

Debunking Lien Myths: Empirical Evidence for an Essential Tool in the Fight against Wage Theft

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Article abstract

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We use a difference in differences regression analysis to test this argument, using data from the US Small Business Administration 7(a) loan program. We look to two multistate metropolitan areas in the United States with divided wage lien regimes – Chicago and Washington DC. In each case, wage lien laws are permitted in one state (Wisconsin and Maryland, respectively) and not allowed in others (Illinois and Indiana for Chicago, Virginia for Washington).

We test the argument against wage liens through regression analysis, looking at gross approvals and interest rates as a reflection of risk. We also aggregate observations by 4-digit NAICS code, by lending institution, and by ZIP postal code and reran regression analyses. The wage lien law treatment did not lower the gross approval amount of 7(a) loans in either market, and did not increase the interest rate faced by borrowers. Indeed, results, while mostly statistically insignificant, in some cases showed the opposite effect. Although these tests do not definitively prove that wage lien laws do not constrict credit flows to small businesses, they provide absolutely no evidence to support such an argument either. The findings thus support the position of advocates supporting wage lien laws as a common-sense tool in the arsenal against wage theft, while casting substantial doubt on oppositional claims against wage liens.

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This article evaluates the impact on business access to credit of wage lien laws, which are an important tool for low-wage workers to reduce wage theft. Worker advocates have tried to expand access to wage liens. Opponents argue that wage liens negatively impact business access to credit. Using data from the Small Business Administration 7(a) loan program, one of the only detailed sources of business loan data, we assess whether lien laws affect business access to credit in two states—Wisconsin and Maryland—using a difference-in-differences framework. We find no evidence to support wage lien opponents' claims that lien laws reduce credit access. The findings contribute to the understanding of wage liens and provide evidence in support of policies that protect workers.

Debunking Lien Myths: Empirical Evidence for an Essential Tool in the Fight against Wage Theft

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Summary

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We use a difference in differences regression analysis to test this argument, using data from the US Small Business Administration 7(a) loan program. We look to two multistate metropolitan areas in the United States with divided wage lien regimes – Chicago and Washington DC. In each case, wage lien laws are permitted in one state (Wisconsin and Maryland, respectively) and not allowed in others (Illinois and Indiana for Chicago, Virginia for Washington).

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states—Wisconsin and Maryland—using a difference-in-differences framework. We find no evidence to support wage lien opponents' claims that lien laws reduce credit access. The findings contribute to the understanding of wage liens and provide evidence in support of policies that protect workers.

Keywords: wage liens; wage theft; small business loans; difference-in-differences analysis

Résumé

Les lois sur les privilèges salariaux (*Wage lien law*) ont un immense potentiel pour aider les travailleurs à recouvrer les salaires dus. Parce que les privilèges peuvent garantir les droits de propriété avant la répartition des sommes, les travailleurs escrocs ne pouvant pas facilement cacher les actifs. Malgré l'utilité avérée des lois sur les privilèges salariaux, les opposants soutiennent fréquemment que ces régimes de privilèges restreindraient le crédit disponible.

Nous utilisons une analyse de régression pour vérifier cet argument, en utilisant les données du programme de prêt 7(a) de la US Small Business Administration. Nous regardons deux zones métropolitaines multi-états aux États-Unis avec des régimes de privilège salarial divisés - Chicago et Washington DC. Dans chaque cas, les lois sur les privilèges salariaux sont autorisées dans un état (Wisconsin et Maryland, respectivement) et n'existe pas dans les autres (Illinois et Indiana pour Chicago, Virginie pour Washington). Nous testons l'argument contre les liens salariaux en examinant les approbations brutes et les taux d'intérêt comme un reflet du risque. Nous agrégeons également les observations par code SCIAN à 4 chiffres, par banque, et par code postal ZIP et refaisons les analyses de régression.

Le traitement prévu par la loi sur les privilèges salariaux n'a pas réduit le montant brut d'approbation des prêts 7 (a) sur l'un ou l'autre marché et n'a pas augmenté le taux d'intérêt auquel sont confrontés les emprunteurs. En effet, les résultats, bien que pour la plupart statistiquement insignifiants, ont dans certains cas montré l'effet inverse. Bien que ces tests ne prouvent pas définitivement que les lois sur les privilèges salariaux ne restreignent pas le crédit, ils ne fournissent absolument aucune preuve pour étayer un tel argument non plus. Les résultats soutiennent ainsi la position des défenseurs soutenant les lois sur les privilèges salariaux en tant qu'outil de bon sens dans l'arsenal contre le vol de salaire, tout en jetant un doute important sur les réclamations de l'opposition contre les liens salariaux.

Précis

Cet article évalue l'impact des lois sur les privilèges salariaux sur l'accès des entreprises au crédit. Constituant un outil important pour les travailleurs à bas salaire afin de réduire le vol de salaire, les opposants affirment plutôt que les privilèges salariaux ont un impact négatif sur l'accès des entreprises au crédit. En utilisant les données du programme de prêt 7(a) de la Small Business Administration, l'une des seules sources détaillées de données sur les prêts aux entreprises, nous évaluons si les lois sur les privilèges affectent l'accès des entreprises au crédit dans deux États - le Wisconsin et le Maryland - en utilisant un cadre de différence dans les différences. Nous ne trouvons aucune preuve pour soutenir les affirmations des opposants au privilège salarial selon lesquelles ces lois réduisent l'accès au crédit. Les résultats contribuent à la compréhension des privilèges salariaux et fournissent des preuves à l'appui des politiques qui protègent les travailleurs.

Mots-clefs: privilège salariaux; vol de salaire; prêts aux entreprises; analyse des différences

Introduction

In January of 2020, New York Governor Andrew Cuomo vetoed S2844B, the Securing Wages Earned Against Theft (SWEAT) bill. It was a setback for a broad coalition of advocates and unions that had hoped to expand workers' options for collecting owed wages when their employers fail to pay. The legal tool called a "wage lien" was perhaps the most important one in the bill. For workers in many industries, a major issue is underpayment of wages, often called "wage theft" by worker advocates, progressive policymakers, and academics (Bernhardt et al. 2009). A wage lien provides victims with an avenue to collect unpaid wages by giving them a legal right to a portion of the value of a piece of real property. The owner thus cannot sell the property without first paying the debt associated with the lien. Wage lien laws, like the one proposed in New York, help ensure that workers can collect money they are owed (Hallett, 2018).

Such laws face strong opposition from employers. The New York bill was just the latest failed effort to expand lien rights, being the fourth in New York in a decade (NYSS, 2020). In California, activists have also tried unsuccessfully to secure wage lien rights, with two bills dying in legislature (CALI, 2013-14a and 2013-14b). In North America, only two US states currently allow workers to file wage liens without restriction: Maryland and Wisconsin.

