

# Procompetitive Effects of State Antitrust Laws: Evidence from the Progressive Era

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## Abstract

Most U.S. states adopted an antitrust statute in the late nineteenth or early twentieth century to regulate anticompetitive conduct and promote competition among firms. I estimate the long-term effects of these laws on manufacturing outcomes using newly digitized state-by-industry data from historical censuses of manufactures. Using difference-in-differences and event study models that account for the staggered nature of treatment timing, I find that these statutes did little to limit the influence of powerful incumbents. The number of manufacturing establishments rose by about 10 percent following adoption, but evidence on ownership structure suggests that this growth was likely driven by the expansion of incumbent firms rather than new entry. I also find that the labor share declined by about 4 percent overall and by about 7 percent in trust-affiliated industries, suggesting that dominant firms held on to their market power despite new legal constraints on anticompetitive conduct. I find no evidence of increased employment or reduced profits, providing further evidence that firms made few meaningful adjustments to pricing or production behavior in response to these laws. These results suggest that antitrust legislation alone is insufficient to disrupt concentrated market power without complementary enforcement mechanisms.

JEL classification: K21, L40, L60, N11, N41, N61, N81

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# 1 Introduction

Between 1888 and 1940, 39 states enacted antitrust statutes, but little is known about how these measures affected the economies of the states that enacted them. Antitrust laws restrict firms' ability to monopolize an industry or collude with each other and are designed to protect competition among firms. In turn, competition among firms can create benefits for consumers, such as downward pressure on prices and incentives for firms to maintain product quality. In this paper, I study the impacts of antitrust laws states enacted in the late nineteenth and early twentieth centuries. In particular, I examine the effects of these laws on competition and innovation.

The origins of U.S. antitrust law lie at the state level of government. Thirteen states had already adopted an antitrust statute of their own by the time the Sherman Act—the first federal antitrust law—was enacted in 1890. As the United States underwent structural changes that transformed the nation's economy from primarily agrarian to primarily industrial, large firms emerged. The rise of these so-called “trusts” challenged the position of weaker firms, fueling antimonopoly sentiment and public support for antitrust legislation at both the state and federal levels.<sup>1</sup> Policymakers today, who continue to debate antitrust reform as a means of addressing rising concentration, would benefit from having more information about the effects of prior changes to the nation's antitrust laws.

I use data from four main sources to assess the impact of state antitrust laws. The first source pertains to the state antitrust laws themselves. To identify state antitrust statutes in force in the late nineteenth and early twentieth centuries, I reviewed state session laws and legal codes from 1860 through 1940 and identified provisions restricting anti-competitive conduct, such as price fixing. After identifying all such provisions, I coded the content of states' laws to understand the specific activities each law condemned, the processes for civil and criminal enforcement under each law, the civil damages each law authorized, and the fines and prison sentences each law authorized. The second data source relates to manufacturing establishments. To assess the impact of state antitrust laws on the manufacturing sector, I use state-by-industry tabulations from the census of manufactures for 1850 through 1940. The particular outcomes I examine are the number of manufacturing establishments, as well as the profits and labor demand of these establishments. The manufacturing sector comprised a relatively large share of the U.S. economy during the study period, which motivates my focus on manufacturing in this paper.<sup>2</sup> Third, I use data from the Comprehensive Universe of U.S. Patents (CUSP) to assess the impact of state antitrust laws on innovation, as proxied by patenting behavior (Berkes 2018). Finally, to identify newspaper articles mentioning one or more state antitrust laws, I searched two popular digital archives for terms

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<sup>1</sup>“Trust” became a catch-all term for industrial combinations perceived as threatening competition during the late nineteenth century (Werden 2020, pp. 11–17). Originally, however, a trust referred to a specific legal arrangement used to coordinate cooperative actions among parties that might otherwise compete. Under a trust, competing firms placed stock under the control of appointed trustees, who acted collectively to manage operations and limit competition among participants.

<sup>2</sup>Figure A1 shows how the manufacturing sector's share of GDP evolved between 1850 and 2023. In 1890, shortly after the first state antitrust laws took effect, manufacturing comprised about 27 percent of nominal GDP.

relating to state antitrust laws. These articles allow me to understand how state antitrust laws were perceived by the general public and enforced by state governments during the study period.

I use difference-in-differences and event study models that account for the staggered nature of treatment timing to study the effects of state antitrust laws (Callaway and Sant'Anna 2021). This approach exploits variation in the timing of states' adoption of antitrust laws and compares outcomes in states with antitrust laws to outcomes in states without antitrust laws, before and after the adoption of an antitrust law. Because the timing of effects is of interest, I also employ event study models in my analysis. One identification advantage of studying antitrust in a historical context is that this approach allows for comparison to a period without any antitrust laws, which papers studying antitrust in modern settings are unable to do.

I find that these statutes increased the number of manufacturing establishments by roughly 10 percent, particularly in industries not dominated by trusts. However, ownership patterns suggest that this growth was driven by the expansion of incumbent firms rather than new entry. At the same time, the labor share declined by 4 percent overall and 7 percent in trust-affiliated industries, while profits and employment remained unchanged, indicating that dominant firms retained their market power despite new legal constraints. I also document an increase in firm-sponsored patents, pointing to positive spillovers on innovation. Taken together, these findings suggest that while state antitrust laws encouraged expansion and innovation, they fell short of meaningfully curtailing the power of dominant firms.

This paper is the first quasi-experimental, quantitative study to examine the effects of state antitrust laws in the late nineteenth and early twentieth centuries. This evidence contributes important knowledge about the broader economic impacts of antitrust law. Existing papers on this topic tend to focus on legal outcomes, such as enforcement actions and cases won against trusts in court (e.g., Lamoreaux and Phillips-Sawyer 2021). In this paper, however, I analyze the extent to which state antitrust laws had a discernible effect on markets by examining important economic indicators. Moreover, this analysis examines long-term effects of antitrust, employing data covering nearly a century. Existing scholarship tends to examine a considerably narrower window of time. This paper also contributes to a thin literature on the significance of early state antitrust policy (Clay and Troesken 2002; Lamoreaux and Phillips-Sawyer 2021; May 1987; Troesken 2000). Prior scholarship has paid relatively more attention to the federal antitrust laws, even though states led the way in establishing antitrust policy in the United States.<sup>3</sup> Another contribution of this paper is the work I have done to characterize the evolution of state antitrust laws over time, which allows me to describe state antitrust law in its formative era in greater detail than previous scholarship. Finally, this paper contributes to our understanding of the Progressive Era, a period in American history characterized by sweeping reform. As society undergoes what some are calling a second Gilded Age,<sup>4</sup> lessons from this period provide valuable insight into the challenges of today's world.

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<sup>3</sup>For example, Stigler (1966) and Burns (1977) study the effects of federal antitrust law.

<sup>4</sup>See, for example, Wu (2018).

## 2 Background

### 2.1 Historical Context

The antitrust movement emerged during a period of rising concentration, as large firms increasingly displaced smaller competitors in the U.S. economy. As shown in Figure 1, references to “trusts” rose sharply in the mid- to late 1880s, underscoring the growing public attention directed toward big business. This public concern emerged alongside major changes in transportation, communications, and other technologies that encouraged consolidation and eroded the position of small firms (Attack 1985; Higgs 1971; James 1983). Railroads, which made long-distance shipping easier and cheaper, allowed firms to serve increasingly distant markets (Donaldson and Hornbeck 2016; Hornbeck and Rotemberg 2019, 2024). New communications technologies, such as the telegraph (and later, the telephone), allowed national markets to emerge as firms could more easily do business with partners in far-flung places (Yates 1986). New production technologies, such as the Bessemer method of steelmaking, favored mass production (Rogers 2009). The efficiencies associated with these technological advances enabled firms to dramatically increase output, often surpassing market demand and resulting in widespread industrial overcapacity that pushed firms toward consolidation as a strategy for stabilizing prices and surviving competition (McCraw 1981). As a result, large firms began to capture increasing shares of the market; as Figure 2 illustrates, concentration in the manufacturing sector rose steadily in the decades preceding the antitrust movement. As large firms became more numerous, concerns about their growing power helped set the stage for the rise of the antitrust movement.

The relative decline of agriculture also played a role in the rise of the antitrust movement. According to the standard “agrarian discontent” explanation for the rise of antitrust, farmers found themselves increasingly vulnerable to the power of large corporations as the United States industrialized, leading them to lend strong support for antitrust legislation (Thorelli 1955, p. 58).<sup>5</sup> The

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<sup>5</sup>Despite extensive scholarly attention, no consensus exists on what—or who—actually drove the antitrust movement. Some scholars have expressed doubt that farmers’ cries for help were the result of genuine anticompetitive abuses and have alleged that the real driving force behind antitrust legislation was interest-group lobbying by agricultural groups (Boudreaux and DiLorenzo 1993; Boudreaux, DiLorenzo, and Parker 1995; Dilorenzo 1985; Dilorenzo and High 1988; Gilligan, Marshall, and Weingast 1989). Other scholars, however, reject the idea that agricultural groups were the primary political force behind early antitrust legislation, instead arguing that urban small business interests, especially those facing displacement by more efficient firms, played a more decisive and sustained role (Baxter 1980; John 2017; Stigler 1985). A third group emphasizes that political pressure to respond to perceived monopoly abuses, especially in the wake of widespread public outrage over trusts like Standard Oil and the Whiskey Trust, played a key role in prompting both state and federal antitrust legislation—even in the absence of clear evidence that market concentration had materially increased (Flynn 1988; Hofstadter 1979; Lande 1982; Lande 1988; Letwin 1965; Millon 1988; Werden 2020). (Importantly, states enacting antitrust statutes were not uniquely characterized by high or rising concentration. If anything, their levels of manufacturing concentration during the mid- to late-nineteenth century were less than those of states that never adopted an antitrust statute (see Figure A2 and Figure A3). This pattern casts doubt on the claim that heightened monopoly power was the primary trigger for early antitrust legislation.) A fourth view, associated with Bork (1966) and defended more recently by Crane (2014), holds that the Sherman Act was a “consumer welfare prescription” intended to prevent only those restraints that harmed allocative efficiency, regardless of the political coalitions that gave rise to its passage. A fifth view emphasizes that the Sherman Act was

geographic pattern of early antitrust legislation reflects the agricultural roots of the movement. As Figure 3 shows, the first states to adopt antitrust laws were concentrated in the largely agricultural Midwestern United States. Monopolization among railroads was an acute concern for farmers, who relied on railroads and railroad-owned grain elevators for the transportation and storage of their crops (Thorelli 1955, p. 143; Phillips-Sawyer 2019, p. 4). In particular, farmers alleged that monopolistic railroads charged unfair prices for their services.<sup>6</sup> Libecap (1992) also highlights the role of the Chicago meat packers in depressing cattle prices, which led Midwestern farmers to lobby for antitrust legislation as a way to counteract what they saw as monopsonistic abuses in livestock markets.

States were the first to respond to popular cries for antitrust legislation, with 13 enacting statutes before the federal government.<sup>7</sup> Although state legislatures do not consistently record vote counts,<sup>8</sup> the historical record suggests that state antitrust laws often enjoyed broad, bipartisan support. For example, Texas's 1889 antitrust statute passed unanimously.<sup>9</sup> When that statute was later overturned, the legislature responded by passing a new antitrust law in 1903, which again received overwhelming support—unanimous approval in the Senate and a 103–2 vote in the House.<sup>10</sup>

This state-level action laid the groundwork for antitrust reform at the federal level, beginning with the Sherman Antitrust Act of 1890. There is some evidence that trusts welcomed the passage of the Sherman Act because they believed its passage would thwart state antitrust policy, which they perceived to be a greater threat. For example, Troesken (2000) finds that trust stocks fell significantly in response to state antitrust enforcement, while trust stocks increased in value after two different federal antitrust laws were proposed, suggesting investors favored at least some forms of federal antitrust legislation. The apparent preference of trusts for federal antitrust legislation over state enforcement underscores the initial ineffectiveness of the Sherman Act. In its early years, federal enforcement was notoriously weak, hampered by a lack of resources and political will. It was

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a political compromise among factions with conflicting goals and that its vague language was designed to paper over disagreement, making any unified theory of original intent—such as Bork's consumer welfare thesis—fundamentally untenable (Grandy 1993; Hazlett 1992).

<sup>6</sup>Economic historians have provided empirical support for farmers' allegations of high railroad freight rates. Higgs (1970) argues that, despite nominal declines in freight rates, the real cost of railroad transportation relative to crop prices remained essentially flat from the 1860s through the 1890s. Responding to Higgs's paper, Aldrich (1980) uses transaction data from the Chicago Board of Trade and published freight rates to argue that real freight rates fell until the early 1880s, then stabilized or rose during the late 1880s and 1890s—a pattern consistent with the political science theory of “relative deprivation,” which posits that protest arises not from steady hardship but from the reversal of improving conditions.

<sup>7</sup>Antitrust legislation was neither the first nor the sole form of government regulation that state legislatures pursued in response to political pressure by aggrieved farmers in the mid-nineteenth century. In the 1870s, over a decade before Iowa became the first state to adopt an antitrust statute in 1888, several state legislatures enacted “Granger” laws to regulate the prices and practices of railroads and grain elevators (Noll and Kanazawa 1994). In 1889, state legislatures in several Midwestern states also passed or considered livestock inspection laws to respond to farmer concerns about falling cattle prices and the market power of Chicago meat packers (Libecap 1992).

<sup>8</sup>Texas is an exception; its published session laws include vote counts for each legislative chamber.

<sup>9</sup>Act of March 30, 1889, Chapter 117, 1889 Texas Acts 141.

<sup>10</sup>Act of March 31, 1903, Chapter 94, 1903 Texas Acts 119.

not until 1903 that Congress appropriated funds to support federal antitrust enforcement (Thorelli 1955, p. 3). Even then, staffing remained minimal; just two professional staff members responsible for antitrust enforcement are identified in the 1904 *Department of Justice Register* (Werden 2018, p. 425). Moreover, the Antitrust Division within the U.S. Department of Justice was not created until 1919 (Werden 2018). Thus, despite its landmark status, the Sherman Act's immediate impact on trust-busting was limited by inadequate implementation.

Relative to the federal government's meager enforcement efforts, state antitrust enforcement was robust in the late nineteenth and early twentieth centuries. State enforcement actions were numerous and sometimes targeted major interstate combinations (May 1987, 1990). For example, Standard Oil faced 24 separate state actions (Bringhurst 1979, p. 204).<sup>11</sup> Weak federal enforcement also meant that state action was often the only remedy available to address anticompetitive behavior. For example, in *United States v. E. C. Knight Co.*, the U.S. Supreme Court ruled that the Sherman Act did not apply to manufacturing.<sup>12</sup> By ruling that the Sherman Act did not apply to manufacturing, the Court effectively made state courts the primary venue for addressing anticompetitive conduct in this crucial sector of the economy. Until the expansion of federal commerce power in the New Deal era, antitrust enforcement was largely a state responsibility (Columbia Law Review 1961).

Federal antitrust law was intended to supplement state antitrust laws already in place. Senator John Sherman, the namesake of the Sherman Act, advocated for the passage of the Sherman Act by arguing that the law would simply apply on the national level law that already existed within several states. As Hovenkamp (1983, p. 375) writes, "the legislative history of the federal antitrust law indicates that Congress intended to leave state antitrust enforcement more or less intact but to provide an additional federal forum for dealing with restraints of trade which exceeded the jurisdiction of the courts of any particular state." The enduring importance of state antitrust laws, even after the enactment of federal legislation, is reflected in early court decisions that often interpreted state antitrust statutes expansively (Lamb 2001; May 1990).<sup>13</sup> As Brinkerhoff (2017) shows, state courts during the formative period typically focused on whether the restraint itself crossed state lines—not whether its effects did. This approach allowed for meaningful state enforcement even when the alleged anticompetitive conduct affected interstate commerce.

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<sup>11</sup>May (1987, p. 537) notes that Texas collected \$1,623,500 in penalties in a single case against the Texas affiliate of the Standard Oil Company in *Waters-Pierce Oil Co. v. Texas*. The state fined the defendant \$1,500 per day for a total of 1,033 days in violation of a 1899 law and \$50 per day for a total of 1,480 days in violation of a 1903 law. The fine amounts to about \$57 million in 2024 dollars (Federal Reserve Bank of Minneapolis n.d.). The defendant paid 32 percent of the fine and Standard Oil of New Jersey paid the remaining balance. 212 U.S. 86 (1909). Act of May 25, 1899, Chapter 146, 1899 Texas Acts 246. Act of March 31, 1903, Chapter 94, 1903 Texas Acts 119.

<sup>12</sup>156 U.S. 1 (1895).

<sup>13</sup>For example, in *Hammond Packing Co. v. Arkansas*, the U.S. Supreme Court upheld Arkansas's ability to fine a corporation for participating in a price-fixing conspiracy, even though the conspiracy was not alleged to have occurred or had effects within the state. 212 U.S. 322 (1909). Likewise, in *International Harvester Co. v. Missouri*, the U.S. Supreme Court upheld Missouri's antitrust action against an out-of-state trust that had formed a monopoly affecting local markets, concluding that the corporation's presence and conduct within the state subjected it to Missouri's jurisdiction. 234 U.S. 199 (1914).

