07)
Linear trend difference						
Base × Post 2017	−19.26	−62.09	−467.51***	−507.48***	−0.15*	−0.17**
	(101.49)	(105.30)	(72.99)	(79.71)	(0.08)	(0.08)
County-year trends						
Base × Post 2017	−18.97	1.20	−467.38***	−465.01***	−0.15*	−0.16*
	(108.49)	(109.26)	(78.03)	(78.51)	(0.09)	(0.09)
Linear spline trends						
Base × Post2017	−340.75***	−388.13***	−320.07***	−361.67***	−0.07	−0.10
	(108.22)	(112.45)	(74.68)	(78.22)	(0.08)	(0.08)
Doubly robust DID						
ATT	539.71***	−852.81***	702.05***	−569.02*	0.05	0.01
	(136.36)	(272.28)	(114.64)	(294.48)	(0.07)	(0.07)
Synthetic DID						
ATT	528.04***	180.13	635.15***	365.53***	−0.02	0.02
	(180.19)	(135.68)	(101.99)	(119.01)	(0.05)	(0.07)
Controls		YES		YES		YES
Observations	24,903	24,711	24,903	24,711	25,127	24,707

Note: Each column and each italicized row subheading reports estimates from a separate regression. Specification is Equation (1), beginning with the standard TWFE Difference in Differences framework, and then extensions follow under italicized subheadings to additionally include the linear trend difference ($\theta Base_c \sigma_t$, see Bilinski and Hatfield (2019)) and county-time linear trends ($\theta \alpha_c \sigma_t$). "Doubly robust DID" refers to Callaway and Sant'Anna (2021) and "Synthetic DID" refers to Arkhangelsky et al. (2021). Even column numbers contain our set of control variables following Carter and Skimmyhorn (2017). Standard errors are clustered at county level and reported in parentheses.

Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

poverty (panel 4c) after the MLA Final Rule. However, all display statistically significant evidence of pretrends, so the significance of dynamic estimates relative to 2016, cannot be interpreted in the absence of accounting for these trends, which we discuss below.

Table 2 shows the standard TWFE DID estimates from specifications of Equation (1) with different regional economic outcomes, including average wage, per capita income, and poverty rate as the dependent variables. Subsequent rows under italicized subheadings test alternative specifications that include the linear trend difference ($\theta Base_c \sigma_t$ following Bilinski & Hatfield, 2019), county-time linear trends ($\theta \alpha_c \sigma_t$), linear spline trends, the doubly robust difference-in-differences estimator (Callaway & Sant'Anna, 2021), and the synthetic difference-in-differences estimator (Arkhangelsky et al., 2021). Given the evidence of pre-treatment trends, the standard TWFE estimator does not provide consistent estimates of effects. Although our results are strongly robust to Callaway (2021), our robustness checks that include trends, lose significance or reverse the sign of our estimates of effects on regional economic outcomes.

4.2.1 | Robustness to deviations from pre-treatment trends

Given the strong evidence of pre-treatment trends Figure 4 and the sensitivity to the inclusion of linear trends Table 2, we loosen the assumption of parallel trends in favor of the assumption of pre-treatment trends continuing in