Opponents of wage lien laws justify their opposition on three grounds. First, they make a legal argument that wage liens circumvent due process rights for employers. Second, they make a logistical argument that a wage lien system will encourage fraudulent claims, thus tying up courts in lengthy battles. Third, they argue that liens constrict access to credit by complicating repayment priority. New York Assemblymember Andy Goodell, for example, argued in a hearing over S2844B that if "[wage] liens automatically take priority over any secured loans like lines of credit, the company... won't be able to get a line of credit because the bank or lending institution is not assured that [the] money that they're advancing this company will be secured in the future" (NYSS, 2019: 149). In California, an opposition letter to AB1164—penned by the California Chamber of Commerce and co-signed by 49 other groups—argued that wage liens would "basically destroy commercial investments or lending in California... It is impossible to imagine that a financial lender would provide a mortgage on real property if its interest in that property could be surpassed at any time by a wage lien" (CA CoC, 2013). Yet, though often made, to our knowledge no empirical research has assessed the validity of this claim.

In this study, we ask: does the presence of a wage lien law constrict credit access for small businesses? Our study will contribute to debate over wage lien laws by examining their effect on access to credit in two US markets. In doing so, we will advance ongoing discussions in industrial relations over the role of public policy in employment relations. With the decline of organized labour, public policy has taken on an increasingly important role in shaping the relationship between workers and employers (Fine and Gordon, 2010; Galvin, 2021). Yet enforcement of employment standards remains a vexing problem in the face of violations (Hallett 2018). Wage theft is, in fact, one of several widespread violations. To fight them, wage liens offer an effective mechanism in the absence of consistent government oversight.

Using data from the US Small Business Administration (SBA) 7(a) loan program, we will assess the effect of wage lien laws on loan activity. We will focus on two urban markets bisected by state lines prior to and following the establishment of a system of wage liens in one state but not the other. One of these urban markets is the Washington, D.C. metropolitan area. It extends into Maryland, which has a wage lien law, and into Virginia, which does not. The other urban market is the Chicago metropolitan area. It includes parts of Wisconsin, which allows such liens, and Illinois and Indiana, which do not. Informed by the difference-in-differences methodology used by Card and

Krueger (1994) in their landmark study of minimum wage increases and unemployment, we will use regression analysis to assess the difference-in-differences between states with and without wage lien laws, before and after the laws were adopted. We will examine the effect of such laws on total loan activity and initial interest rates over multiple timeframes and by lender, industry, and location.

We find no compelling evidence that wage lien laws are associated with reduced access to credit in either of the two markets. This finding casts doubts on the financial argument made by opponents of wage lien policies, while providing valuable information to worker advocates and policymakers interested in arguments for wage liens as a common-sense tool for workers to collect owed wages. This finding also contributes to research on pathways for workers to voice concern over, and seek redress for, employment violations, especially when those pathways do not rely on state agencies for enforcement.

We will begin with a review of the literature on wage theft, wage liens, and credit markets. We will then proceed with a discussion of our dataset and methods before presenting the results of our analyses. We will close with a discussion of implications for debates over wage liens.

Wage Theft, Wage Liens, and Business Access to Credit

2.1 Employment Standards Enforcement

A core tenet of industrial relations is that employment standards are central to labour markets. Through public policy, the state can impose higher standards for employment conditions, including wages, scheduling, and benefits, thus forcing employers to improve these conditions (Hallett 2018). Such policy interventions are particularly important because traditional forms of worker representation are losing effectiveness in a “new world of work” (Brunelle, Hayden and Murray, 2011) marked by decentralized employment relationships (Weil 2014). Worker organizations have increasingly looked to public policy to bolster workplace rights, rather than pursuing collective bargaining (Fine and Gordon, 2010; Galvin, 2021). Unlike private governance models, which rely on employer good will (Underhill and Rimmer, 2017), employment policies can enable worker activists to exert power over a region or industry rather than an individual employer (Fine 2011).

Despite the benefits of public policy as a tool for improving employment standards, industrial relations research has shown that violations are widespread (Bernhardt et al., 2009). Scholars have proposed three main explanations: misaligned employer incentives, labour market arrangements, and enforcement strategies. Ashenfelter and Smith (1979), taking an incentive approach, argued that rational profit-maximizing employers balance the probability of getting caught and likely paying a penalty against the potential savings from the violation. In many cases, the rational choice will be to violate the law. Other scholars argue that under-regulated low-wage labour markets facilitate violations. In the context of workplace fissuring—dilution of employer responsibility by means of subcontracting or worker misclassification—workers often do not know who to pursue for claims (Weil 2014). Finally, lack of enforcement exacerbates these problems. State enforcement agencies are chronically understaffed (Hallett 2018) and thus have low inspection rates. As a result, agencies often adopt fire-alarm enforcement regimes that rely on complaints (McCubbins and Schwartz 1984). But complaint-based systems disadvantage workers who lack collective voice or are less likely to complain to a government agency, for instance because of immigration status (Weil and Pyles, 2006). Moreover, enforcement regimes frequently emphasize compliance programs (i.e., encouraging employers to become compliant) over penalties (i.e., penalizing past non-compliance). If caught, the employer typically has to pay the missing wages. The cost is no

more than if the employer had complied with the law, and even complete payment is rare in settlement agreements (Fritz-Mauer, 2016). By eschewing fines, enforcement agencies increase the economic incentive to violate the law (Vosko et al., 2017). Together, these labour market, enforcement, and incentive factors limit the effectiveness of policy-driven employment standards and leave responsibility for enforcement to workers.

2.2 Wage Theft

Illegal underpayment, or “wage theft,” can include not only failure to pay minimum wage and overtime but also other statutory violations, such as failure to provide meal breaks. Activist and faith leader Kim Bobo, who popularized the term, defined wage theft as “when an employer violates the law and deprives a worker of legally mandated wages” (Bobo, 2009: 7). We will use Bobo’s definition, which encompasses any instance in which a worker receives less than what is owed in compensation for work the worker has already performed.

Wage theft is widespread and especially affects low-wage workers. A landmark survey of over 4,000 low-wage workers in Chicago, Los Angeles, and New York found that over one-quarter of respondents had received less than the minimum wage in the prior week (Bernhardt et al., 2009). A more recent survey of the ten most populous states has found that minimum wage violations alone affect 2.4 million workers and amount to \$8 billion in stolen wages annually (Cooper and Kroeger, 2017). A nationwide study in 2012 found that wage theft amounted to more than double the combined value of most types of robberies nationwide—this estimate included only wage theft identified by the US Department of Labor and excluded wage theft actions by state agencies or pursued privately (Meixell and Eisenbrey, 2014). In addition to affecting individual workers, pervasive aggregate wage theft has societal impacts by increasing poverty, by reducing tax revenues (ERG, 2014), and by increasing dependence on government assistance (Jacobs, Perry and MacGillivray, 2015). The issue is not unique to the United States. A diverse literature has documented wage theft, among other violations of employment standards, in countries around the world (Bhorat, Kanbur, and Stanwix, 2019; Buckley, 2020; Farbenblum and Berg, 2020; Judge and Stansbury, 2020; Ronconi, 2010).