## 2.2 Existing Literature and Contributions

The effects of antitrust policy on competition have been extensively studied, yet the literature remains divided on the upshot.<sup>14</sup> Several papers studying antitrust policy in historical contexts suggest that the Sherman Act and its initial implementation had limited or even counterproductive effects. These papers argue that landmark antitrust interventions—including early enforcement of the Sherman Act (Stigler 1966); the 1911 dissolutions of Standard Oil, American Tobacco, and American Snuff (Burns 1977); Supreme Court decisions against railroad combinations (Binder 1988); and the 1948 Paramount decision mandating vertical divestiture in the film industry (Gil 2015)—failed to meaningfully alter market structures, suppress anticompetitive behavior, or lower prices. Bittlingmayer (1992, 1993) echoes this skepticism by arguing that uncertainty surrounding antitrust enforcement disrupted financial markets and real investment, potentially causing unintended contractionary effects on the economy. In contrast, subsequent studies argue that historical episodes of antitrust enforcement constrained monopoly power and altered firm behavior. For example, Mullin, Mullin, and Mullin (1995) find that the initiation of the 1911 antitrust suit against U.S. Steel led to stock gains among the conglomerate's customers, indicating expectations that a dissolution would lower steel prices and increase output, and Mullin and Snyder (2021) find that competition increased following the 1952 DuPont patent-licensing remedy and the 1911 breakup of American Tobacco.<sup>15</sup> Moreover, both Prager (1992) and Baker, Frydman, and Hilt (2023) show that financial markets responded strongly to signals of heightened antitrust enforcement, suggesting that investors viewed stricter antitrust policy as a credible threat to firm profitability. Together, these studies highlight ongoing disagreement about whether early antitrust enforcement meaningfully affected markets.

This pattern of disagreement persists in the literature studying antitrust policy in relatively more modern settings.<sup>16</sup> Several studies question the effectiveness of antitrust enforcement, finding

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<sup>14</sup>Though I focus on studies of U.S. antitrust policy in this literature review, studies from international contexts also provide insights. Across multiple papers, George Symeonidis finds that cartelization generally harmed economic performance in the United Kingdom, while cartel policy reforms led to more intense competition, which in turn increased labor productivity growth, had no adverse effect on wages, shifted firms toward price rather than non-price competition, encouraged innovation, and sometimes even forced unprofitable cartels to disband (Symeonidis 2000, 2008, 2019, 2024). Geloso (2020) and Crane (2023) also study historical decartelization policies in Canada and Germany, respectively, while Reed et al. (2022) examine modern Mexico and Aguzzoni, Langus, and Motta (2013) study the European Union. Several studies examine how antitrust laws influence economic outcomes across countries, analogous to this paper's focus on variation across U.S. states. They find that antitrust policies reduce firms' complaints about anticompetitive obstacles (Mayo and Schiffer 2006); increase productivity growth (Buccirossi et al. 2013); induce firms to raise investment and external financing in response to greater competition (Dasgupta and Žaldokas 2019); reduce profit margins but encourage mergers as a strategic response to enforcement (Dong, Massa, and Žaldokas 2019); and are effective at constraining market power in nontradable sectors, where competitive pressures from international trade are absent (Besley, Fontana, and Limodio 2021). Another set of papers shows that effective enforcement—not just the adoption of antitrust laws—ultimately shapes outcomes (Bradford, Chilton, and Linos 2025; Hylton and Deng 2007).

<sup>15</sup>The work of Mullin and Snyder (2021) contrasts with the earlier assessment by Burns (1977) that the dissolution of American Tobacco had little effect on the industry's structure or anticompetitive conduct. Mullin and Snyder (2021) do not cite Burns (1977) or discuss why their conclusions differ.

<sup>16</sup>For literature reviews representing opposing perspectives on the effectiveness of antitrust enforcement, see Cran-

that prices often rise following major interventions (Sproul 1993); that enforcement functions as a negative productivity shock (Young and Shughart 2010); and that lax merger policy was not to blame for rising industrial concentration in the late twentieth century (Peltzman 2014). However, other scholars have challenged these conclusions. Bosch and Eckard (1991) show that price-fixing indictments led to substantial equity losses, consistent with the expectation that enforcement would curb monopoly profits, and Shepherd (1982) attributes declining industrial concentration in the mid-twentieth century partly to efficacious antitrust policy. Similarly, Babina et al. (2023) find that federal antitrust cases between 1971 and 2018 increased employment, wages, and business formation, suggesting broad economic benefits. Thus, despite extensive study, no consensus exists on the effectiveness of antitrust enforcement in promoting competition.

Beyond effects on competition, the effect of antitrust policy on innovation has also been a subject of scholarly inquiry, with studies yielding mixed results. A series of studies find that antitrust actions against Bell Labs (Watzinger et al. 2020), the Bell System (Watzinger and Schnitzer 2022), and Microsoft (Thatchenkery and Katila 2023) led to increased innovation—as proxied by patenting behavior—in affected sectors. Likewise, Kwon and Marco (2021) find that antitrust regulation of patent transfers curbs the negative impact of patent consolidation on rivals' innovation. However, other studies complicate this picture. Kang (2025) finds that collusion, which antitrust aims to prevent, raises both the quantity and quality of patents among cartel members. Similarly, Hashmi (2013) reports a mildly negative association between competition and innovation. This unresolved debate highlights the need for further empirical work on when and how antitrust policy affects economic outcomes.

Responding to this need, this paper advances both the literature on historical antitrust enforcement and the broader literature on how antitrust policy affects competition and innovation in several ways. First, unlike previous studies focusing on incremental changes or specific antitrust cases, I examine the overall impact of implementing an antitrust regime where none existed before. This approach allows for a more comprehensive assessment of effects, including potential deterrence, by analyzing broader market outcomes rather than focusing solely on parties involved in particular cases.<sup>17</sup> Second, while many existing studies concentrate on individual industries or a small number of industries, I provide a wider perspective by estimating effects across the entire manufacturing sector. Third, I analyze long-term effects over nearly a century, in contrast to the narrower time windows examined in most of the existing literature. This extended time frame provides a better understanding of the enduring impact of antitrust policy. Fourth, while most existing papers focus on the federal antitrust regime; I instead examine the role of state antitrust policy, allowing me to

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dall and Winston (2003) for a skeptical view and Baker (2003) for a pro-enforcement one.

<sup>17</sup>Antitrust laws influence firm behavior by deterring anticompetitive conduct, even among firms never prosecuted for violations. Just as most taxpayers never face an audit but still comply with tax laws because of the threat of scrutiny, antitrust laws may deter firms from engaging in anticompetitive conduct even if enforcement actions are uncommon. While the direct effects of antitrust enforcement are visible in prosecuted cases, deterrence is harder to observe, which underscores the significance of this paper's ability to estimate these broader, indirect effects.

bring new evidence to bear on this ongoing and important economic debate.

This paper also contributes to the broader literature on collusion and firm behavior in the historical United States. A central theme in this literature is that collusion can be both unstable and highly adaptive to institutional and market conditions. Porter (1983) highlights the fragility of cartel arrangements, showing that the nineteenth-century railroad cartel known as the Joint Executive Committee repeatedly broke down in response to unexpected shocks. In other cases, collusion has proven more durable. Alexander (1994), Chicu, Vickers, and Ziebarth (2013), and Vickers and Ziebarth (2014) show that the National Industrial Recovery Act enabled widespread and sustained collusive behavior across multiple industries, with firms reducing price responsiveness to cost changes and ceasing competitive price adjustments altogether. Another strand of the literature examines how collusion interacts with investment and entry. Gross (2020) shows that coordination among Southern railroads facilitated rapid technological standardization, but cartel discipline prevented efficiency gains from reaching consumers. Similarly, Syverson (2024) finds that railroads used strategic duplication to deter entry and temporarily raise prices for grain sellers, but this came at the cost of wasteful overinvestment. Finally, Huang (2025) shows that the Beef Trust manipulated price signals by temporarily raising cattle prices to attract supply, then lowering them, causing more harm to sellers than standard monopsony would predict. Together, these studies underscore that collusive behavior is not only shaped by its legal environment, but can also distort investment, entry, and the information available to market participants.

Finally, this paper builds on work by economic and legal historians, who have written about how states shaped the competitive landscape during the late nineteenth and early twentieth centuries. Most papers in this literature show that state enforcement actions, including lawsuits by attorneys general and shareholders, effectively curtailed anticompetitive conduct during this period (Clay and Troesken 2002; Lamoreaux 2023; Lamoreaux and Phillips-Sawyer 2021; May 1987; Nolette 2012; Phillips-Sawyer 2018; Pratt 1980).<sup>18</sup> I build upon and extend this literature in several important ways. First, I bring quasi-experimental evidence to bear on the effects of state antitrust laws, focusing on the identification of causal effects. Furthermore, unlike much of the existing scholarship that concentrates on legal outcomes such as individual enforcement actions and cases won against trusts in court, I measure “success” by analyzing the extent to which antitrust laws had a discernible effect on important economic indicators. Finally, by comprehensively tracing the evolution of state antitrust laws over time, I offer a more detailed description of state antitrust law in its formative era than previously available in the literature. These contributions allow me to present new evidence on how state antitrust laws shaped economic outcomes during the formative era of U.S. competition policy.

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<sup>18</sup>Two papers by Werner Troesken offer a perspective that differs from this prevailing view. In a study of the 1889 dissolution of the Chicago Gas Trust, Troesken (1995) shows that the breakup, carried out through a *quo warranto* proceeding under corporate law rather than antitrust law, had little effect on market structure or performance. Similarly, Troesken (1998) argues that competition—not state antitrust enforcement—disciplined the Whiskey Trust.

## 2.3 Theoretical Predictions

Before turning to the empirical analysis, it is helpful to consider what standard economic theories suggest about the expected effects of state antitrust laws on the outcomes examined in this paper. Because early antitrust policy primarily targeted cartels and other horizontal restraints, state antitrust laws can be understood as interventions designed to shift markets from monopolistic or oligopolistic structures toward more competitive ones. Accordingly, they are expected *ex-ante* to reduce—or at least not increase—firms’ profits.<sup>19</sup> If a new state antitrust law meaningfully curtails ongoing anticompetitive behavior or deters new conduct from emerging, markets may shift away from monopoly or oligopoly—where profits are positive—towards perfect competition—where economic profits are zero under standard assumptions. From a welfare perspective, while profits may decline, surplus may be reallocated from producers to consumers, and aggregate surplus may increase. *Ceteris paribus*, increased competition should reduce firms’ profits. However, if antitrust laws also enhance efficiency or spur innovation, profits could rise even as market power declines. Further, if enforcement is weak or firms perceive little risk of detection and punishment—or if little anticompetitive conduct existed in the first place—profits may remain unchanged.

Greater competition may also lead to more firms operating in a market. A sizable literature in industrial organization documents a positive relationship between the number of firms and competition (Bresnahan and Reiss 1991; Melitz and Ottaviano 2008; Syverson 2004; Tirole 1988). Standard Cournot models of oligopolistic competition, which show that price converges to marginal cost as the number of firms tends towards infinity, reinforce this view. Additionally, antitrust jurisprudence tends to view a larger number of firms in a market as a sign of increased competition (Hovenkamp and Areeda 2022). Thus, to the extent that state antitrust laws increased competition by weakening the market power of large firms, one would expect them to also increase the number of firms as new entrants were able to more easily set up shop. One important caveat to this theory is that most state antitrust laws explicitly prohibited horizontal price-fixing arrangements but did not affect firms’ ability to merge with each other. Thus, if firms responded to a new antitrust law in their state by merging with competitors to avoid prosecution for cartel behavior, state antitrust laws may have reduced the number of firms.<sup>20</sup> My ability to test this theory is partially limited by the fact that the census of manufactures reports establishment counts rather than firm counts. While a firm refers to a distinct business entity, an establishment refers to a single physical location where manufacturing takes place; a single firm can operate multiple establishments. Thus, any observed increase in the number of establishments could stem from new firm entry or growth by existing firms, a distinction that cannot be directly observed in the data.

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<sup>19</sup>Similarly, they are expected *ex-ante* to reduce or leave unchanged the retail prices consumers pay. Though not the focus of this paper, I examine effects on food retail prices in Appendix C.

<sup>20</sup>Federal lawmakers similarly declined to limit firms’ ability to consolidate with competitors when they passed the Sherman Act in 1890. Lamoreaux (1985) and Bittlingmayer (1985) provide evidence that the wave of mergers that occurred around the turn of the twentieth century was partially spurred by the Sherman Act.

Beyond market structure, economic theory also predicts that increased competition from antitrust policy should affect production and labor demand. Standard models predict that the implementation of antitrust laws should lead to higher employment by lifting artificial constraints on output imposed by monopolistic or collusive firms. This prediction reflects a well-established result in industrial organization, which holds that collusion and anticompetitive mergers suppress output (Asker and Nocke 2021; Whinston 2006). In markets where firms had previously restricted output to maintain high prices, stronger antitrust enforcement can foster greater competition, pushing firms to expand production in response to lower prices and increased market demand. As output rises, so too does the demand for production inputs, including labor, resulting in higher employment levels.

A further dimension of firm behavior potentially affected by antitrust policy is innovation. The adoption of an antitrust statute may increase or decrease a firm's incentive to innovate. One hand, when firms can no longer rely on monopoly power or collusive arrangements to sustain profits, they face stronger pressure to improve their products and processes to stay ahead of rivals. Antitrust policy can also reduce barriers to entry, enabling new firms to challenge incumbents and spurring industry-wide innovation. In such an environment, innovation becomes a key strategy for differentiation and survival.<sup>21</sup> On the other hand, monopolies may foster innovation by investing their substantial resources in research and development (Hashmi 2013; Schumpeter 1942). A possible reconciliation of these two theories posits that the relationship between product market competition and innovation follows an inverted-U shape—that is, innovation increases with competition at low levels of competition, but decreases with competition at high levels (Aghion et al. 2005). According to this view, innovation rises with competition at low levels because closely matched firms are motivated to innovate to gain market advantage, but it falls at high levels of competition because profit margins become too thin to justify the cost of innovation. Overall, these predictions show that antitrust laws' implications for several of the outcomes I study in this paper are theoretically uncertain, underscoring the need for empirical analysis.

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<sup>21</sup>A twenty-first-century observer might expect the adoption of a state antitrust law to make firms more cautious in their patenting behavior, encouraging a focus on genuine innovation rather than the strategic use of patents for market control. This expectation reflects a modern legal landscape in which tensions between patent and antitrust law are well recognized, particularly when patents are used to exclude rivals or entrench market power. However, early twentieth-century courts generally treated patents as government-sanctioned monopolies that were immune from antitrust scrutiny (Jacobson 2018). As a result, a reduction in patenting motivated by legal risk is unlikely to have materialized in the period I study.

### 3 Data Sources and Descriptive Statistics

#### *State Antitrust Laws*

I created a historical compendium of state antitrust statutes to trace the development of state antitrust law from 1888 (when the first state antitrust law was enacted) to 1940. My database includes statutes from every U.S. state except Alaska and Hawaii, which were not yet states during the study period, allowing for a comprehensive view of subnational competition law across the continental United States. To create this database, I reviewed state session laws and legal codes for each year from 1860 through 1940 and identified statutes outlawing anticompetitive conduct.<sup>22</sup> In total, I identified 161 statutes enacted between 1888 and 1940. This set of statutes includes amendments, which allows me to characterize the evolution of state antitrust regimes over the study period. To my knowledge, no other database documents both the initial enactment and subsequent evolution of state antitrust laws over the late nineteenth and early twentieth centuries. I do not regard statutes applying to a single industry, rather than to commerce more generally, as antitrust statutes in this paper.<sup>23</sup> I find that Iowa became the first state to adopt an antitrust statute in 1888.<sup>24</sup> I also compared my database to contemporaneous compilations of state antitrust laws to ensure accuracy.<sup>25</sup> After identifying state antitrust statutes in effect between 1860 and 1940, I analyzed the content of states' laws. Specifically, I coded the specific activities outlawed; whether the law made violations a crime, a tort, or both; the minimum and maximum fines and prison sentences authorized; the civil damages authorized, if any; and any other penalties authorized. Appendix B provides more information on how I identified and coded these laws.

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<sup>22</sup>I considered any state statute outlawing restraints against trade, monopolization, horizontal price fixing, horizontal output restrictions, trustee control of corporations, anticompetitive stock purchases, refusals to deal, or some combination thereof to be a state antitrust statute. See table B1 for definitions of these acts. I began my review with materials from 1860 onward to verify that no relevant statutes had been enacted prior to 1888.

<sup>23</sup>I exclude industry-specific antitrust statutes from my analysis because these laws were limited in scope, and the focus of this paper is to study the broad economic impacts of state antitrust laws. Moreover, applying a uniform identification strategy to statutes targeting very different industries would likely be challenging. For example, an 1897 Florida statute applied antitrust principles to the sale of beef or “other edible animal[s],” while several states, such as Georgia in 1891, passed laws restricting anticompetitive behavior in the business of insurance only. I do not include these statutes in my database of state antitrust laws. Act of June 11, 1897, Chapter 4534, 1897 Florida Acts 60. Act of October 21, 1891, Chapter 745, 1891 Georgia Acts 206.

<sup>24</sup>Previous scholars have reached different conclusions about which state laws should be considered state antitrust statutes. For example, Millon (1990) considers an 1889 Kansas statute to be the United States' first antitrust statute. I consider the 1889 Kansas law to be Kansas' first antitrust statute but recognize Iowa's 1888 law directed at “the punishment of pools, trusts and conspiracies” as being first in the nation. Any time I encountered an accounting of state antitrust statutes that disagreed with my own, I reviewed the differences to confirm the accuracy of my analysis. Act of March 9, 1889, Chapter 257, 1889 Kansas Acts 389. Act of April 26, 1888, Chapter 84, 1888 Iowa Acts 124.