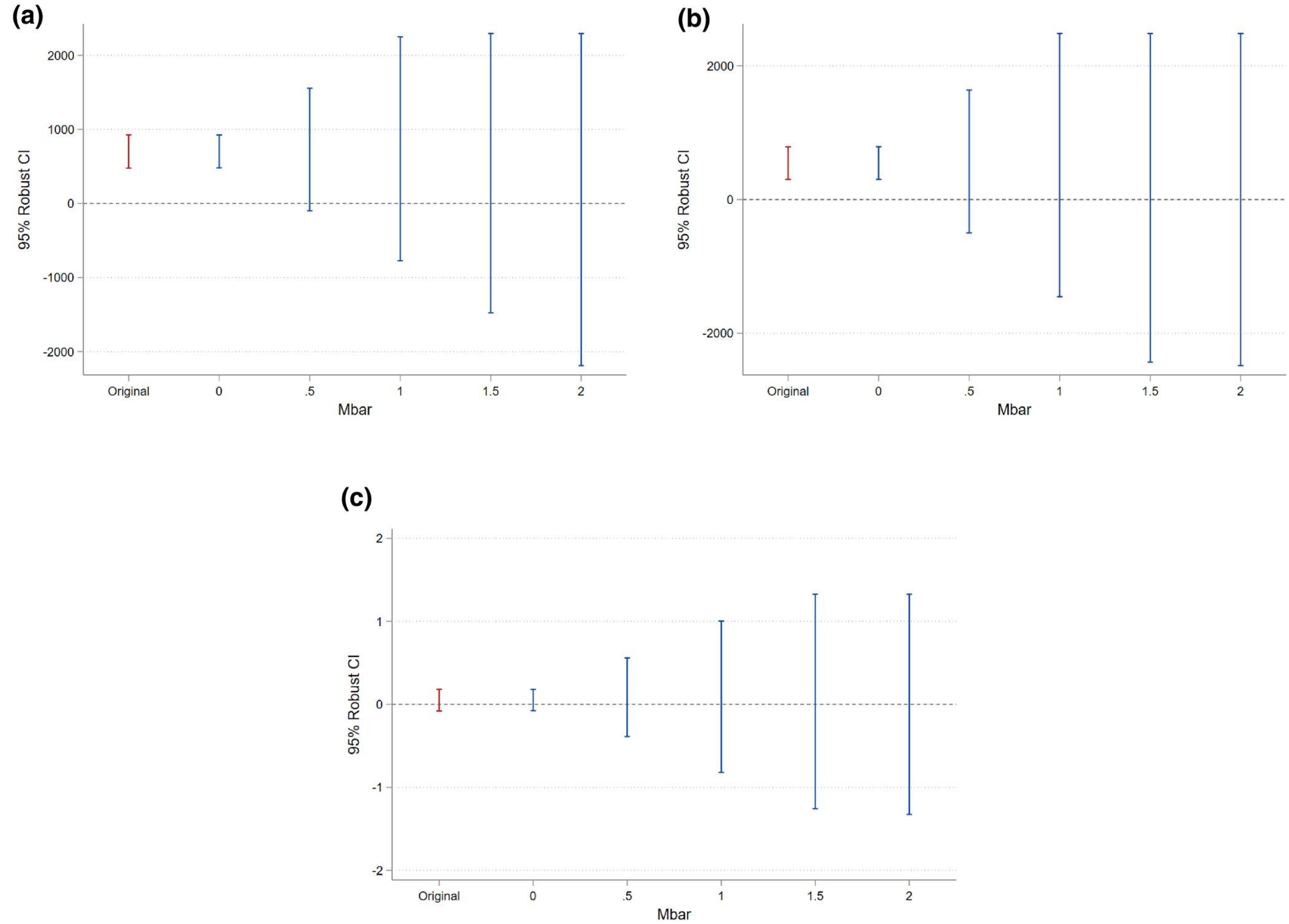

FIGURE 5 Regional Economic Outcomes: Robustness to Deviation from Pre-trends. (a) Average wage. (b) Income per capita. (c) Poverty. Figure shows the robustness of deviation from pre-treatment trends for out regional economic outcomes in our difference in differences estimates following Rambachan and Roth (2023). Mbar (x-axis) represents the hypothetical deviations from the pre-treatment trend slope in the counterfactual post-treatment. The y-axis provides the 95% confidence interval for treatment estimates under each Mbar. Data is from U.S. Bureau of Labor Statistics and the U.S. Small Area Income and Poverty Estimates.

counterfactual post-treatment trends. Following Rambachan and Roth (2023), Figure 5 shows the robustness to deviations from the pre-treatment trends for the final rule. We find a positive effect on average wage and incomes (about 1% of their means) and a statistically insignificant effects on poverty. However, Figure 5 also shows that none of these estimates are robust to deviations of varying sizes of $\overline{M}$, from the pre-treatment trends. Given this lack of robustness and the sensitivity to the inclusion of trends in our point estimates, we interpret these mixed results with caution and focus our conclusions on the more-robust forthcoming entrepreneurial outcomes.

4.3 | Potential mechanisms

Payday loans are an important source of capital for entrepreneurs (Herkenhoff et al., 2021), with more than 20% of self-employed individuals having borrowed from payday lenders or alternative financial services (Nitani et al., 2020). Thus, regional changes in payday lending access may have important implications for regional entrepreneurship. And given entrepreneurship is an important contributor to regional economic growth (Acs & Armington, 2004; Acs et al., 2009; Stephens & Partridge, 2011), effects on regional entrepreneurship represent a plausible mechanism for broader regional economic outcomes. We thus explore effects of the MLA Final Rule on employer and nonemployer outcomes as potential mechanisms for regional economic effects.

4.3.1 | Employer outcomes

Figure 6a–d show the dynamic estimates from specifications of Equation (2) with different employer outcomes, including total employment, total establishments, establishment entry rate, and establishment exit rate. We find evidence of a decrease establishment entry rate (panel 6c), and an increase the establishment exit rate (panel 6d) after the MLA Final Rule. We again see evidence of pre-treatment trends in employment (panel 6a) establishments (panel 6b), making these effects more difficult to interpret.