Wage law enforcement is largely left to individual workers, many of whom are ill-equipped to pursue complaints. The effort of seeking restitution is a long and costly battle that requires individual workers to document how they were underpaid, find legal counsel, and pierce the veil of the corporation that employed them (Wage Justice Center, 2018). This process can take years and requires financial resources and perseverance (Buckley, 2020). The difficulties are compounded by factors that further marginalize workers, such as lack of work authorization (Gleeson 2015). Workers are therefore very unlikely to make a wage complaint (Hallett 2018).

Moreover, victory for a wage claimant is only the start of the battle. There is still the challenge of obtaining payment through a difficult collections process. The California Bureau of Field Enforcement, for example, collects a small fraction of what they assess employers each year in owed wages (Cho, Koonse, and Mischel, 2013). Employers found guilty of a wage and hour infraction have multiple options for avoiding payment, including bankruptcy, hiding assets, or a paper transfer of business assets to a new corporate shell (Adler, Genduso, and Yee, 2019; Shen, Kao, and Pastore, 2013). To ensure that workers can collect wages at the conclusion of a successful wage claim, worker advocates have increasingly pushed for a way to secure assets *during* the legal proceedings by encumbering employer property.

2.3 – Wage Liens

A lien is a state-registered legal claim against a piece of real property. It grants a creditor, called a lienholder, a partial right over the property until a debt is paid. A lien does not grant a lienholder possession of the property itself; rather a lienholder is entitled to some portion of the value of the property if the owner attempts to sell it before the debt has been paid (Black's Law Dictionary, 1999: 933). Many states allow post-judgment liens as a tool for collections, where the claimant is granted a lien *after* the legal system has validated the claim.¹ However, the lengthy wage claim process provides the defendant with an opportunity to dispose of real property. If a firm has no real assets left by the time the plaintiff is awarded a judgment, the plaintiff can no longer place a lien. Indeed, employers in some places have perfected the art of concealing property through transactions between multiple corporate entities during the wage claim process to ensure that no assets remain by the time an aggrieved worker prevails (Fuentes 2019).

Worker advocates thus argue that *pre-judgment* wage liens are a common-sense solution (NELP, 2011; Cho, Koonse, and Mischel, 2013; Hollander, 2015; Reed, Saylor and Spitzer, 2015; Colodny et al., 2015). A pre-judgment lien² prevents an employer from hiding real assets during the wage claim process, thus ensuring that a worker's claim can be fulfilled should the worker prevail in court.

There is a precedent in the US construction industry for pre-judgment liens, known as "construction liens" or "mechanics' liens." In the late 1700s, when planning to build Washington, D.C., Maryland legislators were worried about attracting qualified contractors. The construction lien was thus created to reassure wary contractors that they would be paid (Nelson 2007). This legal instrument was so successful that it became widespread across the US: every state has some version of a construction lien law. Although rules vary by jurisdiction,³ in general most people and firms working on a construction site can place a lien on the final property if they believe they have not been fully compensated for their work. The property owner then has the burden of either paying the lien-holding party or determining who owes the lienholder and intervening before the property can be sold (Kelley, 2013: 187-188).

Construction liens are successful because they change the incentives facing employers. Ashenfelter and Smith (1979) argued that employers decide whether to comply with the law based on the likelihood of getting caught and the magnitude of the fines that would result. Overlooked in their model, however, is the reality that employers have options for avoiding fines or restitution, and those options further increase the incentive to underpay. A wage lien mitigates these incentives by barring the employer from alienating real property while a lien is in place.

Proposed wage lien regimes would extend the construction lien provisions to workers beyond the construction industry (Hollander, 2015). Although 10 states have some version of a wage lien law (Hallett, 2018),⁴ only two have provided workers with broad rights to file a wage lien. Wisconsin updated its wage lien law in 1993 to encompass all workers (Cho, Koonse and Mischel, 2013), and Maryland did so in 2013 (Ertman and Weil, 2018).⁵ The Wisconsin regime has been highly effective at encouraging settlement of wage disputes: the state has a very high rate of wage collections, even though wage liens are rarely used (Cho, Koonse and Mischel, 2013). The Maryland regime is relatively new, and no study has yet tested its effectiveness.

2.4 – Hypothesized Effects of Wage Lien Laws on Business Access to Credit

Opponents of wage lien laws frequently argue that lien rights constrict credit markets and thus curtail economic activity. Yet, to date, they have not provided any empirical evidence to support this claim. In this section, we will put forward hypotheses that represent the views of wage lien opponents on how lien laws affect access to credit.

Liens have implications for creditors, just as they do for borrowers. Liens are equivalent to secured loans, which can take priority over unsecured loans in a bankruptcy under US law (Ayer, Bernstein and Friedland, 2004 but cf. Duca 1998). Even without a bankruptcy, liens may take precedence over other secured claims, such as mortgages, depending on local lien laws, a legal reality that may deter lenders (Kelley, 2013). Moreover, the presence of a lien might affect a small business's credit score, since debt is a key factor in determining the creditworthiness of small businesses (Mester, 1997). Opponents argue that lien laws might cause lenders to be less willing to provide loans or cause them to offer loans on less desirable terms because a lender's claim could potentially come into conflict with a future lien. Wage liens may therefore decrease the overall amount of credit available to small businesses. If wage lien opponents are correct, we should find evidence for the following hypothesis:

Hypothesis 1: The presence of laws permitting wage liens will reduce the gross amount of approved loans for small businesses.

Alternatively, if opponents of wage liens are correct, banks might be willing to loan an equivalent amount to small businesses but charge higher interest rates to compensate for the perceived increase in risk. We should therefore find evidence for the following hypothesis:

Hypothesis 2: The presence of laws permitting wage liens will increase the interest rates charged for small business loans.

Data and Methods

3.1 The Small Business Administration 7(a) Program

The US Small Business Administration (SBA) operates multiple programs to assist firms that “might not otherwise obtain financing on reasonable terms” (Dilger 2016: 1). The 7(a) program, the most widely used one, covers between 40,000 and 65,000 loans to for-profit firms every year, which total tens of billions of dollars.⁶ SBA 7(a) loans are not direct loans from the federal government. Rather, the SBA guarantees a portion of a loan issued by a commercial lender against default—up to 85%, depending on the applicant and the size of the loan. The potential borrower and lender jointly apply for 7(a) loan protection and the SBA reviews applications on a case-by-case basis (Dilger 2016). Lenders are supposed to ensure that borrowers cannot access reasonable loans on market terms before applying for a 7(a) loan, but lenders have substantial autonomy in making this determination (Temkin and Theodos, 2008). SBA 7(a) loans can range from a few thousand dollars to \$5 million. Direct lenders and borrowers negotiate the interest rate, but rates must be within a range set by the SBA (SBA, nd). Firms can use the loans for a wide variety of purposes, but most loans are used to purchase new equipment, to access working capital, or to purchase or start a business (Dilger, 2016).