<sup>25</sup>Sources I used to check my work include Forrest (1896), Halle (1899), Bureau of Corporations (1915), and Works Progress Administration (1940). I also compared my compilation of state antitrust statutes to internal memoranda from the now-defunct Bureau of Corporations. The Bureau of Corporations was a government agency that existed from 1903 to 1915, had broad authority to conduct investigations and gather data on the operations of interstate corporations, and predated the Federal Trade Commission. Given the Bureau's focus on investigating large corporations and trusts, I identified a number of internal agency documents relating to state and federal antitrust laws during a visit to the National Archives. These documents helped me ensure the accuracy of my state antitrust database.

State antitrust statutes evolved along diverse paths but frequently converged around a few key regulatory priorities. Figure 4 traces the enactment and subsequent trajectories of state antitrust statutes during the study period, illustrating that nearly every state revised its statute at least once. Although the content of state antitrust laws varied significantly from state to state, some trends emerge. For example, as Figure 6 shows, state antitrust laws most commonly prohibited horizontal arrangements to fix prices or limit quantity. Such provisions were aimed squarely at dismantling cartels, which were the main target of early antitrust law. Several states also included provisions mirroring section 1 of the Sherman Act, which broadly prohibits restraints of trade, in their antitrust statutes. Provisions mirroring section 2 of the Sherman Act, which condemns monopolization, were somewhat less common at the state level, particularly in the first several decades states began adopting antitrust statutes. Several states also prohibited the sale of trust certificates, which were commonly used to coordinate horizontal restraints starting in the late nineteenth century. Few states adopted provisions barring firms from buying their competitors' stock or refusing to do business with buyers or sellers that also transacted with a competitor.

States also varied in how they enforced antitrust violations, with earlier adopters favoring criminal sanctions and later adopters incorporating civil remedies. As Figure 7 shows, states that adopted antitrust laws early were more likely to classify violations as criminal offenses, punishable by fines or imprisonment.<sup>26</sup> In contrast, when antitrust violations were treated as torts, injured parties could seek compensation through private lawsuits. Civil remedies shifted the burden of enforcement away from the state and toward private actors, potentially expanding the reach of antitrust law. Perhaps for this reason, later adopters tended to include means of both civil and criminal recourse in their statutes. Further, as civil enforcement gained traction, states began to implement remedies that ranged from compensation for actual harm to more punitive measures aimed at deterrence. As seen in Figure 8, the first crop of states that allowed parties injured by antitrust violations to sue for damages tended to limit damages to the amount of actual losses incurred. Enhanced remedies—such as double or treble damages—became more popular in the early 1900s, providing greater financial incentives for private enforcement.

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<sup>26</sup>For states in which antitrust violations were criminal offenses, Figure A9 illustrates the fines and prison sentences authorized under state antitrust statutes between 1888 and 1940. An important caveat is that Figure A9 illustrates the *statutory* minima and maxima for fines and prison sentences. Systematic data on the fines and prison sentences actually imposed under state antitrust laws are not available. However, contemporaneous press coverage suggests that sizable fines were not uncommon, and that at least some individuals were sentenced to jail for antitrust violations. As Figure A9 shows, the median maximum fine was \$5,000 among states with antitrust statutes during the study period. (According to historical inflation data, \$5,000 in 1890 dollars amounts to approximately \$174,981 in 2024 dollars (Federal Reserve Bank of Minneapolis n.d.).) Moreover, many statutes considered each day a party was in violation of statute to be a separate offense. The median minimum fine hovered around \$0 during the study period; that is, the average state did not set a minimum fine for antitrust violations in its antitrust statute. Moreover, as Figure A9 shows, the median maximum prison sentence was 12 months during most years between 1888 and 1840. The year 1897 provides a notable exception; the median maximum prison sentence jumped to 24 months that year when three states (Arkansas, Indiana, and South Carolina) enacted antitrust statutes imposing 10-year maximum prison sentences. As with fines, the median minimum prison sentence also hovered around \$0 during the study period, implying that the average state did not set a minimum prison sentence for antitrust violations.

### *State-by-Industry Manufacturing Outcomes*

To assess the impact of state antitrust laws on competition within the manufacturing sector, I use newly digitized state-by-industry tabulations from the census of manufactures (Barkai, Karger, and Schaller 2025).<sup>27</sup> These data cover the full scope of manufacturing activity in the United States and represent the most comprehensive data source on manufacturing for this period that is disaggregated by both geography and industry. The data span each of the 20 manufacturing censuses that took place between 1850 and 1940, allowing for a systematic examination of long-run trends in manufacturing activity across states and industries.<sup>28</sup> The main outcomes I examine are the number of manufacturing establishments, as well as the profits and labor utilization of these establishments. To obtain profits per county-by-industry cell, I compute

$$\pi_{ict} = Y_{ict} - M_{ict} - W_{ict} \quad (1)$$

where  $i$  indexes an industry,  $c$  indexes a county, and  $t$  indexes the year. Revenue, the value of materials (i.e., inputs), and the total wage bill are denoted by  $Y_{ict}$ ,  $M_{ict}$ , and  $W_{ict}$ , respectively. Of course, this approach yields a measure of *accounting* profits rather than *economic* profits. Accounting profits imperfectly proxy real economic returns (Fisher and McGowan 1983). However, the aggregated census of manufactures data I employ in this paper leave me with few feasible alternatives.

Though the census of manufactures provides valuable state-by-industry data, it is not without its limitations. One challenge associated with these data is that the census of manufactures did not include industry codes until 1947, when it adopted the Standard Industrial Classification (SIC) system. Accordingly, I use the industry descriptions included in the census of manufactures tabulations to map industries to their corresponding four-digit SIC codes.<sup>29</sup> To do this, I use the detailed narratives explaining each four-digit SIC code in a contemporaneous version of the *Standard Industrial Classification Manual* (Technical Committee on Industrial Classification 1945). In addition, some censuses report only the number of workers—that is, production laborers such as operatives, “hands,” and wage earners—rather than the total number of employees, which would also include salaried staff like managers, superintendents, and clerks. Because comprehensive employee counts do not appear consistently across censuses, I instead rely on the number of workers throughout the analysis.

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<sup>27</sup>Establishment-level schedules from the census of manufactures are presumed lost for all years after 1880 and prior to 1929. As Vickers and Ziebarth (2019, p. 1705) describe, “the underlying schedules [from censuses of manufactures taken between 1880 and 1929] have been lost due to accidents such as the fire that destroyed much of the 1890 Population Census, bureaucratic neglect, or active destruction to conserve space at the National Archives.” As a result, I use aggregated tabulations that appeared in published census volumes for this research. Without establishment- or firm-level data, some outcomes of interest—such as the Herfindahl-Hirschman Index of industry concentration—are not possible to compute.

<sup>28</sup>Figure A4 shows these years and the number of states included in the state-by-industry data each year.

<sup>29</sup>As Pittman and Werden (1990) show, four-digit SIC codes often diverge from the product market definitions used in antitrust enforcement. However, detailed case-specific market definitions are rarely available across a wide range of industries, making four-digit SIC codes the most practical and consistent alternative.

### *Establishment-Level Manufacturing Outcomes*

I draw on the surviving schedules from the 1850–1880 censuses of manufactures to measure concentration in the manufacturing sector prior to the rise of the antitrust movement (Hornbeck et al. 2024). Because establishment-level schedules from the census of manufactures are presumed lost for all years after 1880 and before 1929, the surviving schedules from 1850 to 1880 provide the last comprehensive, nationwide establishment-level data on U.S. manufacturing prior to 1929. The surviving manuscripts contain nearly 700,000 establishment-year observations across 45 states and the District of Columbia.<sup>30</sup> I match the industries in the data to their four-digit SIC codes and compute Herfindahl–Hirschman Index (HHI) measures based on each establishment’s share of total industry output in a given year. A key limitation of these data is the absence of firm identifiers, which prevents aggregation of establishments under common ownership. As a result, these establishment-based HHIs may underestimate true market concentration, especially in cases where many small establishments are in fact owned by a single firm. While a small number of large establishments correctly indicates high concentration, a diffuse structure of small establishments need not imply a competitive market if ownership is consolidated. Despite this limitation, the data provide a useful—if imperfect—proxy for local industrial concentration. I use these measures to examine whether the effects of state antitrust laws varied with pre-existing levels of concentration.

### *Trusts*

To identify and characterize major industrial combinations active around the turn of the twentieth century, I rely on *The Truth About the Trusts*, which documents the structure, capitalization, and constituent firms of trusts in the United States at the height of the merger wave (Moody 1904).<sup>31</sup> This source provides detailed information on 445 trusts, which I use to infer each trust’s primary industry and assign a four-digit SIC code to enable linkage with other historical economic data. *The Truth About the Trusts* has been used in major works such as those by Nutter and Einhorn (1969) and Lamoreaux (1985) and primarily draws on *Moody’s Manual of Corporation Securities*, supplemented by the *Commercial & Financial Chronicle*, the *Boston News Bureau*, the *Wall Street Journal*, and other sources (Moody 1904, p. xxii). Subsequent research shows that *The Truth About the Trusts* also relies on the *Manual of Statistics: Stock Exchange Handbook* (Bunting 1971). In keeping with contemporaneous usage of the word “trust,” Moody (1904, p. xiii) defines a trust not by its legal form but by its perceived market power; the definition encompasses any business combination believed to have the power, intent, or tendency to monopolize, restrict trade, or influence prices.

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<sup>30</sup>Of the contiguous 48 states, no data are available for Arizona, Oklahoma, and South Dakota.

<sup>31</sup>I digitized Table 1 in section VI, *Classified Statistics of All Trusts*, to create a firm-level dataset on major industrial trusts (Moody 1904, pp. 453–477). Trusts are designated as “greater industrial trusts,” “lesser industrial trusts,” “important industrial trusts in process of reorganization or readjustment,” “leading franchise trusts” (which are comprised of “telephone and telegraph consolidations” and “electric light, railway, and gas companies”), “the great steam railroad groups,” or “allied independent’ steam railroad systems.”

Figure 9 shows the distribution of capital and establishments by sector for the trusts cataloged in Moody (1904). Panel A, which covers all 445 trusts listed in Moody (1904), highlights the dominance of transportation and utilities in capital terms. Panel B hones in on the 305 entities designated by Moody as “industrial” trusts. Industrial trusts were heavily concentrated in manufacturing, which motivates my focus on that sector in this paper. The scale of trusts’ economic footprint is striking; in 1904, four-digit SIC industries with a trust generated 59 percent of total manufacturing revenue. This fact highlights the outsized role that industrial trusts played in the early twentieth-century economy.

### ***Stock Returns***

To understand how the stock market reacted to the passage of state antitrust laws, I use data on daily stock returns to the Dow Jones composite portfolio (Schwert 1990). The dataset covers 1885 to 1962 and reports a daily time series over this period, capturing average returns, capital gains, and dividend yields for the composite portfolio. The index is composed of 12 to 50 industrial and transportation stocks. While the index’s composition changed over time, it consistently aimed to include the most prominent stocks in terms of trading volume and market capitalization. The resulting data thus enable analysis of market-level reactions to changes in antitrust policy. Because stock prices reflect investors’ expectations about firms’ future earnings, stock market responses to the passage of state antitrust statutes can provide insight into whether these laws were anticipated to constrain or enhance profitability.

### ***Patents***

I use Comprehensive Universe of U.S. Patents (CUSP) data to identify the effects of state antitrust laws on innovation (Berkes 2018). CUSP data cover all U.S. patents issued between 1836 and 2016, allowing for longitudinal analysis at the inventor, assignee, or geographic level. The data provide a rich array of information for each patent, including issue and filing years, technological classes, backward and forward citations, and detailed inventor and assignee data, including names and geolocated addresses. CUSP improves upon existing historical patent datasets by offering a more comprehensive time range, a wider set of variables, and more detailed geographical information. Berkes (2018) collected the data using multiple sources, including records from the U.S. Patent and Trade Office (USPTO), state-level databases, and digitized patent documents. The accuracy and coverage of CUSP data have been validated through comparisons with official USPTO records and other historical patent datasets, such as HistPat (Petralia, Balland, and Rigby 2016).

### ***Newspaper Articles on State Antitrust Topics***

I assembled a collection of 118,560 historical newspaper articles that mention one or more state antitrust laws during the study period.<sup>32</sup> To identify newspaper articles mentioning state antitrust

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<sup>32</sup>These articles come from two digital newspaper archives. The first is the *Chronicling America* project established

laws, I searched for articles that (1) contain either `antitrust` or `anti-trust`, (2) contain a state name or contain the title of a named state antitrust law (i.e., `cartwright` in reference to California's Cartwright Act, `donnelly` in reference to New York's Donnelly Act, or `valentine` in reference to Ohio's Valentine Act), and (3) were published between 1850 and 1940. Figure 5 plots the number of articles in my dataset by year of publication and by the treatment status of the state mentioned in the article.<sup>33</sup> The figure corroborates that the collection of newspaper articles I assembled generally reflects discussion of state antitrust laws. Because never-treated states never passed a statute, journalists in those states had fewer reasons to cover antitrust-related topics than their counterparts in states that eventually adopted such laws.<sup>34</sup> Accordingly, these states have very few mentions in the collection of newspaper articles I assembled, particularly relative to ever-treated states. These articles allow me to validate that the passage of a statute was publicly visible—that is, people in treated states were likely aware of the law's existence. The content of these articles also allow me to understand how state antitrust laws were enforced and perceived by contemporaneous observers.

### **Controls**

To account for potential confounding demographic and economic conditions, I control for the population, literacy rate, median occupational score, and state-level personal income in state  $s$  and year  $t$  across specifications. These variables are constructed from full-count U.S. population census microdata from 1850 to 1940 (Ruggles et al. 2021) and a composite series on state personal income drawn from four sources (Bureau of Economic Analysis 2025; Easterlin 1960; Easterlin 1957; Klein 2009).<sup>35</sup> Because the personal income and census-based variables are observed in different years, I linearly interpolate values between data points to generate consistent annual controls. Population captures local market size, literacy proxies human capital, and occupational score—a widely used metric in historical research—serves as a proxy for socioeconomic status in the absence of individual income data. However, occupational score may reflect social prestige or long-run status more than current economic conditions. Thus, to more directly account for contemporaneous state-level economic performance, I also control for personal income. In robustness checks, I control for a number of additional variables to rule out potential confounders. The first of these is the

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by the Library of Congress, and the second is the ProQuest Historical Newspapers database. Together, these sources' rich holdings comprise over 73 million digitized pages. *Chronicling America* includes about 21 million pages from 4,051 newspapers, many of which are small-town publications (Library of Congress 2024). ProQuest, meanwhile, includes about 52 million pages from 39 major newspapers (ProQuest 2024). Just three newspapers appear in both datasets; I remove duplicate articles from these newspapers to ensure no publication is over-represented. Combining articles from these two databases provides comprehensive coverage of newspaper media during the study period.

<sup>33</sup>Figure A5 provides several examples of articles that appear in my dataset.

<sup>34</sup>Articles mentioning antitrust law in never-treated states may include, for example, editorials calling on the state's legislature to pass an antitrust statute. Some false positives are likely also included in the dataset and may reflect mentions of *federal* antitrust enforcement within never-treated states (and ever-treated states). To address this issue, I attempt to identify and remove articles containing words and names (such as the names of U.S. attorneys general) that likely reflected federal actions.

<sup>35</sup>Figure A6 shows how I pieced these sources together over time.

share of employment in agriculture, given the agrarian roots of the antitrust movement. Second, information on whether the state's constitution included an anti-monopoly provision helps to fully account for the legal environment vis-à-vis competition policy during the study period.<sup>36</sup> Third, using data from Barkai, Karger, and Schaller (2025), I control for antitrust cases initiated by the U.S. Department of Justice during the study period to ensure that observed effects are not driven by federal enforcement actions.

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<sup>36</sup>I searched a database of historical state constitutions for keywords related to competition policy to identify all anti-monopoly provisions in state constitutions from 1776 (when the first such provision was ratified) through 1940. Figure A7 illustrates states' adoption of anti-monopoly constitutional provisions, and Figure A12 illustrates the contents of states' anti-monopoly constitutional provisions. These provisions were short (63 words on average) and typically quite general in nature. Most frequently, these provisions would simply direct the state legislature to pass laws to regulate monopolies. They would also commonly decry the existence of monopolies or declare combinations, trusts, and/or other restraints of trade to be illegal. These provisions made restraints of trade neither criminal nor tortious, and designating a state officer, such as the Attorney General, to enforce a constitutional provision against monopolies, was relatively rare.