Table 3 shows the standard TWFE DID estimates from specifications of Equation (1) with different employer outcomes, including total employment, total establishments, establishment entry rate, and establishment exit rate. As in Section 4.2, subsequent rows under italicized subheadings test robustness to inclusion of a linear trend difference ($\theta Base_c \sigma_t$, county-year linear trends ($\theta \alpha_c \sigma_t$), linear spline trends, following Bilinski and Hatfield (2019), the doubly robust difference-in-differences estimator (Callaway & Sant'Anna, 2021), and the synthetic difference-in-differences estimator (Arkhangelsky et al., 2021). Although our results are generally robust to the doubly robust estimator, consistent with the evidence of pre-treatment trends in the number of establishments and employment, the effect of the Final Rule loses statistical significance when we control for linear trends.

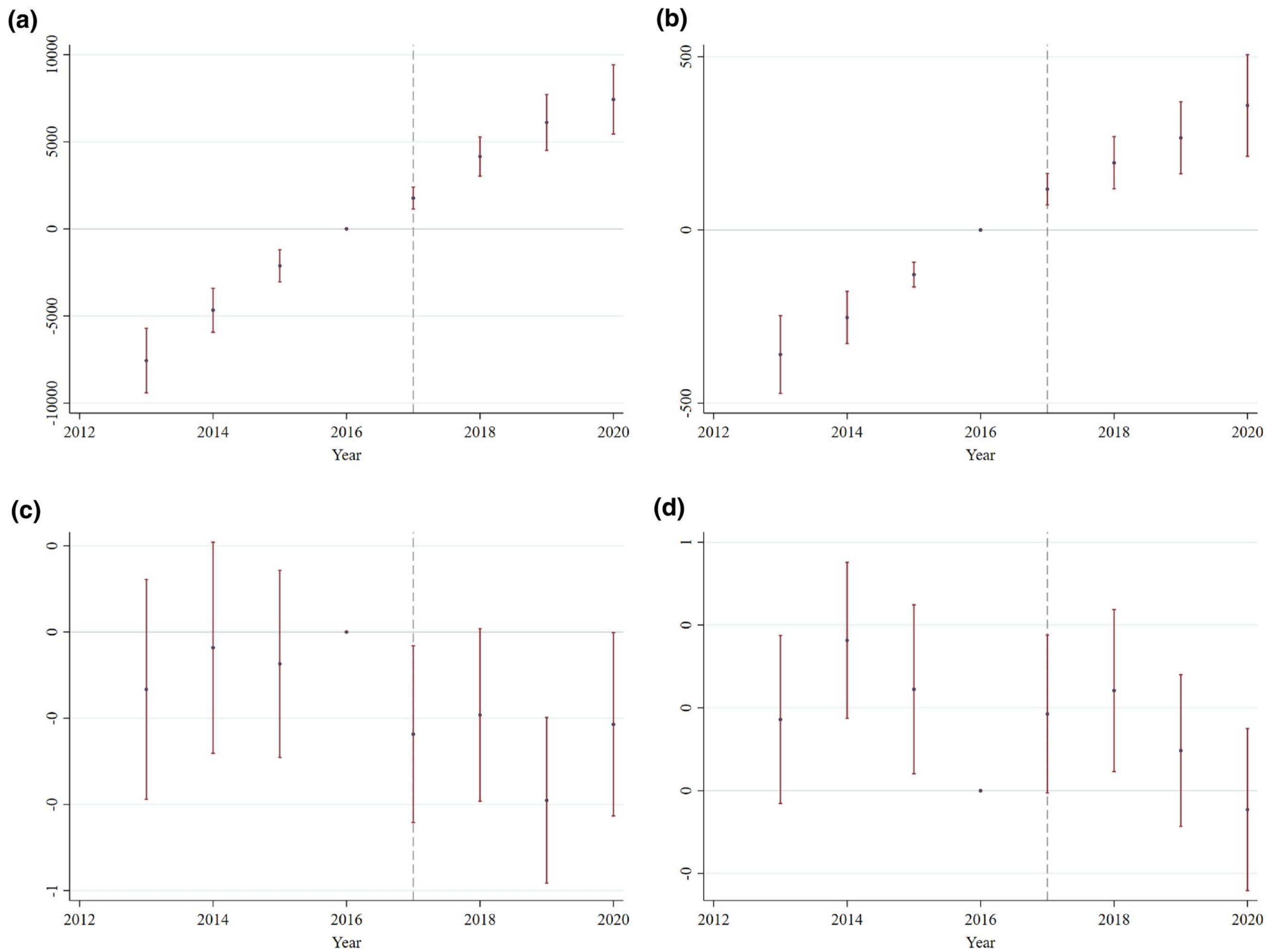

FIGURE 6 Employer Impacts. (a) Total employment. (b) Total establishments. (c) Establishment entry rate. (d) Establishment exit rate. Figure shows specifications of Equation (2) with employer outcomes as the dependent variable. Data is from Bartik et al. (2017) and the U.S. Census Bureau's Business Dynamics Statistics.

TABLE 3 Impact of MLA final rule: Employer outcomes.

	(1) Total emp	(2) Total emp	(3) Total estabs	(4) Total estabs	(5) Entry rate	(6) Entry rate	(7) Exit rate	(8) Exit rate
TWFE DID								
Base × post 2017	8451.55***	2943.86***	419.65***	162.43**	−0.20***	−0.21***	−0.08	−0.09*
	(1088.54)	(727.45)	(73.75)	(68.22)	(0.06)	(0.06)	(0.05)	(0.05)
Linear trend difference								
Base × post 2017	−380.39	−629.76	19.75	12.36	−0.24**	−0.24**	0.22**	0.22**
	(422.59)	(424.22)	(13.39)	(14.35)	(0.10)	(0.10)	(0.10)	(0.10)
County-year trends								
Base × post 2017	−378.90	−493.16	19.80	17.44	−0.24**	−0.24**	0.23**	0.23**
	(451.78)	(443.78)	(14.32)	(14.62)	(0.11)	(0.11)	(0.10)	(0.11)
Linear spline trends								
Base × post 2017	−694.06	−608.45	−0.59	10.68	−0.27**	−0.27**	0.21**	0.21**
	(434.27)	(391.30)	(14.50)	(15.17)	(0.11)	(0.11)	(0.10)	(0.10)
Doubly robust DID								
ATT	4866.87***	−3578.98*	234.25***	106.38	−0.28***	−0.15*	0.13*	0.03
	(662.23)	(2093.95)	(46.86)	(93.55)	(0.08)	(0.08)	(0.07)	(0.07)
Synthetic DID								
ATT	113.29	530.77	35.90	83.30	−0.14**	−0.16***	−0.03	−0.05
	(484.54)	(428.66)	(40.11)	(56.87)	(0.07)	(0.05)	(0.05)	(0.05)
Controls		YES		YES		YES		YES