The SBA 7(a) program covers the majority of firms, but borrowers are not necessarily representative of the broader market. Although the 7(a) program is by name aimed at “small” businesses, it is widely accessible: approximately 97% of US firms qualify according to the current size limitations (Dilger, 2021: 24). Nonetheless, 7(a) borrowers differ from businesses in the conventional loan market. About 25% of businesses that use SBA loan programs are minority-owned, 20% are woman-owned, and 25% are startups— proportions substantially higher than those found in conventional small business loan markets (Temkin, Theodos and Gentsch, 2008). Aside from owner characteristics and firm age, 7(a) borrowers otherwise mirror conventional small business borrowers in firm structure, size, and industry (GAO 1996). By policy design, interest rates on SBA loans generally hover above the prime rate, as do those for conventional

small business loans.⁷ In the two regions of our dataset, 70% of 7(a) borrowers between October 2009 and September 2019 were awarded initial interest rates within 3 percentage points of the prime rate, and 89% within 5 percentage points of prime.

The SBA publicly provides detailed information about all 7(a) loans, thus offering one of the few data sources available on the otherwise opaque commercial lending market. Whereas private lenders are under no obligation to publicly disclose the number, size, or recipient of commercial loans, the SBA has annual disclosures on tens of thousands of specific loans, including information on the total amount of the loan, the percentage of the loan covered by the SBA, the name and location of the lender and the borrower, the interest rate, and the borrower's industry.⁸ SBA data are a rich source of information and have been used in numerous academic studies as a proxy for commercial lending data, especially when researchers aim to understand marginal lending patterns in the commercial lending market (Glennon and Nigro, 2005; DeYoung et al., 2011; Heil, 2019; Srivastav and Vallascas, 2021).

Nonetheless, the data have several limitations. Most importantly, SBA loans make up only a small percentage of commercial lending activity. Commercial banks reported a total of around \$2.75 trillion in outstanding commercial loans in 2020 (BGFRS 2021), a total that far exceeds the 7(a) program's approximately \$159 billion in outstanding loans. SBA data thus provide only a partial view of the entire market. Second, SBA loans by definition go to marginal borrowers, which otherwise have difficulty accessing credit. Specifically, SBA 7(a) borrowers are likely to be younger, and thus potentially less profitable, than conventional borrowers. For our purposes, these marginal borrowers are an appropriate focus: we are interested in understanding the effect of wage lien laws on the commercial credit market, and we expect that marginal borrowers will be more susceptible to the risks alleged by wage lien opponents.

Despite these limitations, we believe that SBA data are adequate for our study. First, although SBA loan-seeking companies are led by principals who differ demographically from those of US businesses in general, the General Accounting Office (GAO) found that firms seeking SBA loans were similar to those seeking conventional loans (GAO, 1996). Research has shown that SBA loan market trends generally mirror those of the conventional loan market when credit tightens (Hancock and Wilcox, 1998) and are aligned with patterns in other small business loan data sources (Bates and Robb, 2013).

3.2 Difference-in-Differences Design

We analyzed the data using regression models based on the difference-in-differences framework (Card and Krueger, 1994). We examined trends in gross amounts of approved loans in two metropolitan areas that span state lines, where one state permits wage liens and the other does not, before and after implementation of the wage lien law. We also examined trends in the initial interest rate of loans in the Washington D.C. metro area. By comparing trends within the same metropolitan area, this approach controls for unobserved features of the regions under study, including industry concentration and labour force composition, and controls for changes over time that are similar for regions on both sides of the state line. The difference-in-differences approach therefore provides a straightforward way to identify the impacts of a change in law.

Our sample included all SBA loans issued to borrowers in the geographic areas under study. For the Chicago region, our treatment group was composed of two Wisconsin counties in the Chicago-Naperville-Elgin US Census Bureau Metropolitan Statistical Area (MSA), i.e., Kenosha and Racine counties. Because Wisconsin lies on the peri-urban fringe of the Chicago metropolitan area, Kenosha and Racine counties are not directly comparable to the primary counties of the Chicago metropolitan area, such as Cook County (which includes Chicago). To find comparable counties for a control group, we compared US Census data for fourteen other Chicago-area counties using

several key variables: population; percentage of residents who self-identified as non-Hispanic/Latino Black or Hispanic/Latino of any race; high school graduation rate; median household income; poverty rate; and unemployment rate. Based on the data, the most similar counties in the MSA were DeKalb, Kankakee, Kendall, and La Salle in Illinois and De Kalb and Porter in Indiana.

For the Washington, D.C. metropolitan region, our treatment group was composed of two Maryland counties: Frederick and Prince Georges. Washington, D.C. itself has low 7(a) activity. As a control group, we thus selected two comparable Virginia counties equally near the District of Columbia: Fairfax and Prince William counties. In Virginia, many incorporated cities are politically independent of their county. We therefore included any independent city lying wholly within one of the two counties: Alexandria, Arlington, City of Fairfax, Falls Church, Frederick, Manassas, and Manassas Park.

For each geographic area, we collected the names of the lender and the borrower, the borrower's ZIP code, the borrower's NAICS code, the gross loan amount approved (gross approval), and the initial interest rate of the loan.⁹ To confirm that the geographic areas were comparable in terms of SBA loan activity, and therefore useful for evaluating the impact of the wage lien law, we plotted the average loan amount in counties with and without such laws. The plots showed similar trends in the Chicago area before and after implementation of the 1994 wage lien law (Figure 1) and in the Washington, D.C. area before and after implementation of the 2014 wage lien law (Figure 2). The SBA started reporting interest rates for specific loans only in 2009, thus limiting the availability of interest rate data to the period following adoption of the Maryland law. By plotting the average interest rate, standardized to the Federal Reserve's interest rate,¹⁰ we see that the rates were similar in 2010 and 2011 and then steeply declined in the untreated areas immediately prior to adoption of the wage lien law in Maryland. In the treated areas, interest rates fell in the year of implementation and then briefly increased the year after, before tracking rates in untreated areas in subsequent years.