## 4 Identification Strategy

The following two-way fixed effects model provides a framework for identifying the effects of state antitrust laws on manufacturing outcomes. Specifically, I base my analysis on:

$$y_{ist} = \beta_0 + \beta_1(statelaw_s \times post_{st}) + \Gamma X_{st} + \delta_s + \delta_t + \delta_{is} + \delta_{it} + \varepsilon_{ist} \quad (2)$$

where  $i$  denotes an industry,  $s$  denotes a state, and  $t$  denotes the year. The model includes state fixed effects  $\delta_s$  and year fixed effects  $\delta_t$ , as well as state-by-industry fixed effects  $\delta_{is}$ , industry-by-year fixed effects  $\delta_{it}$ , and a vector of time-varying state-level controls  $X_{st}$  in most specifications. The variable  $y_{ist}$  denotes the outcome,  $statelaw_s$  indicates whether state  $s$  adopted an antitrust law, and  $post_{st}$  indicates the time period after which a state antitrust law was adopted.<sup>37</sup> Because no antitrust statutes existed in the United States prior to 1888, this approach allows for comparison to a period without any antitrust statutes, which papers studying antitrust in modern settings are unable to do.<sup>38</sup> Throughout the analysis, I cluster standard errors by state to correct for serial correlation and because treatment occurs at the state level. Because the timing of effects is of interest, I also employ event study models in this paper. The two-way fixed effects version of the event study model I estimate is given by:

$$y_{ist} = \sum_{j \neq 0} \beta_j 1[t = j] \times statelaw_s + \Gamma X_{st} + \delta_s + \delta_t + \delta_{is} + \delta_{it} + \varepsilon_{ist} \quad (3)$$

where variables are defined as above.

Recent research in econometrics has documented that two-way fixed effects specifications may produce biased estimates of average treatment effects in difference-in-differences and event study models when treatment timing is staggered and treatment effects may be heterogeneous across groups or over time (Borusyak, Jaravel, and Spiess 2024; Callaway and Sant'Anna 2021; Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021). Given that states adopted antitrust statutes at different times, this concern is relevant in the present context. As a result, I estimate average treatment effects using a stacked difference-in-differences design that accommodates variation in treatment timing across states (Baker, Larcker, and Wang 2022; Cengiz et al. 2019; Deshpande and Li 2019; Gormley and Matsa 2011). This approach mitigates the bias from negative weighting and treatment effect heterogeneity, yielding more credible estimates of the impact of state antitrust laws. Two-way fixed effects estimates produce similar results.

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<sup>37</sup>As illustrated by Figure 4, several states' antitrust laws were overturned by a court ruling or repealed by a legislative act during the period I study. Several state legislatures also repealed their states' antitrust laws during the study period. To avoid treating these states in the same manner as states that never adopted any antitrust statute, I exclude states whose antitrust laws were overturned or repealed from my analysis.

<sup>38</sup>Though Iowa became the first state to adopt an antitrust statute in 1888, courts had long declined to enforce contracts in restraint of trade prior by 1888. Antitrust statutes supplemented these common law principles by making restraints against trade crimes (and often torts as well).

To implement this methodology, I organize my data into stacks based on year-of-enactment cohort. That is, each stack consists of states that adopted their antitrust statute at the same time. For any given stack, both never-treated and not-yet-treated states serve as controls. I then append the stacks together and estimate the following equation:

$$y_{aist} = \beta_0 + \beta_1(statelaw_s \times post_{st}) + \Gamma X_{st} + \delta_{as} + \delta_{at} + \delta_{is} + \delta_{it} + \varepsilon_{aist} \quad (4)$$

where  $a$  denotes a stack and other variables are defined as above. Equation 4 includes several sets of fixed effects to account for time-invariant and time-varying unobserved heterogeneity. The stack-by-state fixed effects  $\delta_{as}$  control for permanent differences across states within a given stack, such as differences in state-level economic structure, political preferences, legal traditions, or enforcement capacity that persist over time. The stack-by-year fixed effects  $\delta_{at}$  absorb year-specific shocks that are common to all units within a stack, such as macroeconomic conditions. The state-by-industry fixed effects  $\delta_{is}$  account for persistent differences in industry structure or other industry-specific economic characteristics across states, while the industry-by-year fixed effects  $\delta_{it}$  control for time-varying shocks that affect specific industries nationally, such as technological change or national demand fluctuations. Together, these fixed effects allow me to isolate treatment effects by leveraging within-stack variation while controlling for a rich set of potential confounders. Throughout the paper, I weight observations by both the inverse frequency of the census of manufactures and revenue. Specifically, observations from decennial censuses receive a weight of 1, those from quinquennial censuses receive a weight of 0.5, and those from biennial censuses receive a weight of 0.2. I then weight observations by their share of manufacturing revenue in the observed census year.<sup>39</sup>

The key identification assumption underlying the difference-in-differences models I employ is that, in the absence of treatment, treated state-by-industry cells would have followed similar trends as control state-by-industry cells within the same industry and year. This assumption is made conditional on state-by-industry and industry-by-year fixed effects. The assumption is most credible if unobserved shocks affecting treated and control units within the same industry evolve similarly over time. While this assumption is fundamentally untestable, I provide support for its plausibility in several ways. First, I present event study estimates to assess pre-trends. Second, I show that observable characteristics evolve similarly across treated and control states prior to treatment.<sup>40</sup> Finally, I test the sensitivity of my estimates to alternative specifications and control variables. Together, these strategies increase confidence in the plausibility of the parallel trends assumption.

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<sup>39</sup>This period saw rapid economic growth, so weighting by raw or inflation-adjusted revenue disproportionately emphasizes later years. To mitigate this, I weight each observation by its share of total revenue in its census year.

<sup>40</sup>Table A3 compares pre-treatment characteristics in 1880—the last census before the first state antitrust law was adopted—between states that ever adopted an antitrust law and those that never did. Table A4 compares early- and late-adopting states. These comparisons reveal that treated and control states were broadly similar across a range of demographic, economic, and political characteristics prior to any state adopting an antitrust statute. Although a few differences are statistically significant, most variables show no significant differences. This balance provides suggestive evidence that treated and control states would have followed similar trends in the absence of treatment.

My identification strategy has several limitations that merit discussion. First, manufacturing data are available only intermittently (every two, five, or ten years), which may obscure treatment effects that unfold rapidly following a law's adoption. Second, treatment effects may not be contained within state borders. If firms respond to antitrust laws by relocating or adjusting their behavior in untreated neighboring states, these spillovers could potentially bias estimates towards zero. Third, although my design controls for rich sets of fixed effects and observable covariates, I cannot rule out the possibility of selection into treatment. States that adopted antitrust laws may have differed systematically from those that did not in unobservable ways that also affect manufacturing outcomes. Finally, while this historical setting provides a rare opportunity to study the origins of antitrust policy, the economic principles, case law, and politics of antitrust policy have changed significantly since the late nineteenth century. As a result, my findings may not generalize to modern contexts in which antitrust law is embedded in a more complex regulatory environment.

## 5 Results

The initial enactment and ongoing enforcement of state antitrust laws generated noticeable coverage in contemporaneous newspapers, providing evidence that these statutes were publicly salient and known to economic actors. Figure 10 presents an event study estimating the effect of state antitrust law enactments on the number of newspaper articles discussing state antitrust laws. In the years leading up to enactment, the estimates remain close to zero until roughly five years prior, when mentions begin to rise. This pre-enactment uptick suggests that public discourse may have anticipated the introduction and passage of an antitrust statute. Following enactment, there is a sharp and sustained increase in antitrust mentions, suggesting that these laws received meaningful public attention. This pattern corroborates the idea that individuals and firms were aware of the new statutes and that their economic behavior could plausibly have been influenced in response.

The widespread visibility and public awareness of state antitrust laws raise the question of whether they produced measurable effects on markets. I find that they did. In particular, I find evidence that these laws induced growth in the manufacturing sector by increasing the number of manufacturing establishments. As shown in column (4) of Table 1, the number of establishments rose by approximately 10 percent following the adoption of these laws. Notably, this expansion was concentrated in industries that did not have a trust. In these non-trust industries, the number of establishments increased by roughly 16 percent, while trust-affiliated industries saw no comparable growth. This pattern suggests that state antitrust laws did not curtail trusts' market power enough to allow new entrants to emerge in industries dominated by trusts, but they did foster growth in less concentrated industries. Event study estimates presented in Figure 12 reveal that the establishment growth was relatively steady but intensified over time, indicating that the effects of these laws accumulated gradually rather than generating immediate shifts.

Although it is not fully clear whether the observed growth in manufacturing establishments re-

flects new firm entry or the expansion of existing firms, available evidence points toward the latter. To explore this, I examine changes in ownership patterns—specifically, the number of proprietors per county-by-industry cell. Unfortunately, this variable is only available in a limited set of census years.<sup>41</sup> If incumbent firms expanded by opening new establishments without taking on additional owners, we would expect a decline in the average number of proprietors per establishment. Consistent with this possibility, the estimated effect of antitrust law adoption on the average number of proprietors per establishment is negative across all specifications in Table 1, though statistically insignificant. However, more pronounced effects appear when the sample is restricted to industries without a trust. As shown in Table 2, the number of proprietors per establishment declined by roughly 17 percent in these industries—a magnitude similar to the 16 percent increase in the number of establishments. This pattern suggests that, in sectors not dominated by trusts, incumbent firms may have responded to antitrust statutes by expanding their operational footprints through internal growth, thereby reducing the proprietors-to-establishment ratio. Taken together, these findings offer suggestive evidence that the expansion in non-trust industries was driven not by *de novo* entry but by the extensive-margin expansion of existing firms. However, this interpretation should be viewed with caution given the limited temporal coverage of the ownership data.

Although the number of establishments increased, I find no significant effects on profits or employment, suggesting that incumbent firms were not forced to materially alter their pricing or production strategies in response to state antitrust laws. These null results hold across the full sample (see Panel B and Panel D in Table 1) as well as when disaggregated by trust status (see Panel B and Panel D in Table 2). The stability of profits and employment across specifications indicates that real output likely remained unchanged following the adoption of these laws. This, in turn, implies limited evidence—at least at the aggregate level examined here—that markets transitioned from cartelized to more competitive structures as a result of new antitrust laws. Firms did not hire more workers to meet growing production needs, nor did they experience meaningful declines in profitability. These patterns are difficult to reconcile with a strong competitive shock and instead suggest that firms with market power may have adapted to the new legal environment without substantially changing their behavior. Rather than eroding rents, the laws may have formalized certain boundaries on overtly collusive behavior while allowing existing structures of market power to persist.

Another notable and robust finding is the decline in labor share following the adoption of state antitrust laws. As shown in Panel F of Table 1, the labor share decreased by an average of 4 percent post-enactment. Figure A11 reveals that this decline is driven by increases in revenue outpacing increases in total compensation. Importantly, this pattern is driven by trust-affiliated industries, as

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<sup>41</sup>The relatively small number of observations in panel C of Table 1 reflects the fact that the census of manufactures did not include questions about ownership in all years. Data on the number of proprietors are available in 1900, 1904, 1909, 1914, 1919, 1921, 1923, and 1939. By 1900, most states that would adopt an antitrust law had already done so, so this analysis is underpowered.

demonstrated in Panel F of Table 2. This result calls the effectiveness of early state antitrust laws into question and suggests that the net effect of state antitrust laws may have been to entrench rather than disrupt existing hierarchies. The observed decline in the labor share suggests that capital owners in these industries captured a growing share of output, while labor did not see comparable gains. Rather than reducing the market power of dominant firms, the implementation of these laws appears to have left trust-affiliated firms largely unscathed—and may even have reinforced their position. This interpretation is consistent with the broader pattern of muted responses in profits and employment and highlights the limits of weak policy interventions in curbing entrenched economic power.

The main results are robust to a range of alternative specifications and weighting strategies. As shown in Table 3, the positive effect of antitrust laws on the number of manufacturing establishments and the negative effect on the labor share remain statistically significant across all robustness checks. These include alternative weighting schemes based on the number of establishments (column (1)) and employment (column (2)). In column (3), I also control for the share of agricultural employment given that treated states were more agricultural than untreated states. In column (4), I control for whether the state constitution contained an anti-monopoly provision to account for other features of state competition policy that could have plausibly influenced the results. The consistency of results across these specifications indicates that the findings are not sensitive to modeling choices or to specific features of the data.

Figure 15 breaks down the growth in the number of establishments by industry category. The results indicate that growth in establishments following antitrust adoption was widespread across manufacturing sectors but varied in magnitude. Statistically significant increases are observed in several industries, including paper and allied products, printing and publishing, chemicals, fabricated metal products, machinery, and electrical equipment. These estimates suggest that state antitrust laws promoted entry across a broad set of manufacturing activities, rather than concentrating effects in a narrow subset of industries. A few sectors, such as tobacco products and stone, clay, and glass products, exhibit negative point estimates, though these are imprecisely estimated. The diversity in treatment effects across sectors may reflect differences in preexisting market structure, exposure to competition, or sensitivity to legal constraints. Overall, the figure supports the interpretation that antitrust laws contributed to widespread industrial expansion during the study period.

Figure 15 reveals substantial heterogeneity in the effects of state antitrust laws across manufacturing industries. Across most sectors, the number of establishments increased significantly, consistent with incumbent firms expanding their operational footprints. By contrast, industries that were historically more concentrated—such as tobacco and petroleum—exhibited little to no change in establishment counts, suggesting that dominant firms in these sectors were less affected by state antitrust laws. At the same time, labor share declined across a broad range of industries,

with particularly pronounced reductions in sectors like food products and tobacco products. These patterns suggest that while state antitrust statutes may have prompted expansion in some parts of the manufacturing sector, they largely failed to shift the distributional dynamics of industrial output.

Finally, the enactment of state antitrust laws resulted in an increase in firm-sponsored (assignee) patents. Specifically, firm-sponsored patents increased by about 21 percent in the 40 years following the enactment of an antitrust statute, as shown in Table 4. Notably, event study estimates in Figure 16 show that the increase was relatively small at first, with larger effects emerging gradually rather than immediately. This delayed effect may reflect the time it takes for firms to adjust their behaviors and expectations following a policy change. For example, firms may have waited to see whether the statute would be upheld in court, or whether enforcement would be aggressive, before altering their behavior. The lag is also consistent with a dynamic innovation response. By reducing barriers to entry and increasing the number of manufacturing establishments, antitrust laws may have enabled new entrants to foster greater competition, gradually encouraging innovation and patenting.

## 6 Conclusion

State antitrust statutes emerged amid growing concerns about monopoly power, but their economic impact reveals a more complex legacy. In this paper, I show that state antitrust laws increased the number of manufacturing establishments by about 10 percent, but this growth appears to have been driven by the expansion of incumbent firms rather than new entry. Moreover, this expansion was accompanied by a decline in the labor share, especially in trust-affiliated industries, suggesting that dominant firms retained their market power and captured the gains from expanded activity. I find no evidence of increased employment or reduced profits, indicating that the laws did little to disrupt the market power of dominant firms. These results invite further analysis with more detailed data. As a next step, I plan to investigate how prices for manufactured goods changed in response to the significant growth I document in the manufacturing sector.<sup>42</sup> This analysis would allow me to examine whether increased competition in the manufacturing sector translated to lower prices for consumers purchasing manufactured goods. In addition, I plan to analyze the newspaper articles I assembled in greater detail in future versions of this work. So far, I have only leveraged the number of mentions of each state's antitrust regime. In the future, I plan to employ text analysis methods to better understand the content of each article, which I hope will advance our understanding of the economic effects of early competition policy in the United States.<sup>43</sup>

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<sup>42</sup>I am currently digitizing Aldrich (1892) to support such an analysis.

<sup>43</sup>For example, it would be helpful to classify articles according to the following categories: articles announcing a new antitrust statute, articles reporting on new enforcement actions, articles discussing opposition to an antitrust law, articles reporting on a court case interpreting a state antitrust law or declaring a law (un)constitutional, and articles announcing a problem with a law (such as a lack of enforcement). The newspaper article data I obtained from ProQuest include the first few sentences of the text of each article, as well as the article title and any subtitle(s).