Observations	25,128	24,696	25,128	24,696	24,633	24,218	24,656	24,240

Note: Each column and each italicized row subheading reports estimates from a separate regression. Specification is Equation (1), beginning with the standard TWFE Difference in Differences framework, and then extensions follow under italicized subheadings to additionally include the linear trend difference ($\theta Base_c \sigma_t$, see Bilinski & Hatfield, 2019) and county-time linear trends ($\theta \alpha_c \sigma_t$). "Doubly robust DID" refers to Callaway and Sant'Anna (2021) and "Synthetic DID" refers to Arkhangelsky et al. (2021). Column abbreviations include total employment, total establishments, establishment entry rate, and establishment deaths. Even column numbers contain our set of control variables following Carter and Skimmyhorn (2017). Standard errors are clustered at county level and reported in parentheses. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Given the evidence of pre-treatment trends in the dynamic estimates for employer establishments and employment, and the sensitivity of their point estimates to the inclusion of trends, we again loosen the assumption of parallel trends in favor of the assumption of pre-treatment trends continuing in counterfactual post-treatment trends. Following Rambachan and Roth (2023), Figure 7 shows the robustness to deviations of size 0.5 from the pre-treatment trends for the estimates. We find that the positive effect estimates on employer establishments and total employment, but again interpret these two estimates with caution, given the lack of robustness of our points estimates.

However, the point estimates for the effect of the Final Rule on establishment entry rate and exit rate (which do *not* show strong evidence of pre-treatment trends in Figure 6) are robust across all specifications. Specifically, we estimate an average reduction in the employer entry rate of about −0.2% points (about 2% of the mean entry rate), and an increase in the employer exit rate of about 0.2% points (about 2% of the mean exit rate).

4.3.2 | Non-employer outcomes

Figure 8a,b show the dynamic estimates from specifications of Equation (2) with nonemployer establishment counts and nonemployer receipts as the dependent variable and Table 4 shows the standard TWFE DID estimates from specifications of Equation (1) with nonemployer establishments and nonemployer receipts as the dependent variable.