Figure 1

Average Loan Size in Chicago's Treated and Untreated Areas

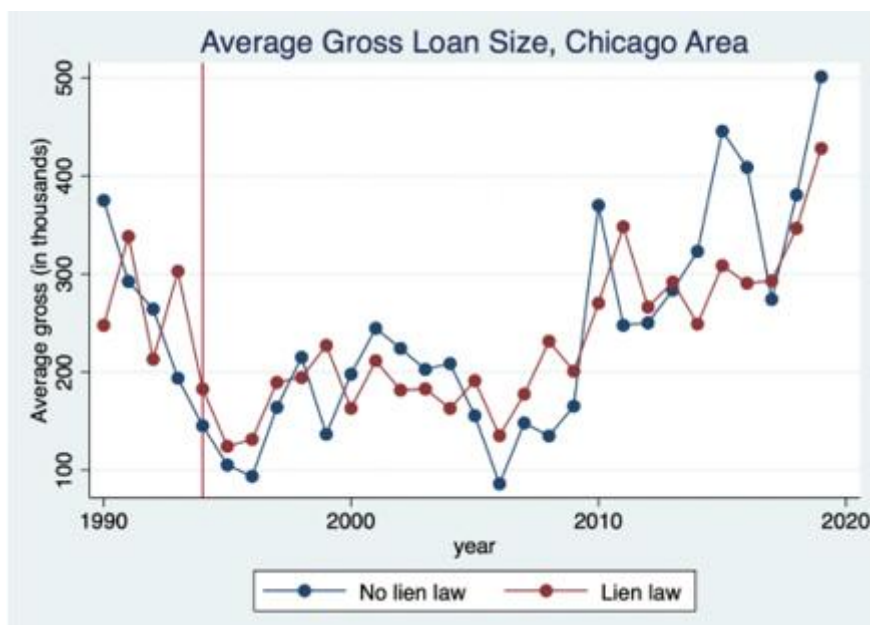


Figure 2

Average Loan Size in D.C.'s Treated and Untreated Areas

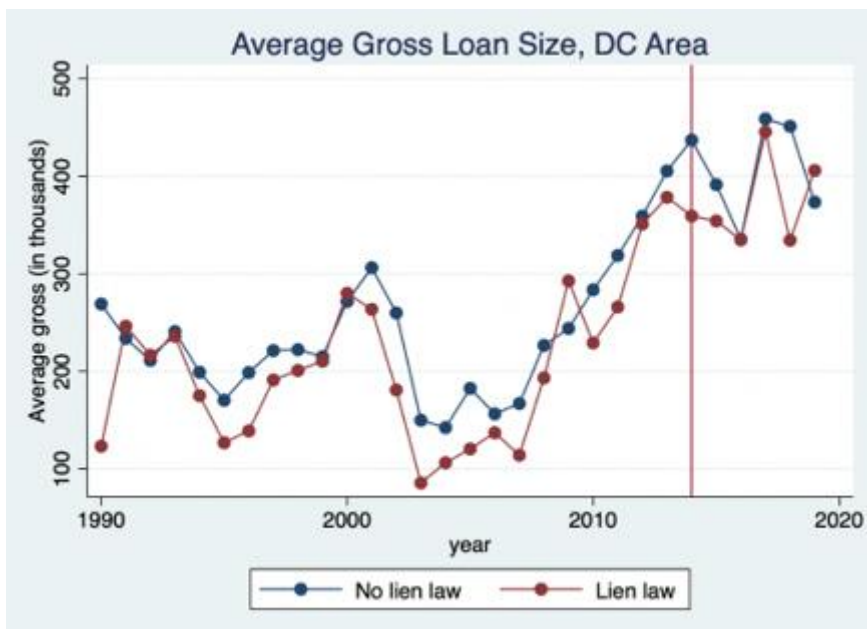


Figure 3

Average Standardized Interest Rate in Treated and Untreated Areas



We explored the effects of wage lien laws on credit markets using five approaches. The first was a regression of gross approval on an interaction between the state and the time period, before and after adoption of the wage lien law in one state, including indicators for both the time of treatment and the treatment group. This approach was not technically a difference-in-differences analysis, insofar as the observations were not the same before and after the treatment: some loans were approved prior to the law and others approved after. It was instead a standard regression analysis using a dummy representing whether the loan was approved before or after adoption of the wage lien law and a dummy treatment group indicator (i.e., being in the state that adopted the law), as well as an interaction between these binary variables. The interaction provided an estimate of the difference (across states) in differences (over time). We also performed the same regression for the initial interest rate of each loan issued in the Washington, D.C. metro area. We did not include additional controls in the model due to the limitations of the data, which lacked information on relevant factors like firm size. However, the difference-in-differences framework should reduce concerns about omitted variable bias insofar as the distributions of the control should be similar across time and treatment groups.

To assess both short- and medium-term effects and to cast as wide a net as possible in assessing the validity of wage lien opponents' arguments, we analyzed the data over multiple time spans. For Washington, D.C., we used intervals of 1 year, 3 years, and 5 years on either side of wage lien law adoption, at the start of 2014. Because the Wisconsin law went into effect at the start of 1994, and the SBA provides only data starting in 1991, we were restricted to 1-year and 3-year analyses for the Chicago area. Table 1 provides descriptive statistics. Because the gross approval outcome variable was skewed, we performed additional analyses using a log transformation on this variable as a robustness check, and those analyses yielded similar outcomes.

Table 1

Descriptive Statistics for Loans

		n	Mean	Median	SD
Gross Approval (US Dollars)					
Chicago	<i>3-year</i> (1991-1996)	479	\$156,510	\$80,000	\$203,435
	<i>1-year</i> (1993-1994)	157	\$178,156	\$92,000	\$217,276
D.C.	<i>5-year</i> (2009-2018)	4102	\$364,062	\$150,000	\$681,046
	<i>3-year</i> (2011-2016)	2260	\$365,059	\$150,000	\$643,733
	<i>1-year</i> (2013-2014)	724	\$403,745	\$188,750	\$679,842
Interest Rate					
D.C.	<i>5-year</i> (2009-2018)	4092	6.647%	6%	1.635
	<i>3-year</i> (2011-2016)	2260	6.286%	6%	1.469
	<i>1-year</i> (2013-2014)	724	5.987%	5.945%	1.349

In addition to analyzing the effects of wage lien laws by using loans as observations, we also grouped the loans by industry, lender, and ZIP code, using these groups as alternative units of analysis. By using the same units of analysis before and after implementation of the wage lien laws, these analyses more closely approximated a traditional difference-in-differences design and served as a robustness check on the loan-based analysis. For each analysis, we took the average gross approved loan size or initial interest rate within the unit of observation (industry, bank, or ZIP code) and ran an identical analysis, using two binary variables and their interaction. We again performed multiple analyses over several time frames, ranging from one year to five years before and after the time of change. Descriptive statistics for gross approval and interest rates by industry, lender, and ZIP code are shown in Table 2.