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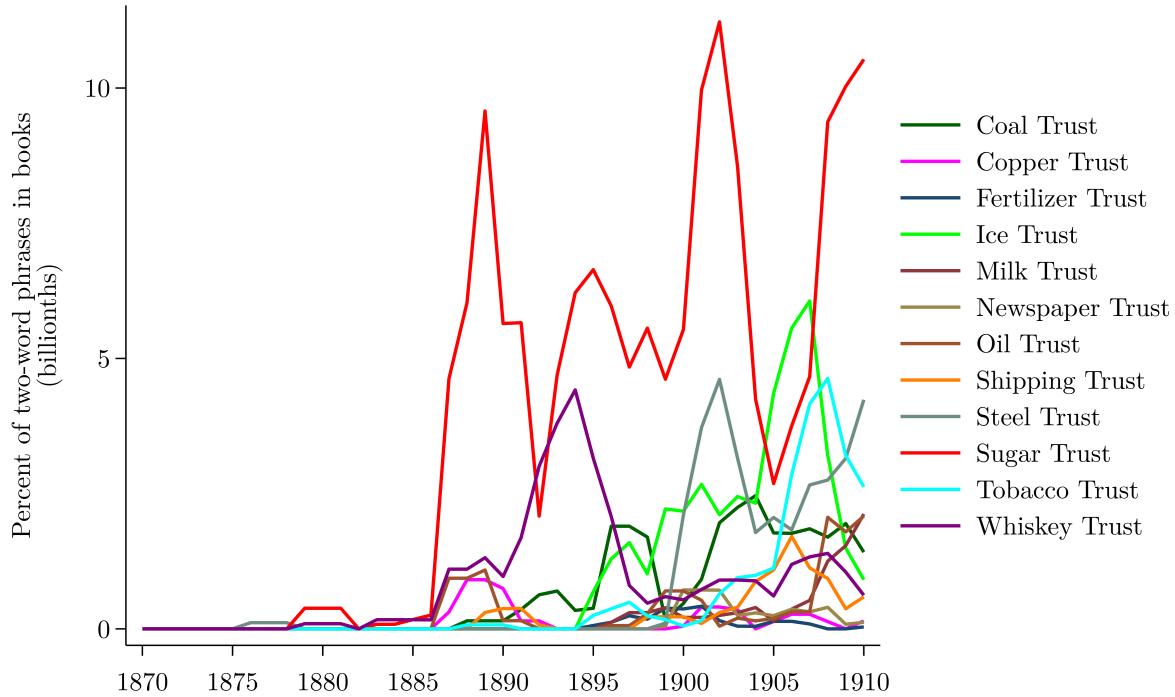
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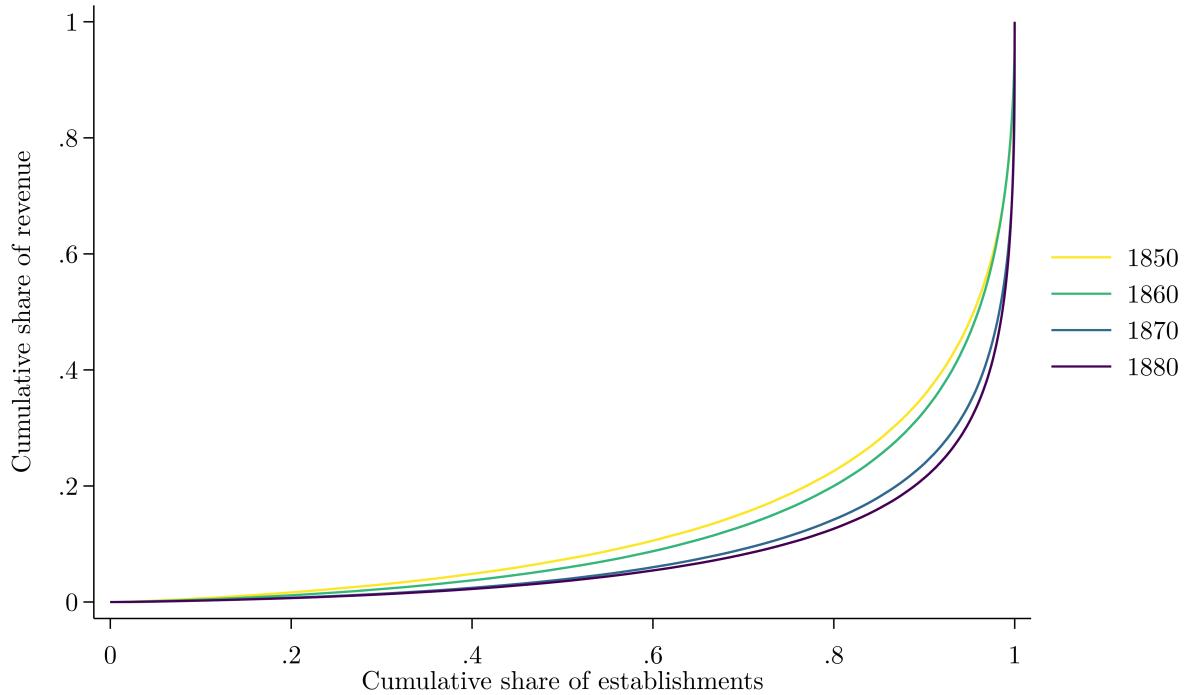
## Main Figures and Tables

Figure 1: Mentions of Well-Known Trusts in the *Google Books Corpus*



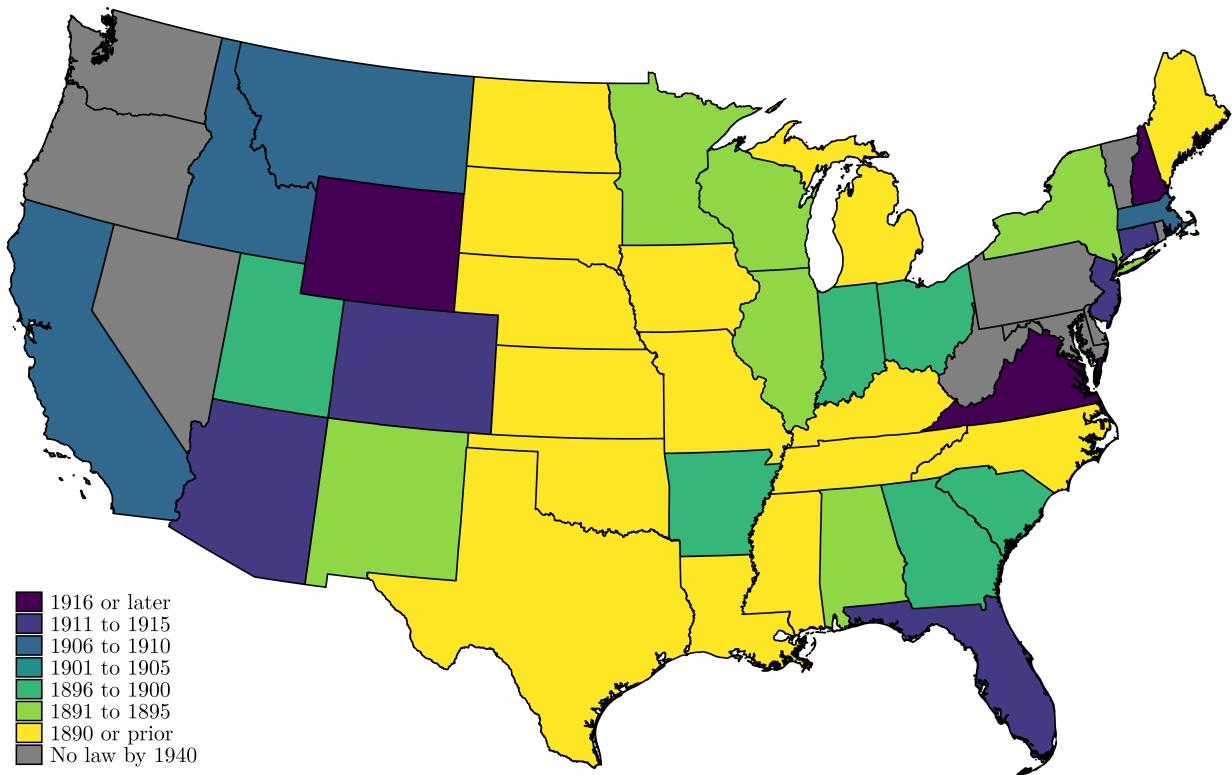
**Notes:** This graph shows the percentage of two-word phrases in the *Google Books* corpus equal to each of the two-word trust names listed in the legend, by publication year. I obtained the underlying data from the Google Ngram Viewer (Michel et al. 2011).

**Figure 2: Concentration of Revenue in Manufacturing, 1850-1880**



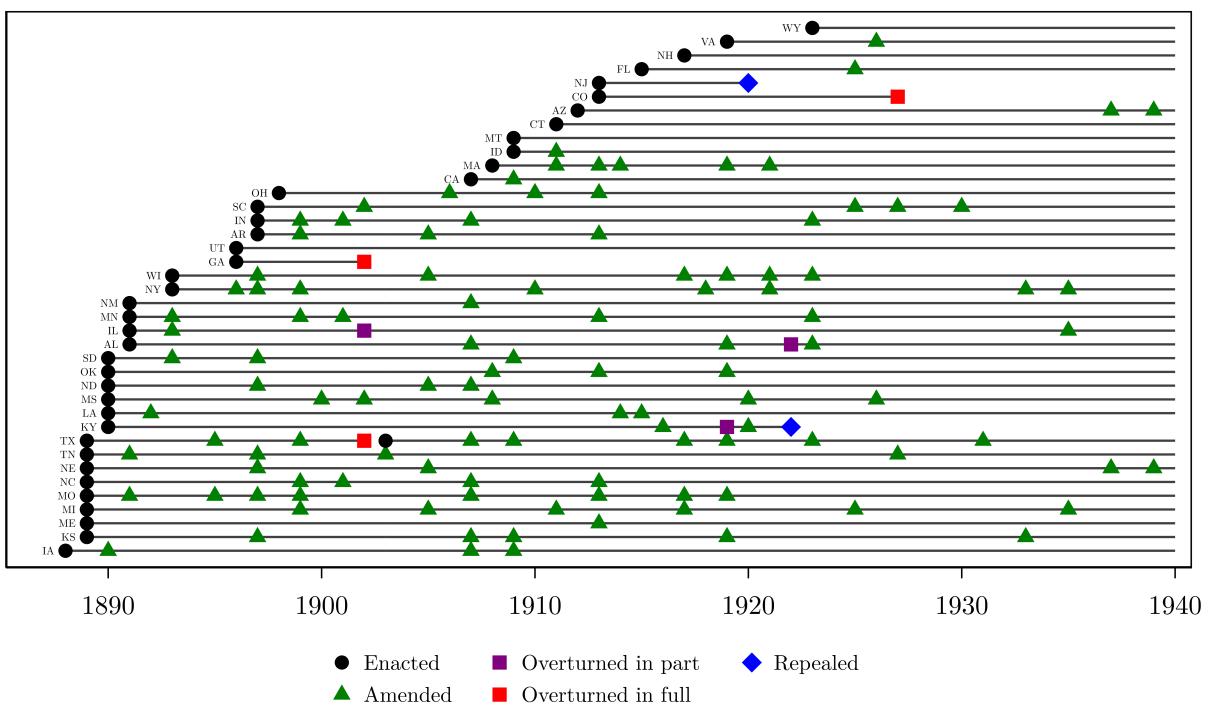
**Notes:** I estimated these Lorenz curves using the surviving schedules from the 1850-1880 censuses of manufactures (Hornbeck et al. 2024). Schedules for 1880 are missing from industries in which special agents were appointed to collect manufacturing data (Atack and Bateman 1999; Delle Donne 1973). These industries are iron and steel, cotton goods, woolen and worsted goods, silk and silk goods, chemical products and salt, coke and glass, shipbuilding, fisheries, all types of mining, coal, and petroleum (Wright 1900, p. 63). For consistency over time, I removed these industries in estimating the Lorenz curves shown above.

**Figure 3: Map of State Antitrust Law Adoption**



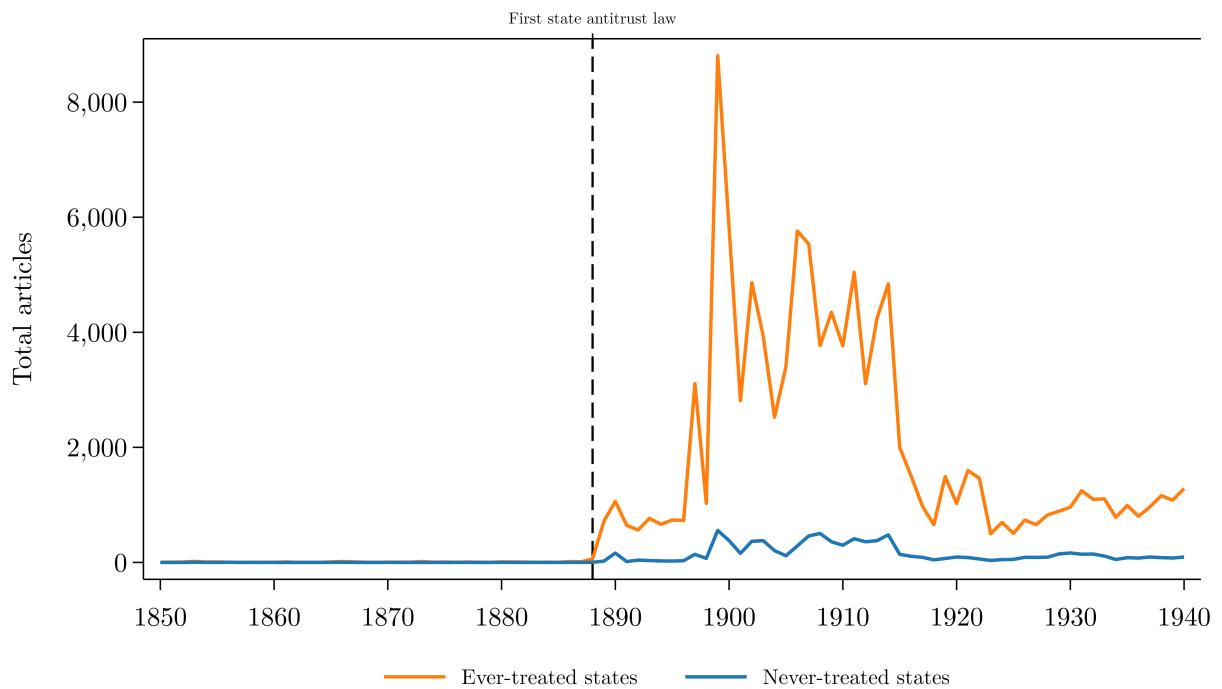
**Notes:** This map is shaded to show the year in which a state first adopted an antitrust law. In some cases, laws were later repealed by legislative act or overturned by court ruling, but this map does not reflect those changes. Thirteen states had already adopted an antitrust statute of their own by the time the Sherman Act—the first federal antitrust law—was enacted in 1890. These are the states shaded in yellow, excepting Oklahoma and Louisiana, which enacted antitrust statutes in 1890 after the passage of the Sherman Act on July 2, 1890. State boundaries in 1940 are shown.

**Figure 4: State Antitrust Statutes, 1888-1940**



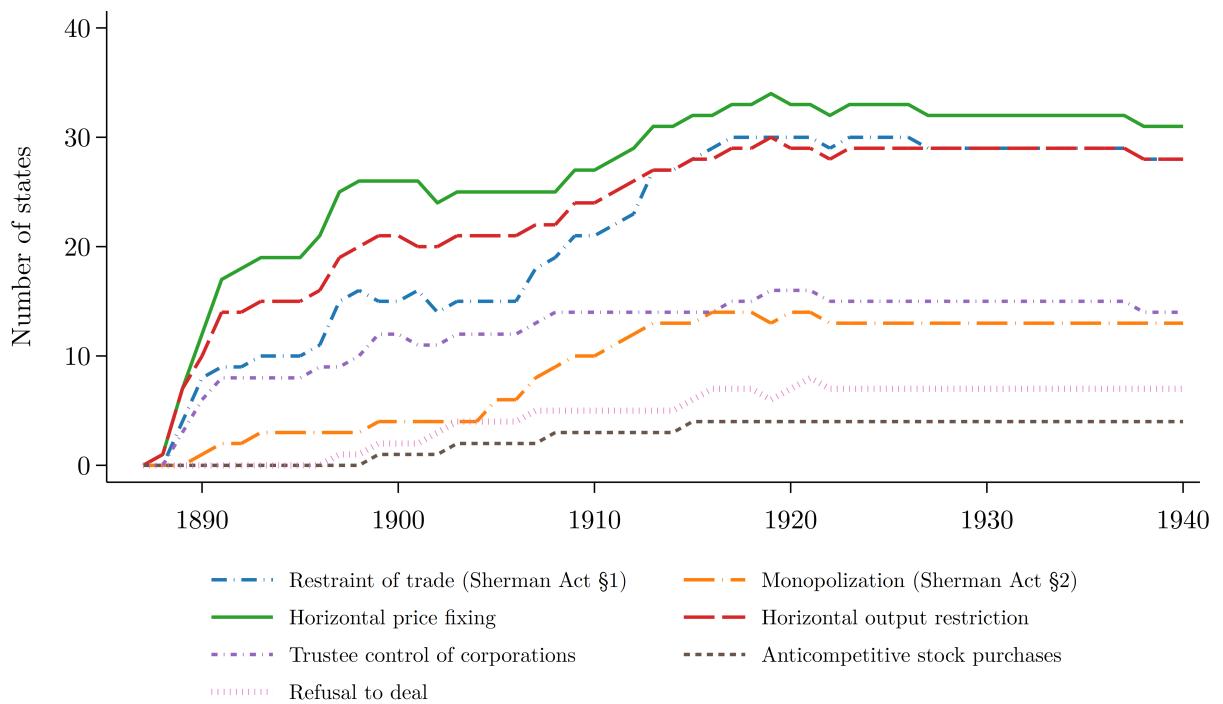
**Notes:** This figure illustrates the years in which state legislatures adopted and amended antitrust statutes, as well as the years in which antitrust statutes were repealed by legislative act or overturned in full or in part by a court. Delaware, Maryland, Nevada, Oregon, Pennsylvania, Rhode Island, Vermont, Washington, and West Virginia are not included in this figure because these states did not enact a general antitrust statute between 1860 and 1940. Alaska and Hawaii are not included in this figure because these states achieved statehood after 1940.