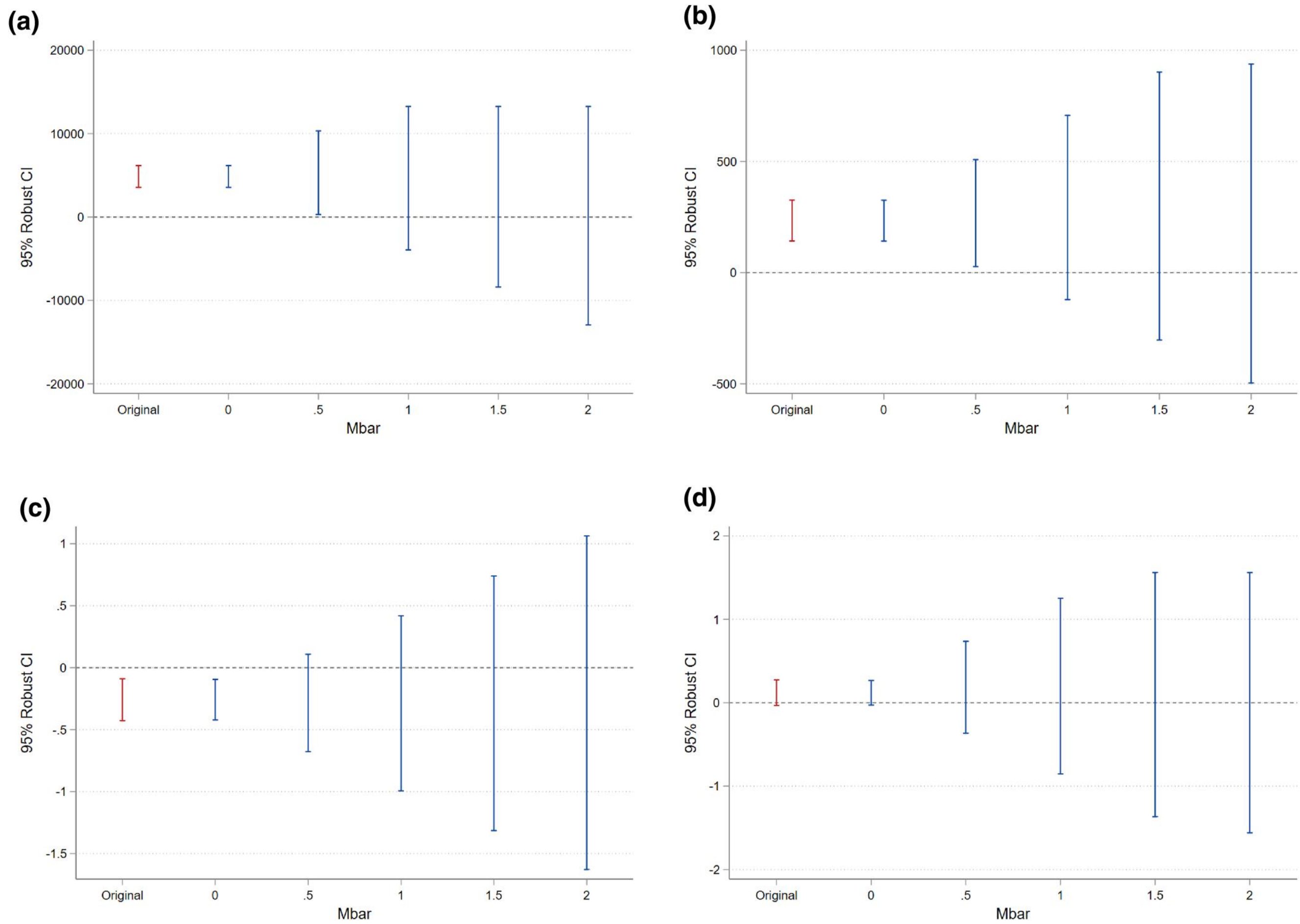

FIGURE 7 Employer Impacts: 2017 Robustness to Deviation from Pre-trends. (a) Total employment. (b) Total establishments. (c) Establishment entry rate. (d) Establishment exit rate. Figure shows the robustness of deviation from pre-treatment trends for employer outcomes in our difference in differences estimates following Rambachan and Roth (2023). Mbar (x-axis) represents the hypothetical deviations from the pre-treatment trend slope in the counterfactual post-treatment. The y-axis provides the 95% confidence interval for treatment estimates under each Mbar. Data is from Bartik et al. (2017) and the U.S. Census Bureau's Business Dynamics Statistics.