Table 2

Descriptive Statistics by Industry, Lender, and ZIP Code

		Industry (4-digit NAICS code)				
Gross Approval (USD)	n	Mean	SD	Min	Max	
Chicago	3-year	82	160,187	181,924	10,000	1,100,000
	1-year	28	192,333	243,412	36,000	1,100,000
D.C.	5-year	830	377,366	579,899	5,000	4,288,000
	3-year	480	393,858	615,919	5,000	4,288,000
	1-year	163	441,824	671,809	5,000	4,288,000
Interest Rate						
D.C.	5-year	829	6.68	1.30	3.25	11.25
	3-year	480	6.44	1.22	3.25	10.00
	1-year	163	6.08	1.05	3.25	9.75
		Lender				
Gross Approval (USD)	n	Mean	SD	Min	Max	
Chicago	3-year	126	198,869	184,552	25,000	900,000
	1-year	46	217,766	182,989	48,667	750,000
D.C.	5-year	489	590,214	689,107	8,981	5,000,000
	3-year	293	574,193	666,644	12,167	4,612,000
	1-year	103	593,286	661,259	20,000	3,719,000
Interest Rate						
D.C.	5-year	487	6.22	1.02	1.42	10.75
	3-year	293	6.00	0.87	3.25	9.75
	1-year	103	5.82	0.71	3.25	8.19
		Borrower's Zip Code				
Gross Approval (USD)	n	Mean	SD	Min	Max	
Chicago	3-year	198	190,815	228,568	13,000	1,500,000
	1-year	65	202,298	221,901	16,500	1,100,000
D.C.	5-year	1,028	348,261	436,919	5,000	4,720,000
	3-year	611	346,839	400,160	5,000	3,930,000
	1-year	206	399,726	484,601	5,000	3,930,000
Interest Rate						
D.C.	5-year	1,028	6.61	1.22	2.50	11.38
	3-year	611	6.33	1.08	3.25	10.00
	1-year	206	6.13	1.04	3.25	9.75

Results

When all loan activity was used in regression analyses to predict gross approval, no statistically significant effects were found to support the idea that wage liens impact business access to credit. Table 3 shows the results of the first regression analyses. The regressions for gross approvals in the Chicago metro area, using either a one- or three-year before-and-after interval, yielded no significant results on the coefficient of the interaction, which represents the effect of wage lien law adoption. A significant negative coefficient of the time dummy indicates that total loan activity declined following wage lien law adoption, but in ways that did not vary by treatment status. The regressions for the Washington, D.C. metro area in one-, three-, and five-year before-and-after intervals yielded no statistically significant results for the interaction. For these results, the coefficient is indeed negative, as predicted by the hypothesis, but the p -value does not warrant rejecting the null hypothesis. The R-squared value from each regression also shows the limited predictive power of these variables, which explain less than 10% of the variation in Chicago and less than 1% in Washington, D.C.

Table 3

Regression Analyses of Gross Approvals for SBA 7(a) Loans by Metropolitan Area

	Chicago Metropolitan Area		Washington D.C. Metropolitan Area		
	1993-1994 (1-year)	1991-1996 (3-year)	2013-2014 (1-year)	2011-2016 (3-year)	2009-2018 (5-year)
Time	-48,486 (42,430)	-133,760*** (31,667)	31,582 (63,772)	18,371 (32,738)	95,299*** (24,738)
Treatment	109,072 (110,032)	35,240 (56,943)	-27,022 (75202)	-29,598 (43,233)	-20,713 (27,897)
Interaction	-71,600 (116,072)	-1,150 (59,798)	-50,716 (104,449)	-3,464 (57,287)	-31,164 (42,112)
Intercept	242,559*** (51,249)	247,560*** (29,589)	405,429*** (40,802)	364,054*** (22,797)	319,722*** (14,462)
Observations	157	479	724	2,260	4,102
R-Squared	0.030	0.073	.002	.001	.005

Robust standard errors in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

If wage lien laws make lending riskier, an alternative hypothesis might be that more borrowers will be excluded from conventional loans and forced into SBA 7(a) loans instead. Because SBA 7(a) loans carry a government guarantee, lenders may be willing to accept riskier loans and thus take on new borrowers. We cannot determine whether the proportion of loans with SBA guarantees increased following adoption of wage lien laws, given the lack of systematic data on the broader commercial loan market. However, if this were the case, we would expect to see an increase in total gross approved loans in the treated areas, and this increase would diverge from the trend we see in the untreated areas. Figures 3 and 4 suggest otherwise: SBA loan activity in treated areas largely tracked loan activity in untreated areas, both before and after the wage lien law went into effect.