**Figure 5: Total Number of Newspaper Articles Mentioning State Antitrust Laws**



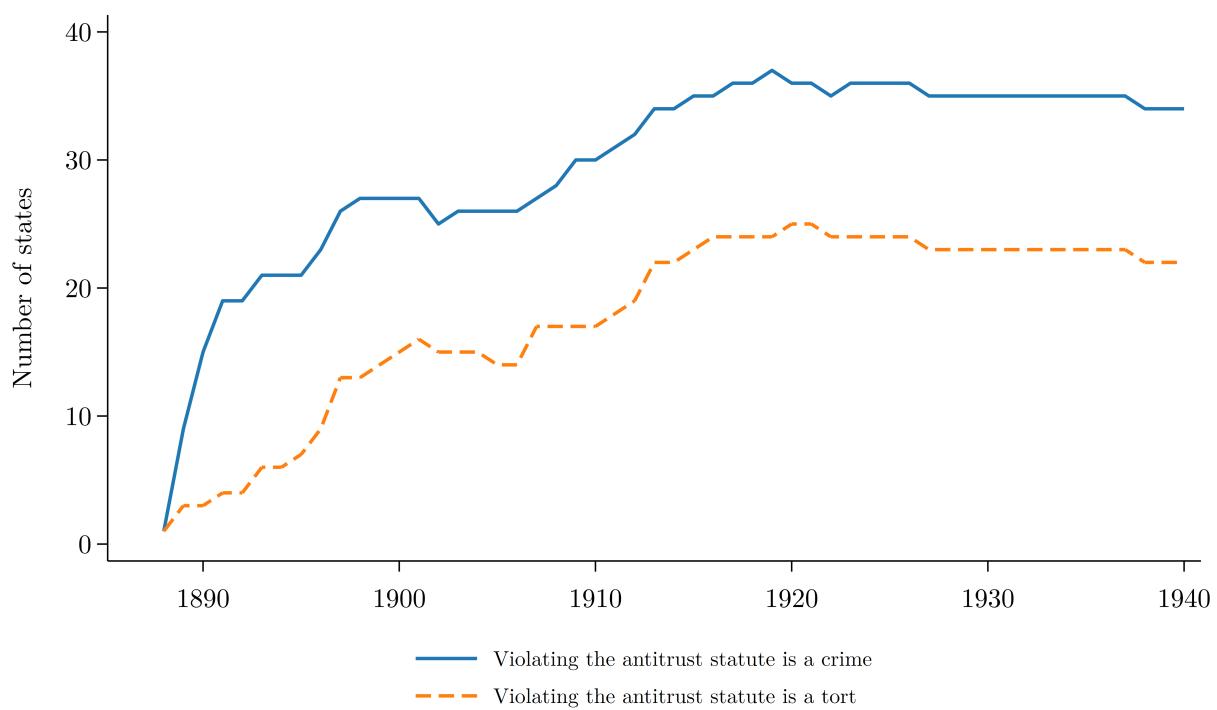
**Notes:** “Ever-treated” states adopted a state antitrust statute at some point between 1888 and 1940. “Never-treated” states did not adopt a state antitrust statute at any point during this period.

**Figure 6: Anticompetitive Acts Made Illegal by State Antitrust Laws, 1888-1940**



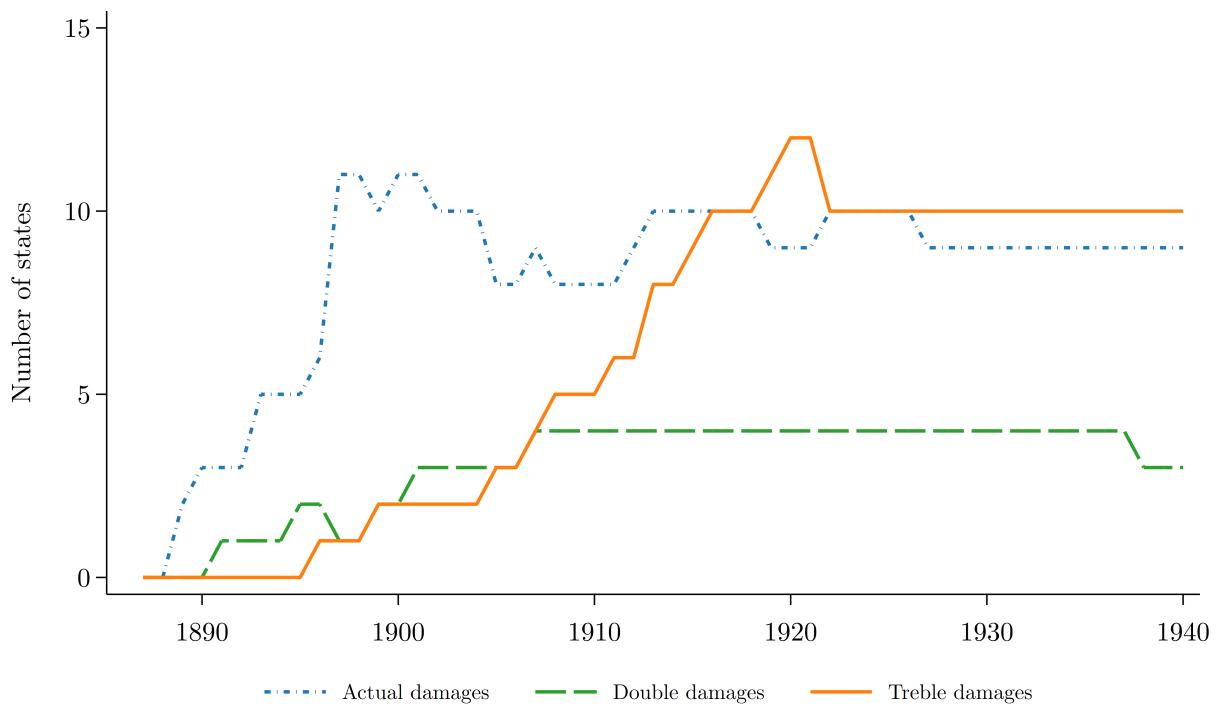
**Notes:** This figure illustrates the number of states that outlawed various anticompetitive acts between 1888 and 1940. See table B1 for definitions of these acts.

**Figure 7: States Declaring Antitrust Violations Crimes vs. Torts, 1888-1940**



**Notes:** This figure illustrates the number of states that declared violations of their antitrust statute to be crimes and the number of states that declared violations of their antitrust statute to be torts between 1888 and 1940. A crime is a wrongful act that violates a law and is prosecuted by the government, while a tort is a civil wrong that causes harm to an individual, who may seek compensation through a lawsuit.

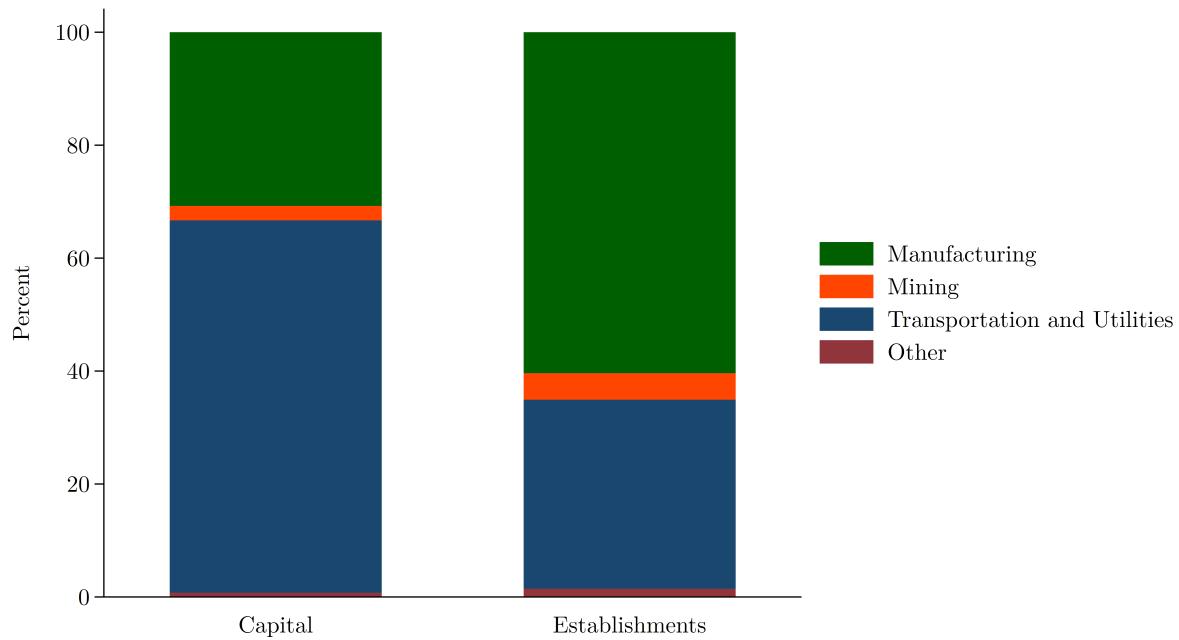
**Figure 8: Civil Damages Under State Antitrust Laws, 1888-1940**



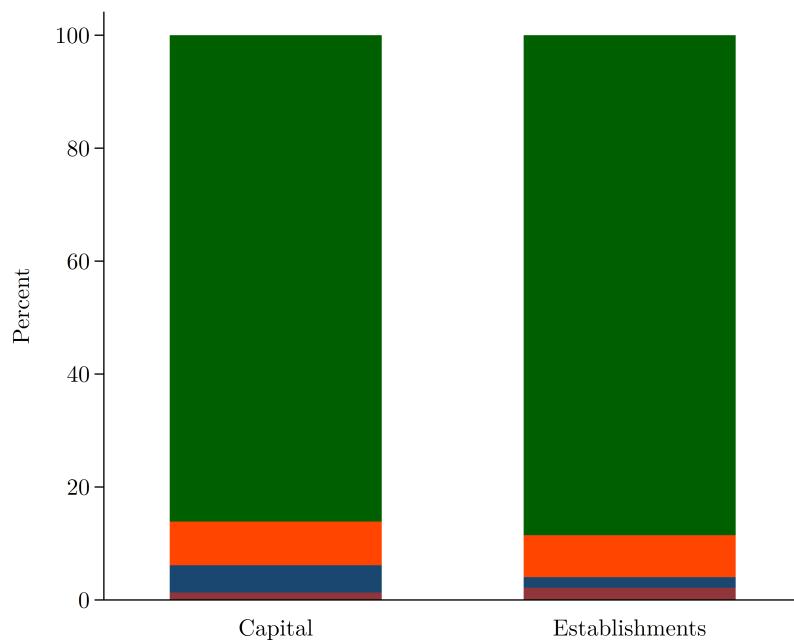
**Notes:** This figure illustrates the number of states authorizing plaintiffs to seek actual damages, double damages, or treble damages in civil actions for injury due to unlawful conduct under state antitrust statutes.

**Figure 9: Trusts by Sector, 1903**

*Panel A: All Trusts*

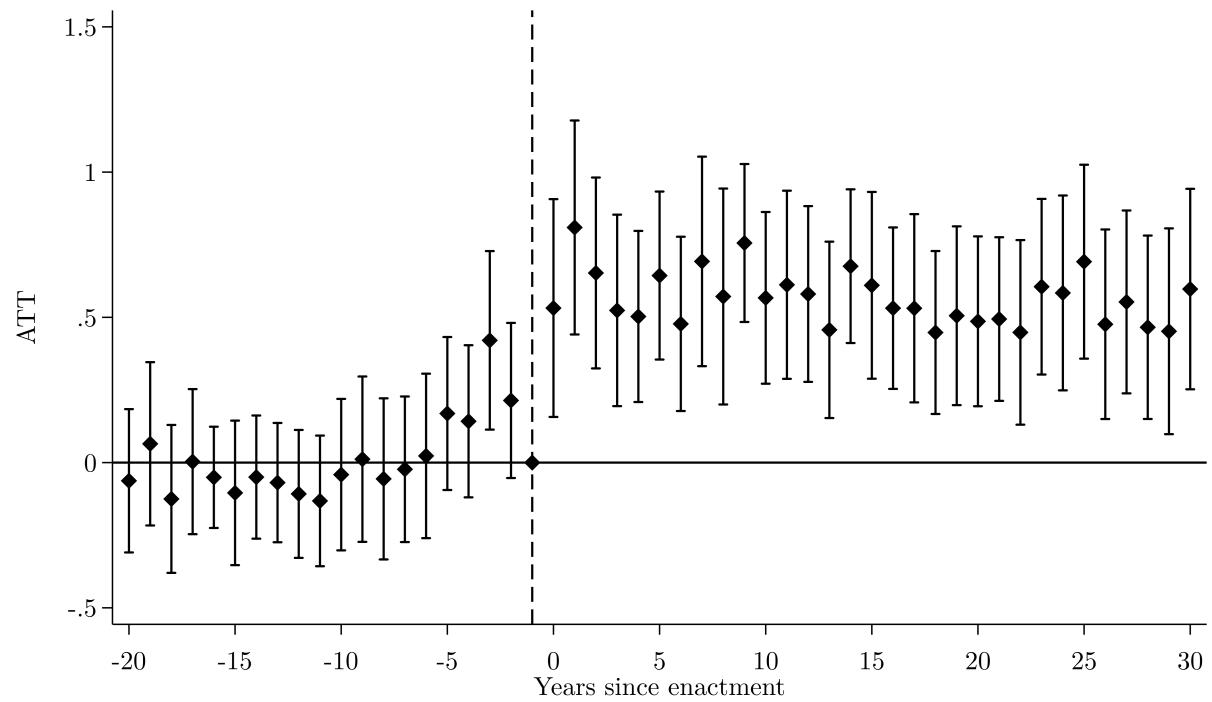


*Panel B: "Industrial" Trusts*



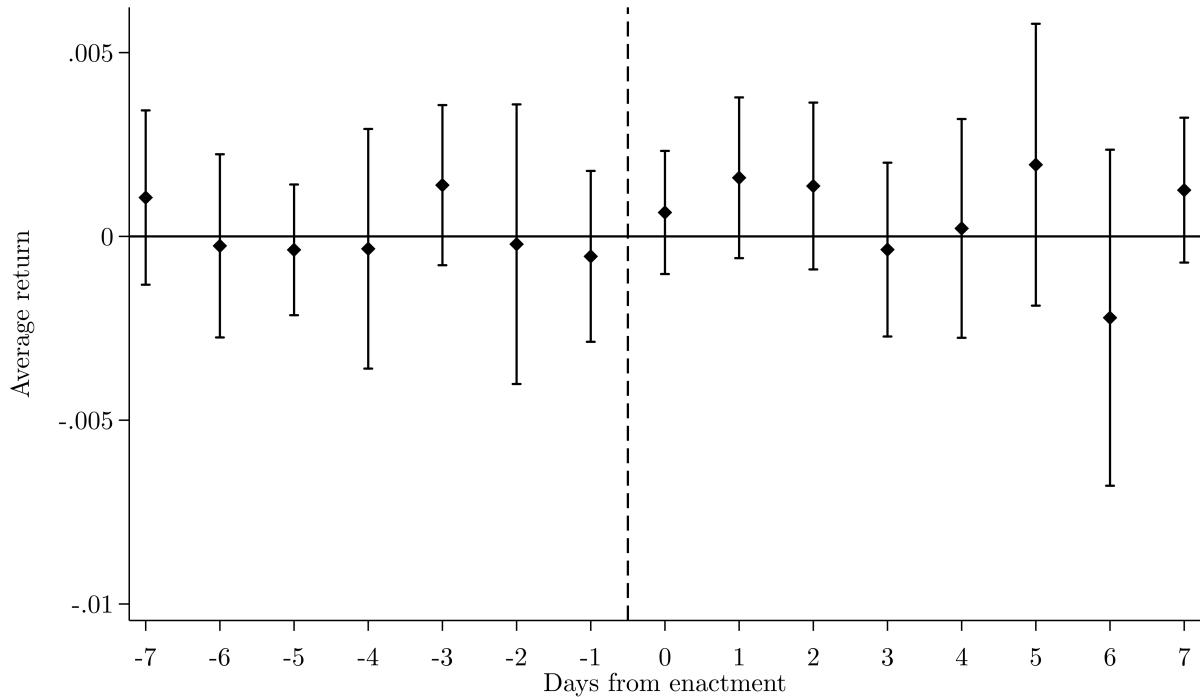
**Notes:** This figure shows the distribution of capital and establishments across the trusts listed in Moody (1904). These distributions are given for all 445 trusts in Panel A and for the 305 "industrial" trusts in Panel B.

**Figure 10: Effect of Enactment on the Number of Newspaper Articles Mentioning State Antitrust Laws**



**Notes:** The dependent variable is the log number of newspaper articles mentioning each state's antitrust law. Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. This event study does not include controls.

**Figure 11: Average Daily Stock Returns Around Enactment**



**Notes:** This figure shows the average daily return to the Dow Jones composite portfolio in the seven trading days before and after the passage of a state antitrust law. For each passage date, I extracted a 15-day event window (date of passage  $\pm$  seven days) using historical daily return data (Schwert 1990). To account for non-trading days (i.e., weekends and holidays), I selected the 15 closest trading days around each date of passage. I then stacked the data across all dates of passage and computed the mean return for each relative event day, as well as the standard error. Days appearing in multiple states' windows were thus included multiple times. Bars indicate 95 percent confidence intervals.