However, given both nonemployer outcomes display statistically significant evidence of pre-trends prior to the MLA Final Rule (Figure 8), TWFE DID will not provide consistent estimates. So, as in Section 4.2, subsequent rows under italicized subheadings test robustness to inclusion of a linear trend difference ($\theta Base_c \sigma_t$, county-year linear trends ($\theta \alpha_c \sigma_t$), linear spline trends, following Bilinski and Hatfield (2019)), the doubly robust difference-in-differences estimator (Callaway & Sant'Anna, 2021), and the synthetic difference-in-differences estimator (Arkhangelsky et al., 2021).

While the magnitude of these point estimates varies substantially across specifications, the positive and statistical significance is consistent. But again, given the evidence of pre-treatment trends, we loosen the assumption of no pre-trends and test robustness to deviations from the pre-trend slope in the counterfactual post-treatment in Figure 9 following (Rambachan & Roth, 2023). These estimates are large (about 10% of the mean), but have large confidence intervals that include the smaller point estimates (about 4% of the mean) found when controlling for trends. Figure 9 further shows some robustness of this positive effect toward deviations from the pre-treatment trend.

5 | CONCLUSIONS

There are ongoing policy considerations of extending the ban on usury lending beyond military members and their families. Existing research on the effects of banning payday loans focuses on individual credit outcomes and has mixed findings (Bolen et al., 2020; Ramirez, 2020), while research on direct effects on military members of banning payday

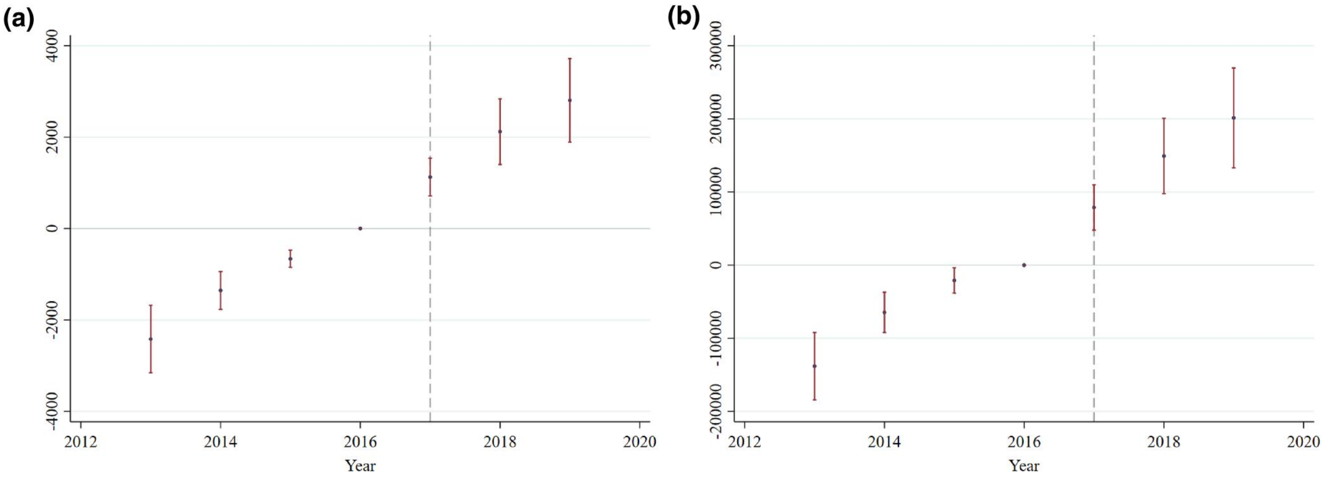

FIGURE 8 Nonemployer Impacts. (a) Nonemployer establishments. (b) Nonemployer receipts. Figure shows specifications of Equation (2) with nonemployer outcomes as the dependent variable. Data is from U.S. Census Bureau's Nonemployer Statistics.

TABLE 4 Impact of MLA final rule: Nonemployer outcomes.