Figure 4

Total Chicago Area SBA Loan Activity

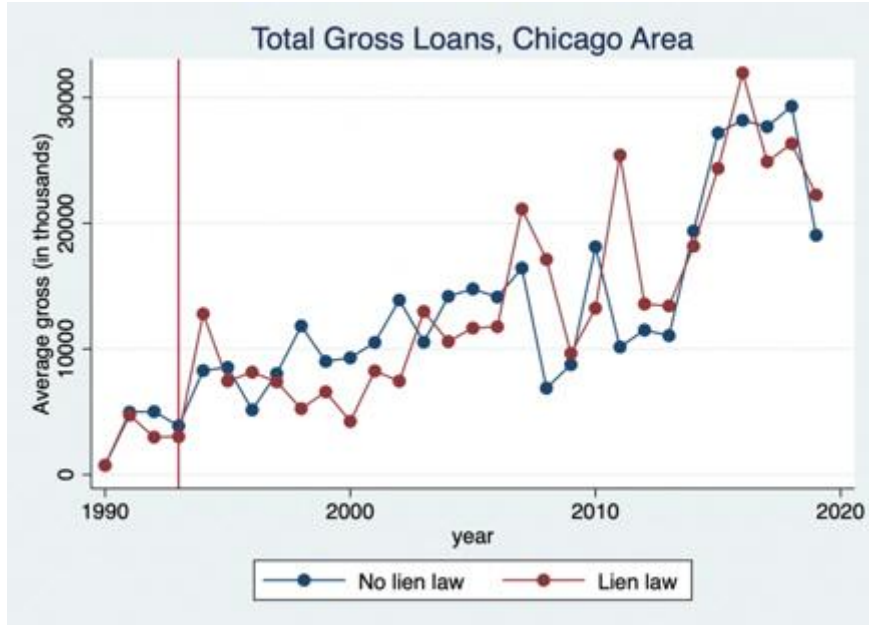


Figure 5

Total D.C. Area SBA Loan Activity

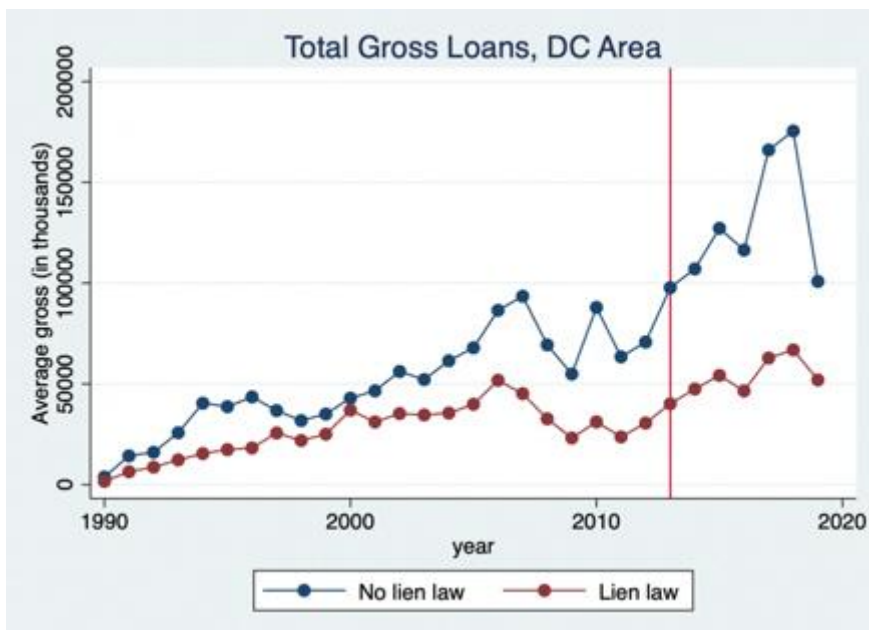


Table 4 provides results from the regression analysis for interest rates in the Washington, D.C. area. The three- and five-year intervals have significant coefficients for both the treatment and the interaction variables. However, in both cases, the negative coefficient of the interaction variable signals a lower interest rate post-treatment. Once again, the low R-squared values demonstrate the limited importance of these variables in explaining differences between local credit markets.

Table 4

Regression Analyses of Interest Rate for SBA 7(a) Loans

	Washington D.C. Metropolitan Area		
	<i>2013-2014 (1-year)</i>	<i>2011-2016 (3-year)</i>	<i>2009-2018 (5-year)</i>
Time	0.106 (0.123)	0.297*** (0.0736)	0.866*** (0.0563)
Treatment	0.239 (0.153)	0.333*** (0.102)	0.294*** (0.070)
Interaction	-0.316 (0.213)	-0.470*** (0.134)	-0.249** (0.105)
Intercept	5.911*** (0.0844)	6.094*** (0.062)	6.088*** (0.036)
Observations	724	1,486	4,092
R-Squared	0.004	0.004	0.060

Table 5 summarizes the findings of difference-in-differences analyses of gross approval, using loans grouped by industry (NAICS 4-digit codes), by lending institution, and by ZIP code for the Chicago metropolitan area. Table 6 summarizes the parallel findings for the Washington, D.C. metropolitan area. Again, the wage lien treatment generally has little effect. Regardless of the grouping, no coefficients had statistical significance.

Table 5

Regression Analyses of Gross Approvals for Chicago-Area SBA 7(a) Loans by Lender, Industry, and ZIP Code

	Chicago Metropolitan Area					
	Lending Bank		NAICS-4 Code		ZIP Code	
	1993- 1994 (1-year)	1991- 1996 (3-year)	1993- 1994 (1-year)	1991- 1996 (3-year)	1993- 1994 (1-year)	1991- 1996 (3-year)
Time	-39,526 (60,905)	140,604* ** (44,384)	-102,435 (69,569)	148,618* ** (50,486)	-80,435* (-47,412)	156,405* ** (-38,847)
Treatment	53,186 (112,451)	-25,349 (62,046)	439,429 (345,338)	105,286 (111,307)	155,371 (-148,279)	53,821 (-74,032)
Interaction	-4,630 (131,278)	60,708 (71,744)	-384,124 (355,372)	-72,751 (117,537)	-63,112 (-161,186)	-8,781 (-81,137)
Intercept	216,889 *** (44,589)	281,812* ** (38,051)	210,571 *** (63,583)	250,861* ** (48,033)	212,865 *** (-40,305)	276,132* ** (-34,265)
Observations	49	137	33	101	65	198
R-Squared	0.028	0.097	0.308	0.174	0.104	0.118

Robust standard errors in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 6

Regression Analyses of Gross Approvals for D.C.-Area SBA 7(a) Loans by Lender, Industry, and ZIP Code

		Washington, DC Metropolitan Area								
		Lending Bank			NAICS-4 Code			ZIP Code		
		2013- 2014 (1- year)	2011- 2016 (3- year)	2009- 2018 (5- year)	2013- 2014 (1- year)	2011- 2016 (3- year)	2009- 2018 (5- year)	2013- 2014 (1- year)	2011- 2016 (3- year)	2009- 2018 (5- year)
Time		110,970 (149,279)	136,994* (82,112)	201,523*** (62,209)	-159,744 (113,079)	-11,866 (65,355)	43,699 (43,251)	-2,564 (79,457)	13,149 (36,855)	88,949*** (31,686)
Treatment		66,933 (192,360)	-9,416 (91,606)	29,797 (64,109)	-91,187 (148,362)	-3,057 (78,209)	15,506 (50,550)	56,297 (129,808)	9,986 (59,134)	6,155 (39,706)
Interaction		-164,313 (253,911)	-118,878 (128,534)	-120,227 (97,457)	211,308 (193,562)	42,998 (105,384)	36,571 (73,888)	-188,666 (146,479)	-31,224 (72,767)	-44,458 (57,969)
Intercept		532,932*** (99,290)	498,252*** (51,214)	451,372*** (34,738)	513,387*** (104,163)	397,294*** (49,335)	338,170*** (30,216)	415,733*** (56,390)	342,227*** (26,273)	306,924*** (17,589)
Observations		139	407	688	237	698	1,203	206	611	1,029
R-Squared		0.004	0.010	0.015	0.008	0.000	0.003	0.016	0.000	0.008

Robust standard errors in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

Table 7 provides the results of the difference-in-differences analyses of interest rates in the Washington D.C. area, again grouping loans by industry (NAICS 4-digit code), by lending institution, and by ZIP code. The significant effect in the loan-level models (Table 3) does not appear when observations are grouped by lender or by industry. However, it does appear in the 5-year timeframe when loans are grouped by ZIP code. Again, the interaction coefficient mitigates the effect of being in the treated area. Small R-squared values suggest that wage lien laws explain very little of the variation in loan outcomes.