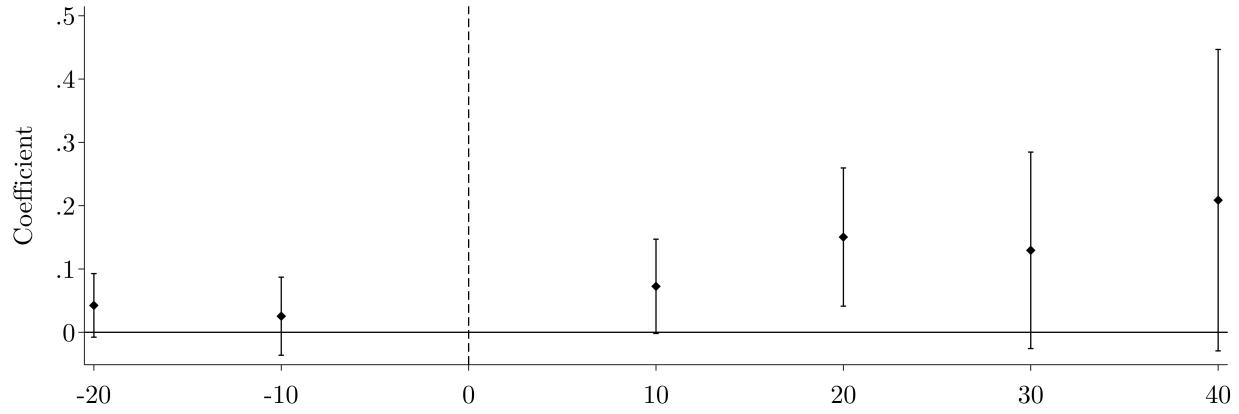
**Table 1: Main Difference-in-Differences Results**

	(1)	(2)	(3)	(4)
<i>Panel A: Log Number of Establishments</i>				
ATT	0.187 (0.125)	0.083 (0.072)	0.181** (0.088)	0.102** (0.050)
Observations	70,322	70,322	70,322	70,322
<i>Panel B: Log Average Profit per Establishment</i>				
ATT	-0.051 (0.093)	-0.074 (0.095)	-0.040 (0.060)	-0.012 (0.058)
Observations	68,801	68,801	68,801	68,801
<i>Panel C: Log Average Number of Proprietors per Establishment</i>				
ATT	-0.091 (0.150)	-0.111 (0.146)	-0.060 (0.124)	-0.098 (0.121)
Observations	29,878	29,878	29,878	29,878
<i>Panel D: Log Average Employment per Establishment</i>				
ATT	-0.077 (0.065)	-0.089 (0.069)	-0.059 (0.037)	-0.002 (0.037)
Observations	70,115	70,115	70,115	70,115
<i>Panel E: Log Average Wage</i>				
ATT	-0.026 (0.024)	-0.026 (0.024)	-0.040* (0.022)	-0.034 (0.021)
Observations	69,160	69,160	69,160	69,160
<i>Panel F: Log Labor Share</i>				
ATT	-0.048** (0.019)	-0.039** (0.019)	-0.053*** (0.018)	-0.042*** (0.014)
Observations	69,158	69,158	69,158	69,158
Baseline controls	✓	✓	✓	✓
Industry FE	✓			
State-by-industry FE		✓		✓
Year-by-industry FE			✓	✓

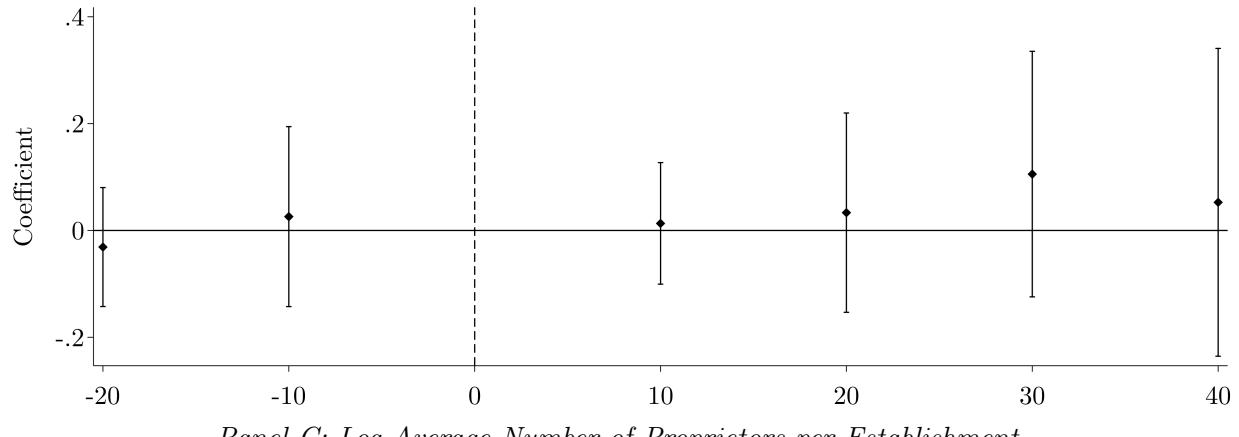
**Notes:** Estimates are obtained using the stacked difference-in-differences method. Robust standard errors clustered at the state level are reported in parentheses. \*\*\* denotes statistical significance at the 1 percent level, \*\* denotes statistical significance at the 5 percent level, and \* denotes statistical significance at the 10 percent level. States whose antitrust laws were overturned or repealed are excluded. All models include controls for the population, median occupational score, estimated personal income, and literacy rate of each state.

**Figure 12: Event Study Results for Establishment Outcomes**

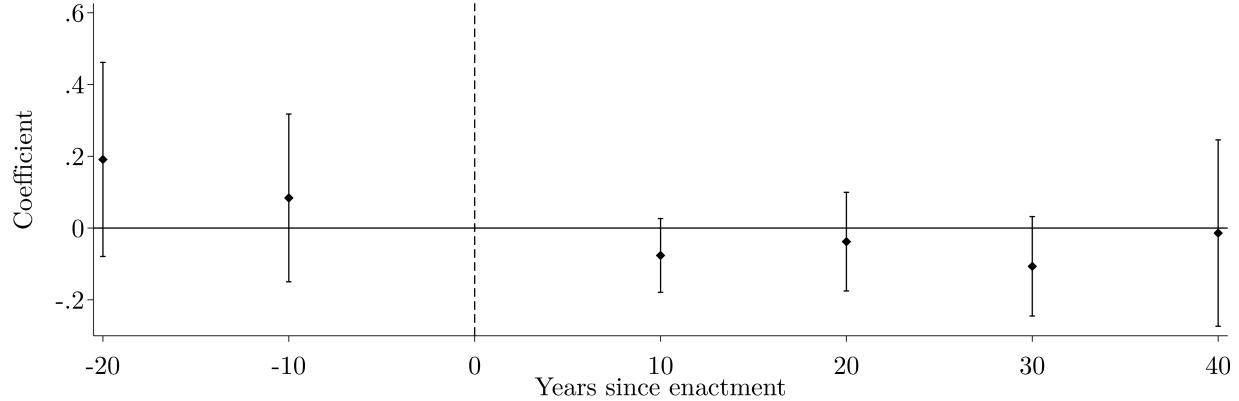
*Panel A: Log Number of Establishments*



*Panel B: Log Average Profit per Establishment*



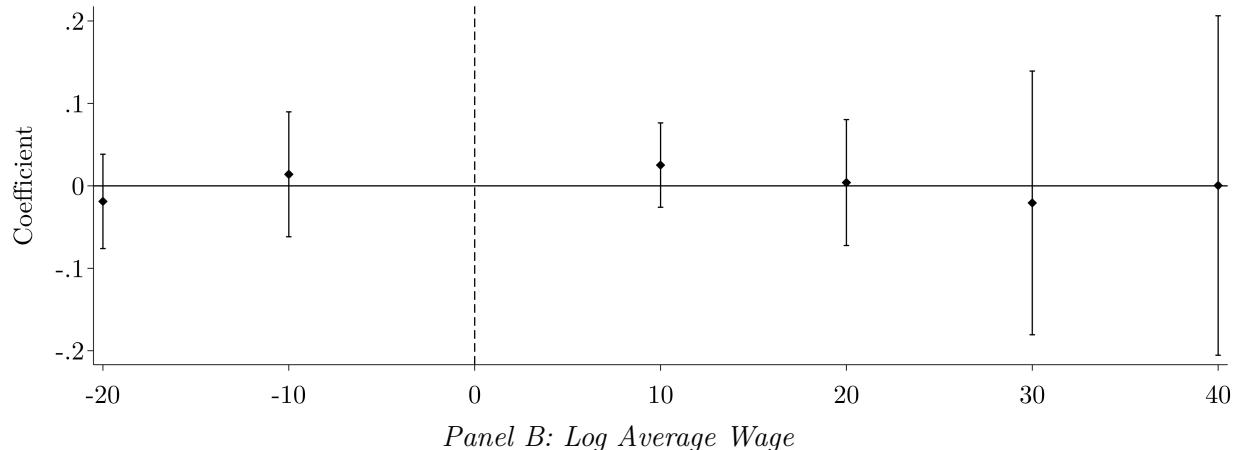
*Panel C: Log Average Number of Proprietors per Establishment*



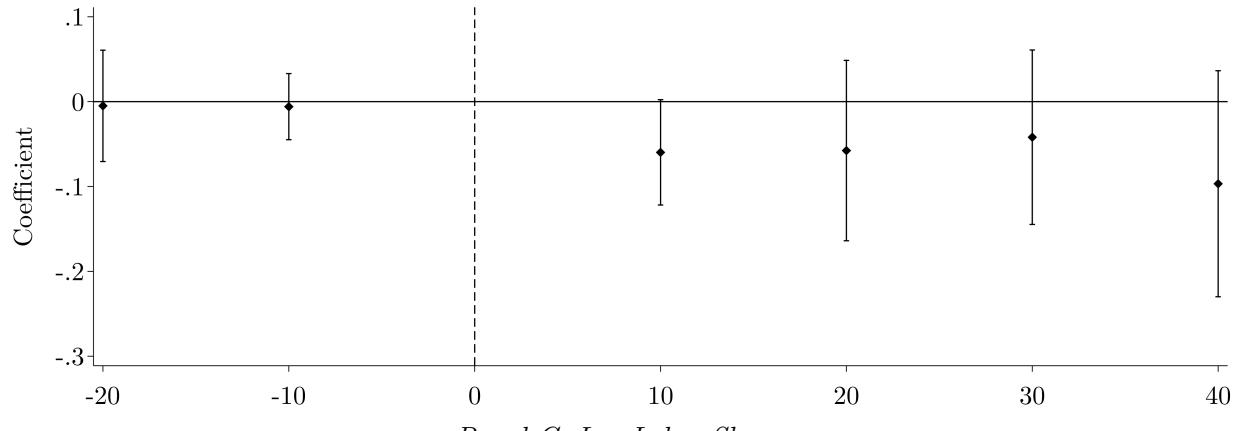
**Notes:** Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

**Figure 13: Event Study Results for Labor Market Outcomes**

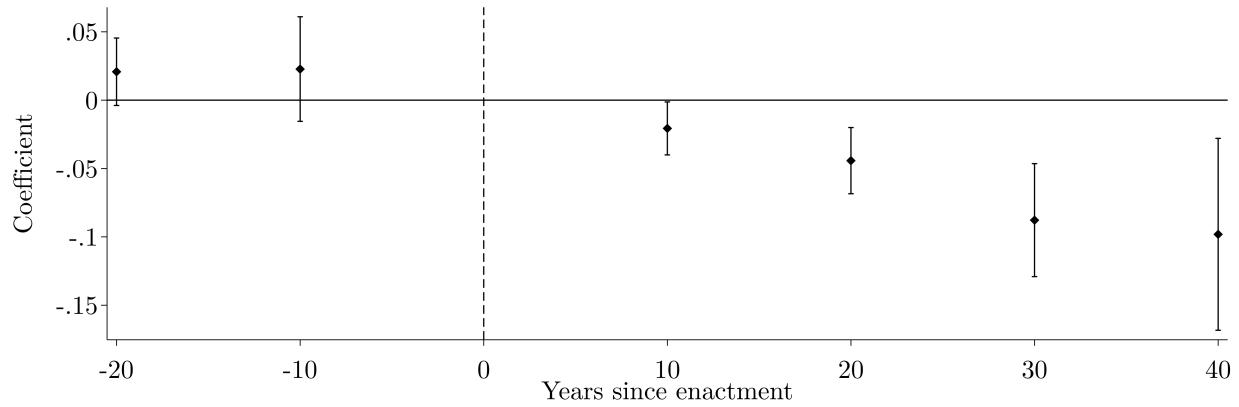
*Panel A: Log Average Employment per Establishment*



*Panel B: Log Average Wage*



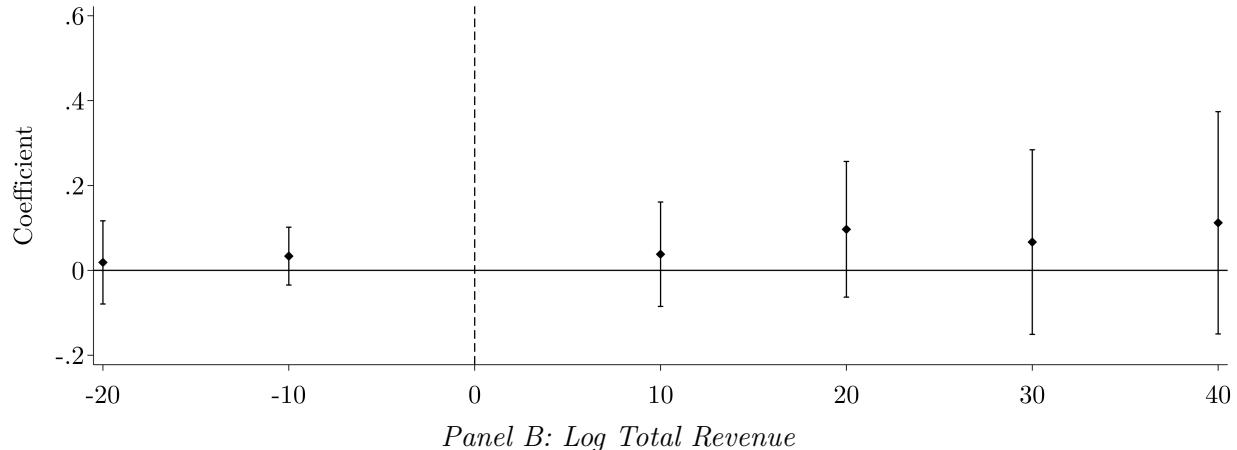
*Panel C: Log Labor Share*



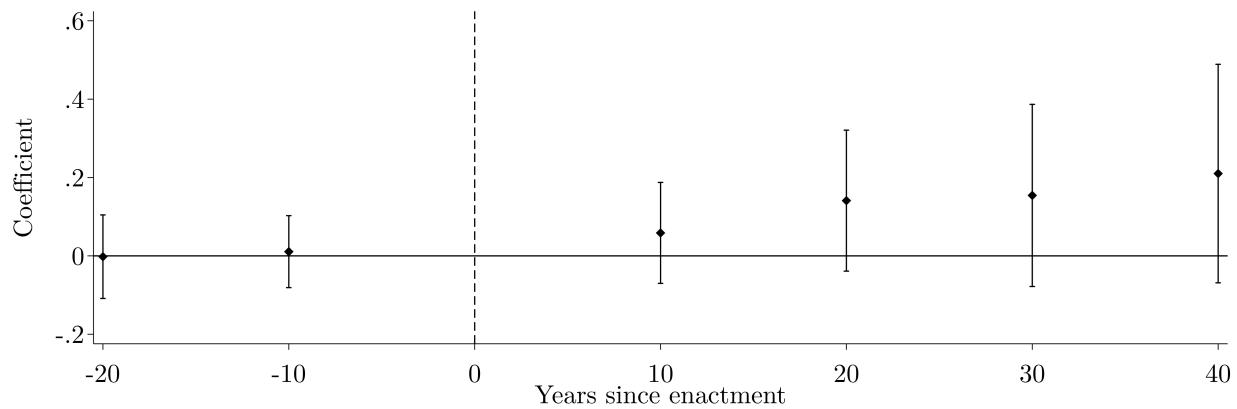
**Notes:** Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

**Figure 14: Event Study Results for Labor Share Components**

*Panel A: Log Total Wage Bill*



*Panel B: Log Total Revenue*



**Notes:** Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

**Table 2: Difference-in-Differences Results by Trust Status**

	Industries with a Trust		Industries without a Trust	
	(1)	(2)	(3)	(4)
<i>Panel A: Log Number of Establishments</i>				
ATT	0.127 (0.088)	0.061 (0.053)	0.211* (0.105)	0.155** (0.066)
Observations	30807	30807	39515	39515
<i>Panel B: Log Average Profit per Establishment</i>				
ATT	0.014 (0.081)	0.003 (0.077)	-0.069 (0.050)	-0.020 (0.039)
Observations	30144	30144	38657	38657
<i>Panel C: Log Average Number of Proprietors per Establishment</i>				
ATT	0.028 (0.156)	-0.011 (0.139)	-0.147* (0.086)	-0.167* (0.088)
Observations	12586	12586	17292	17292
<i>Panel D: Log Average Employment per Establishment</i>				
ATT	-0.019 (0.046)	0.001 (0.050)	-0.062 (0.053)	0.011 (0.033)
Observations	30712	30712	39403	39403
<i>Panel E: Log Average Wage</i>				
ATT	-0.047* (0.024)	-0.033 (0.022)	-0.027 (0.024)	-0.031 (0.025)
Observations	30324	30324	38836	38836
<i>Panel F: Log Labor Share</i>				
ATT	-0.071*** (0.025)	-0.053*** (0.019)	-0.025 (0.018)	-0.022 (0.017)
Observations	30322	30322	38836	38836
Baseline controls	✓	✓	✓	✓
State-by-industry FE		✓		✓
Year-by-industry FE	✓	✓	✓	✓