	(1) Nonemployer estabs	(2) Nonemployer estabs	(3) Nonemployer receipts	(4) Nonemployer receipts
TWFE DID				
Base × post 2017	3125.31***	1311.08***	199,055.72***	99,234.41***
	(491.27)	(318.88)	(31,958.05)	(25,032.30)
Linear trend difference				
Base × post 2017	298.51***	432.96***	23,189.59**	28,273.99**
	(113.42)	(152.94)	(9459.90)	(11,424.56)
County-year trends				
Base × post 2017	298.85**	321.79***	23,211.78**	18,496.82***
	(122.51)	(99.80)	(10,217.91)	(6878.31)
Linear spline trends				
Base × post 2017	298.51***	433.29***	23,189.07**	28,303.02**
	(113.42)	(153.11)	(9460.15)	(11,437.79)
Doubly robust DID				
ATT	2016.53***	394.57	143,073.13***	−4620.02
	(346.10)	(327.48)	(25,536.16)	(21,325.40)
Synthetic DID				
ATT	93.75	448.38**	19,749.95*	30,542.72**
	(210.04)	(208.58)	(11,249.77)	(13,377.01)
Controls		YES		YES
Observations	21,991	21,615	21,991	21,615

Note: Each column and each italicized row subheading reports estimates from a separate regression. Specification is Equation (1), beginning with the standard TWFE Difference in Differences framework, and then extensions follow under italicized subheadings to additionally include the linear trend difference ($\theta Base_c \sigma_t$, see Bilinski & Hatfield, 2019) and county-time linear trends ($\theta \alpha_c \sigma_t$). "Doubly robust DID" refers to Callaway and Sant'Anna (2021) and "Synthetic DID" refers to Arkhangelsky et al. (2021). Even column numbers contain our set of control variables following Carter and Skimmyhorn (2017). Standard errors are clustered at county level and reported in parentheses. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

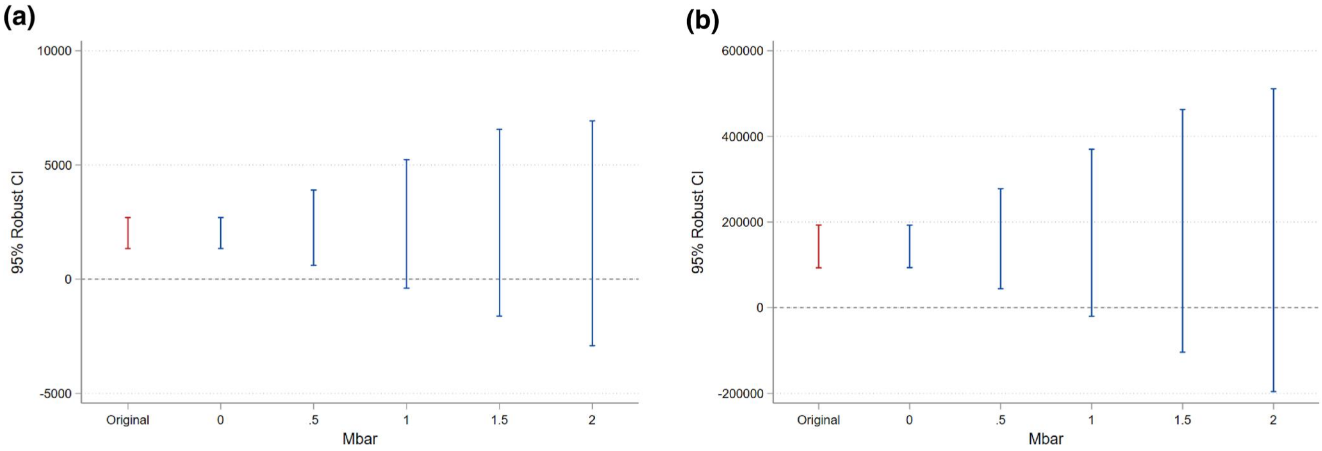

FIGURE 9 Nonemployer Impacts: 2017 Robustness to Deviation from Pre-trends. (a) Nonemployer establishments. (b) Nonemployer receipts. Figure shows the robustness of deviation from pre-treatment trends for nonemployer outcomes in our difference in differences estimates following Rambachan and Roth (2023). Mbar (x-axis) represents the hypothetical deviations from the pre-treatment trend slope in the counterfactual post-treatment. The y-axis provides the 95% confidence interval for treatment estimates under each Mbar. Data is from the U.S. Census Bureau's Nonemployer Statistics.

lending are positive (Carrell & Zinman, 2014) or null (Carter & Skimmyhorn, 2017). This article is the first (to the authors knowledge) to examine the broader regional economic effects of bans and the first to examine the effects of the MLA Final Rule.