Table 7

Regression Analyses of Interest Rates for D.C.-Area SBA 7(a) Loans by Lender, Industry, and ZIP Code

	Washington, DC Metropolitan Area								
	Lending Bank			NAICS-4 Code			ZIP Code		
	2013- 2014 (1- year)	2011- 2016 (3- year)	2009- 2018 (5- year)	2013- 2014 (1- year)	2011- 2016 (3- year)	2009- 2018 (5- year)	2013- 2014 (1- year)	2011- 2016 (3- year)	2009- 2018 (5- year)
Time	6.490* ** (2.476)	- 16.02* ** (2.526)	- 23.45* ** (2.089)	8.771* ** (2.444)	- 15.65* ** (2.192)	- 21.68* ** (1.745)	7.463* ** (2.601)	- 13.39* ** (2.162)	- 20.26* ** (1.773)
Treatment	3.419 (3.181)	4.440 (2.785)	1.723 (2.283)	3.308 (2.873)	1.540 (2.395)	1.657 (1.795)	-0.366 (3.067)	2.878 (2.480)	3.442* (1.883)
Interaction	-5.997 (4.079)	-2.797 (3.981)	-1.593 (3.321)	-2.715 (3.838)	0.0519 (3.484)	-2.190 (2.792)	-1.128 (4.061)	-3.167 (3.649)	-3.641 (2.998)
Intercept	59.40* ** (2.065)	56.38* ** (1.684)	49.35* ** (1.372)	59.64* ** (1.845)	60.25* ** (1.521)	50.64* ** (1.098)	62.15* ** (1.978)	58.40* ** (1.405)	49.43* ** (1.053)
Observations	139	407	688	237	698	1,203	206	611	1,029
R-Squared	0.045	0.156	0.224	0.071	0.108	0.180	0.060	0.103	0.178

Robust standard errors in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$

These analyses fail to provide evidence that wage lien laws constrict access to credit, either in terms of gross approved loans to borrowers or in terms of interest rates. We find little compelling evidence in favour of either hypothesis, a finding that casts doubt on claims that wage lien laws affect credit markets.

Discussion

We examined the evidence for common arguments used by wage lien opponents, specifically that wage liens reduce business access to credit. Using a difference-in-differences framework, we found no evidence to support the claim that such liens reduce access to credit in either of the two US markets with wage lien laws. Point estimates in our models are small and largely not statistically significant. Our findings bolster existing findings that support adoption of wage lien laws (Cho, Koonse and Mischel, 2013).

These results *do not* prove that wage liens have no effect on credit markets. In the framework of frequentist statistics, failure to reject the null hypothesis is not confirmation of the null. However, given the unique value of wage liens for addressing wage theft—and given extensive evidence that wage theft is pervasive and detrimental—a lack of substantial evidence *supporting* opponents of these laws is an important contribution to policy discussions. We cannot definitively claim that wage lien laws do not affect credit markets, but we can argue that no evidence exists to support those who argue for such an effect.

Our study is also limited to aggregate outcomes and may overlook variation in the impact of wage lien laws across types of borrowers. The terms of a business loan are affected by very complex factors, some of which our method glosses over by focusing on the overall effect of the treatment. In doing so, we set aside much of the nuance that lenders, borrowers, and SBA administrators use when determining loan packages. We take a high-level view of the problem, using the terms of wage lien law opponents.

SBA data have several limitations, being most importantly not representative of the credit market as a whole. Our findings thus rely on the assumption that changes are felt first by more marginal borrowers and would thus lead to lower SBA loan activity or higher interest rates. If liens indeed constrict credit markets as opponents of wage lien policies suggest, we believe that borrowers on the margins should experience the strongest effects. However, unobserved dynamics may concentrate the impact of wage lien laws on more conventional borrowers or push them into SBA programs. An alternative hypothesis—that such laws push borrowers out of the conventional loan market and into SBA 7(a) loans—appears to be unsupported by the SBA data, although that hypothesis cannot be completely assessed without systematic data about the conventional market. We follow existing scholarship that uses SBA data to estimate broader trends in small business lending during times of credit constriction (Hancock and Wilcox, 1998). We thus assume that SBA loans reflect the dynamics of the broader business credit market.

Further research may strengthen the case for wage lien laws and weaken the arguments of their opponents. First, more research is needed on their effectiveness and their positive effect on worker outcomes, particularly with respect to the more recent wage lien law in Maryland. Second, although research tends to support their effectiveness, such laws are not likely enough, on their own, to eradicate wage theft. As discussed, employers have numerous options for evading payment of owed wages, including transfer of assets and strategic use of bankruptcy. Wage liens provide some protection against these tactics but are by no means a silver bullet. For instance, although the Wisconsin law has been successful (Cho, Koonse, and Mischel, 2013), the continuing prevalence of wage theft in the construction industry provides ample evidence that wage liens alone do not solve the problem (Bernhardt et al., 2009). Finally, research is needed to address two other opposing arguments: wage liens violate the employer's right to due process; and they impose a logistical burden on the courts. These arguments may be assessed by studying data from the legal system.

Conclusion

Industrial relations scholars have highlighted the role of public policy in improving employment standards in an era of decreased worker power. Despite the benefits of public policy responses, scholars have repeatedly shown that such responses are undermined by difficulties with enforcement. Wage lien laws provide governments with a unique mechanism to deal with employment violations by marshalling the state's power to redress claims without lengthy recourse to a state enforcement agency for adjudication. Wage lien laws have aroused fierce opposition, including bold unsubstantiated claims that they restrict access to credit. We have empirically assessed some of those claims and find no supporting evidence.

Wage theft is ubiquitous. Its pervasiveness does not, however, mitigate the personal injury to workers whose wages have been stolen and who are often already among the most disadvantaged in the labour market. In the aggregate, these personal losses also aggravate broader social problems, including poverty, reliance on government programs, and income inequality. Governments in the US can bring in laws to help workers recover stolen wages and incentivize employers to comply with laws on wages and hours. The biggest obstacle is political. Our study casts doubt on the arguments used against worker advocates and wage liens. The weak case against wage liens only bolsters the strong case for them.

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Notes

[1] The term ‘judgment’ refers to the end result of a US legal process settled by a court. In the context of a wage dispute, a judgment is simply a ruling by the court that a worker is owed a specific amount of money. While a judgment enables options for debt payment, most workers must collect the money by themselves.

[2] From this point forward, all references to “wage liens” in this study refer to pre-judgment liens unless otherwise noted.

[3] Jurisdictions vary in terms of factors, including the cap on the amount associated with the lien, the priority of the lienholder’s claim among creditors, how far removed from the property owner a claimant can be, types of real property that can be used, and degree of homestead exemption (FASA 2020).

[4] Wage liens used to be prevalent in the US. Several other states have vestigial wage lien laws that allow liens for only small amounts of money.

[5] Each bill went into effect the following year.

[6] Based on SBA 7(a) loan data reports filed between 2010 and 2019.

[7] SBA 7(a) loan interest rates are negotiated between the lender and the borrower within a range designated by the SBA. The prime rate is one of three permissible anchor points (SBA nd). The “prime rate” is the lowest rate used by a majority of the 25 largest commercial lenders. The Federal Reserve surveys the 25 banks and reports the prime of those banking institutions for use as a reference (Federal Reserve, nd).

[8] The SBA uses the North American Industry Classification System (NAICS), a six-digit code commonly used in Canada, Mexico, and the United States to classify industry.

[9] Under select circumstances, SBA 7(a) loans can have variable interest rates.

[10] We standardized the interest rate over the Federal Reserve rate for this figure, since we were looking at a broader range in time. In the regression analyses, we did not standardize because the federal rate changed so little during the five-year window in question.

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