Given payday lenders congregate in neighborhoods with higher rates of poverty, lower education, and minority populations (Barth et al., 2015)—analogously to how they congregated around military bases—this article provides important evidence on the effects of reducing these types of congregations on local economies. Specifically, this article provides mixed evidence that bans on usury lending do not negatively effect the broader regional economy, at least in the short run. However, this article also finds robust evidence of reduced employer entry and exit ("churn") by about 2% or 1 standard deviation of the mean. Research indicates that churn provides important information and thereby increases growth and entrepreneurship in the long-run (Bunten et al., 2015). Reduced churn may thereby indicate the potential for negative effects of usury bans in the long-run. On the other hand, the negative effect on churn (and increased nonemployer outcomes) may occur from a short-run increase in financing search costs, while, in the long-run, increased use of lower-cost forms of credit might increase well-being on average (Dobridge, 2016; Galperin & Mauricio, 2015).

Once more time has passed, future research could examine longer-term effects, which may differ, once once individuals have had more time to find alternative financing sources. Additionally, to the authors' knowledge (and prior to this article), while there is research on the original 2006 MLA, there is no research on the effects of the MLA Final Rule across any outcome. So, examining more individual-level effects of the MLA Final Rule is sensible. The confounding timing of the Great Recession would make this article's identification strategy difficult in the context of the original MLA, as discussed in Appendix A. Additionally, future work on the effects of the MLA and the Final Rule could examine the potential for heterogeneous effects among military bases. Appendix B provides some evidence of the potential for heterogenous effects across base size using a difference-in-difference-in-differences estimator. More generally, with access to microdata, researchers could further examine the potential mechanisms proposed herein. For example, researchers could examine entrepreneur-level data to test the potential for heterogeneous racial groups or genders for entrepreneurial outcomes concentrated near military bases. Further, researchers could test for more specific mechanistic effects on funding source, funding amount, and associated hiring and output decisions.

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DATA AVAILABILITY STATEMENT

The data that support the findings of this study are openly available from sources including the U.S. Census Bureau's County Business Patterns using Bartik et al. (2017), U.S. Bureau of Labor Statistics, U.S. Small Area Income and Poverty Estimates, U.S. Nonemployer Statistics, and the U.S. American Community Survey, Decennial Census, and the Fiscal Year 2019 Base Structure Report from the Department of Homeland Security Infrastructure Foundation-Level Data System.

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ENDNOTES

[1] Extended pre-trends in payday lender concentration that includes years not included in the analyses herein are available in Figure A2.

[2] "Payday Lending Services" are categorized under North American Industrial Classification System (NAICS) code 522390, so we use NAICS 522390 in Bartik et al. (2017) to measure payday lending employment and establishment counts. Carpenter et al. (2022a) show Bartik et al. (2017) minimizes measurement error relative to other publicly available options.

[3] Boundaries were defined in the Fiscal Year 2019 Base Structure Report and are not all-encompassing of subordinate sites that are confidential in nature. The original feature layer was retained by the Department of Homeland Security Infrastructure Foundation-Level Data System. We had overlaid it on the 2019 Census TIGER/Line Shapefile of Counties (and equivalent) to generate a list of counties within the United States that contain a Department of Defense (DoD) site. Counties were considered to contain a DoD site if any part of the authoritative boundary overlaid the county polygon.

[4] It may be that payday lenders affect larger regional economies (of individuals commuting to counties with a military base), so commuting zones may represent more accurate economic geographies than counties existing along arbitrary political boundaries (Carpenter et al., 2022b; Tolbert & Sizer, 1996). This relative intensity of payday lenders and our results more generally also hold for commuting zones with a military base (Appendix D).

[5] Another potential explanation for the increase in slope is that Payday Lenders may have hoped that the Military Lending Act would be weakened or abolished; some reporting indicates efforts to weaken the MLA were taking place around the time of the Final Rule (Arnold, 2018; Thrush, 2018). The prospective of the end to the MLA may have increased payday lender employment prior to the Final Rule.

[6] Employment levels were generally trending upwards 2013–2016, with the trend increasing 2016–2017, so pre-treatment trends may be a concern. However, as discussed further in Sections 3.3.1 and 4.2.1, the Rambachan and Roth (2023) estimator still provides consistent estimates to the extent to which those pre-trends would have continued into the counterfactual post period. The detrended Figure 2c highlights the increase in slope 2016–2017, while trend Figure 2a shows this increase in slope in the context of the already-positive slope.

[7] Control variables included in the robustness checks are county population, share of the population over 25 with a bachelor's degree, and the median two-bedroom rent.

[8] Difference-in-differences estimators essentially treat all military base communities as the same. However, there is heterogeneity in the size of military bases relative to the size of their respective county, with some bases being more significant drivers of the economy. We thus also compare small and large military bases using a triple difference-in-differences estimator in Appendix B.

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Additional supporting information can be found online in the Supporting Information section at the end of this article.

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