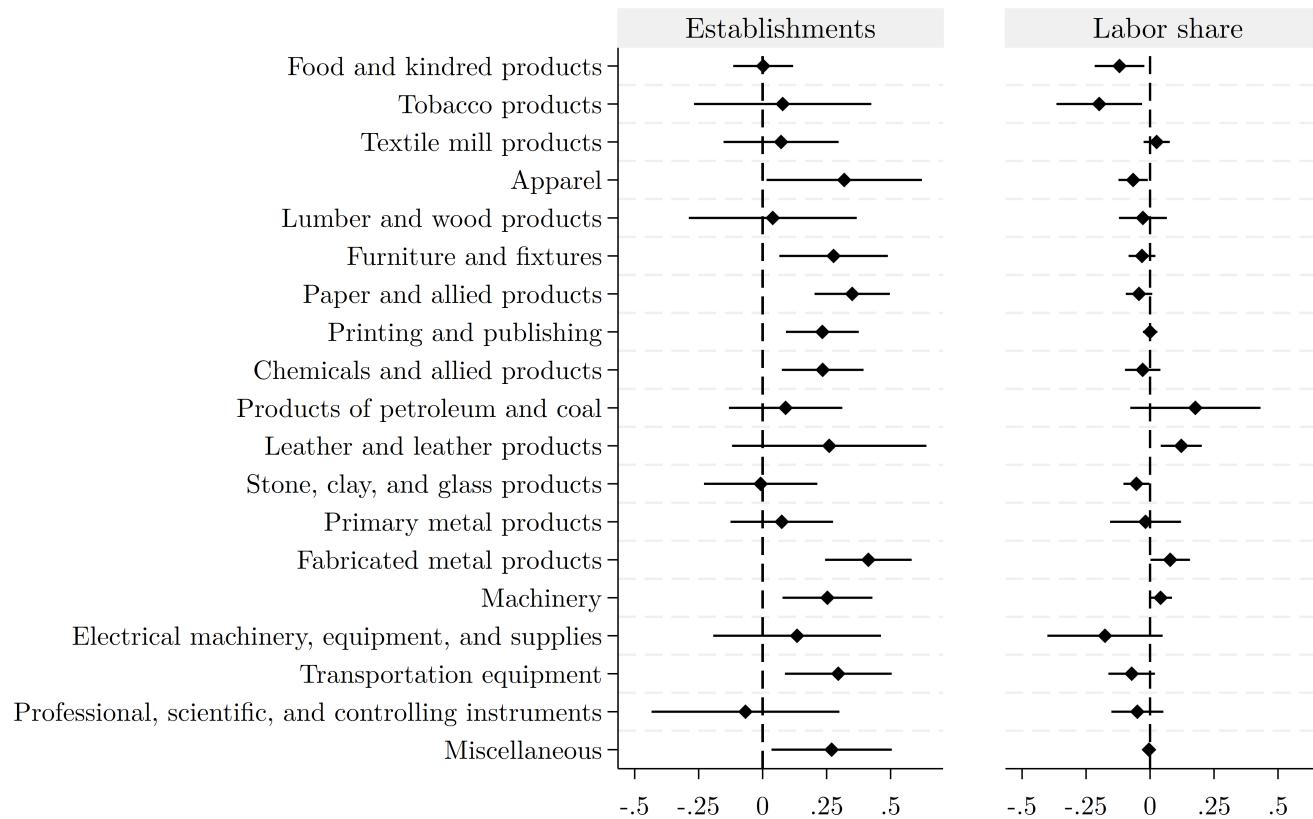
**Notes:** Trust status is measured in 1903 and is from Moody 1904. Estimates are obtained using the stacked difference-in-differences method. Robust standard errors clustered at the state level are reported in parentheses. \*\*\* denotes statistical significance at the 1 percent level, \*\* denotes statistical significance at the 5 percent level, and \* denotes statistical significance at the 10 percent level. States whose antitrust laws were overturned or repealed are excluded. All models include controls for the population, median occupational score, estimated personal income, and literacy rate of each state.

**Table 3: Robustness Checks on Main Difference-in-Differences Results**

	(1)	(2)	(3)	(4)
<i>Panel A: Log Number of Establishments</i>				
ATT	0.148** (0.065)	0.148** (0.062)	0.100* (0.051)	0.092* (0.051)
Observations	70,322	70,322	70,322	70,322
<i>Panel B: Log Average Profit per Establishment</i>				
ATT	-0.010 (0.040)	0.008 (0.049)	-0.015 (0.058)	-0.061 (0.062)
Observations	68,801	68,801	68,801	68,801
<i>Panel C: Log Average Number of Proprietors per Establishment</i>				
ATT	-0.023 (0.073)	-0.140 (0.103)	-0.098 (0.121)	-0.130 (0.112)
Observations	29,878	29,878	29,878	29,878
<i>Panel D: Log Average Employment per Establishment</i>				
ATT	-0.001 (0.035)	-0.013 (0.041)	-0.006 (0.037)	-0.026 (0.038)
Observations	70,115	70,115	70,115	70,115
<i>Panel E: Log Average Wage</i>				
ATT	-0.018 (0.020)	-0.014 (0.020)	-0.032 (0.021)	-0.053* (0.027)
Observations	69,160	69,160	69,160	69,160
<i>Panel F: Log Labor Share</i>				
ATT	-0.030** (0.014)	-0.024* (0.012)	-0.040*** (0.014)	-0.033** (0.013)
Observations	69,158	69,158	69,158	69,158
Baseline controls	✓	✓	✓	✓
State-by-industry FE	✓	✓	✓	✓
Year-by-industry FE	✓	✓	✓	✓
Establishment weighting	✓		✓	
Employment weighting		✓		
Const. provision control			✓	
Ag. share control				✓

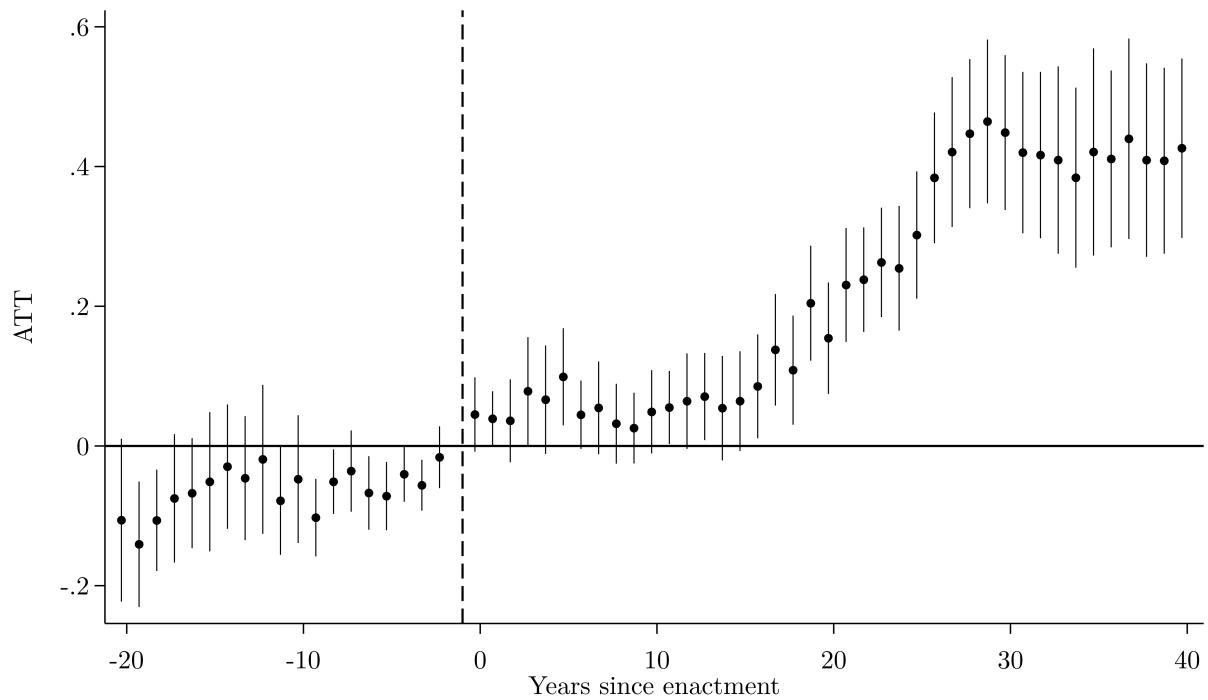
**Notes:** Estimates are obtained using the stacked difference-in-differences method. Robust standard errors clustered at the state level are reported in parentheses. \*\*\* denotes statistical significance at the 1 percent level, \*\* denotes statistical significance at the 5 percent level, and \* denotes statistical significance at the 10 percent level. States whose antitrust laws were overturned or repealed are excluded. All models include controls for the population, median occupational score, estimated personal income, and literacy rate of each state.

**Figure 15: Difference-in-Differences Results by Industry Category**



**Notes:** The dependent variable is the log number of manufacturing establishments in the first column and the log labor share in the second column. Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

**Figure 16: Event Study Results for Firms' Patenting Behavior**



**Notes:** The dependent variable is the log number of assignee patents per county. Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, personal income, median occupational score, and literacy rate. County fixed effects are also included. Only never-treated states are in the control group. Results are similar if not-yet-treated states are also included.

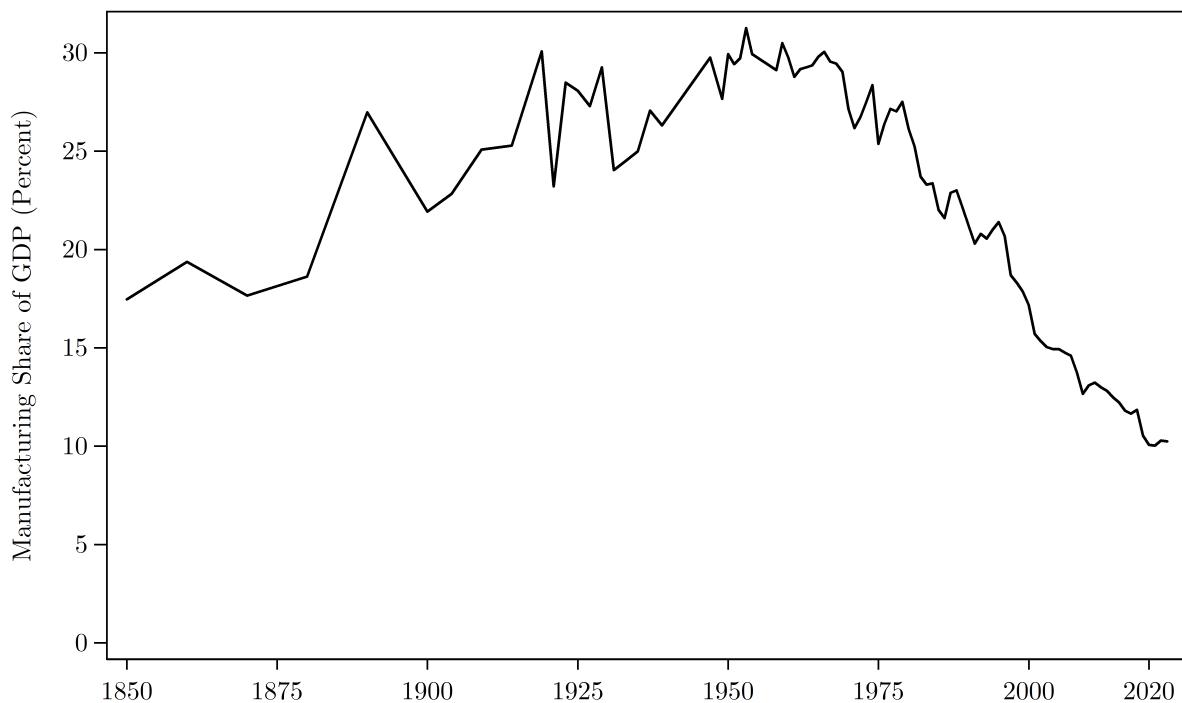
**Table 4: Difference-in-Differences Results for Firms' Patenting Behavior**

	(1)	(2)	(3)	(4)
ATT	0.268*** (0.034)	0.243*** (0.030)	0.232*** (0.036)	0.213*** (0.033)
Observations	221,383	221,383	221,383	221,383
Controls	✓	✓	✓	✓
Not-yet-treated		✓		✓
County FE			✓	✓

**Notes:** The dependent variable is the log number of assignee patents per county. Results are estimated using the Callaway and Sant'Anna (2021) method. Robust standard errors are clustered at the state level. \*\*\* denotes statistical significance at the 1 percent level, \*\* denotes statistical significance at the 5 percent level, and \* denotes statistical significance at the 10 percent level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, personal income, median occupational score, and literacy rate. The second and fourth columns include not-yet-treated states in the control group. The first and third columns include only never-treated states in the control group.

## A Supplemental Figures and Tables

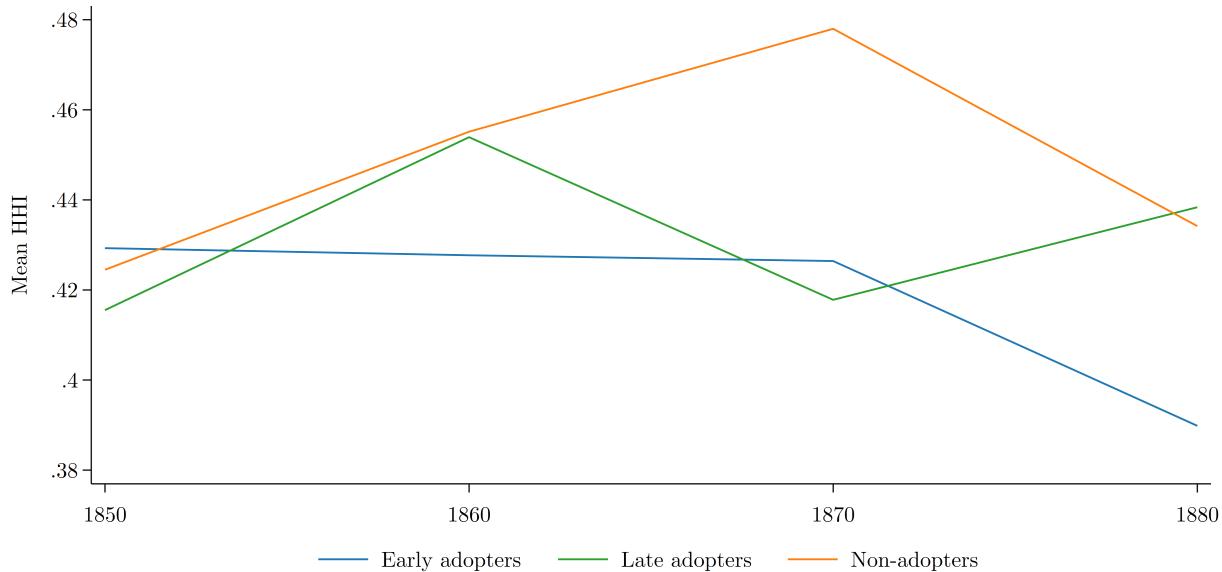
**Figure A1: Manufacturing Share of Gross Domestic Product (GDP), 1850-2023**



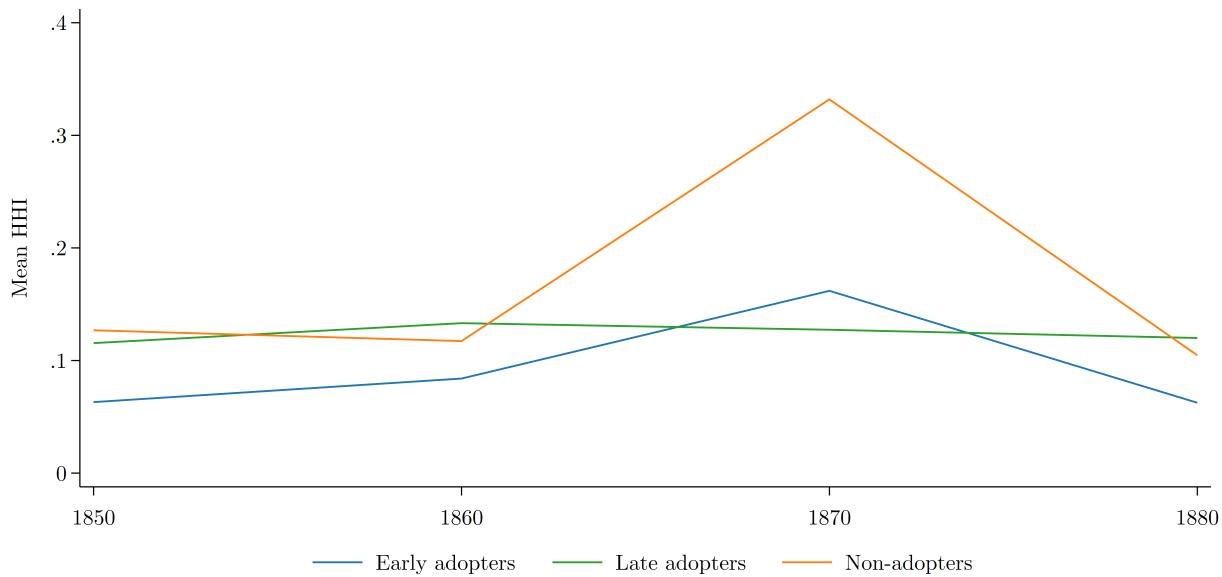
**Notes:** This figure plots the share of GDP in manufacturing, which is aggregate value added in the manufacturing sector over nominal GDP, for 1850 through 2023. I compiled data on value added in the manufacturing sector from several sources. The U.S. Bureau of the Census (1942, p. 19) provides data from past censuses of manufactures spanning 1850 through 1939 in Table 2. Between 1850 and 1890 so-called “hand and neighborhood” establishments are included in the data, but in 1900 and later years, the data exclude these smaller artisans and instead focus on larger factories. The U.S. Bureau of the Census (1957, p. 3) provides values for 1900 through 1954 in Table 1B. Becker, Gray, and Marvakov (2021) provide industry-level data for 1958 through 2018, which I aggregated by year. Finally, U.S. Bureau of Economic Analysis (2024) provide data for 1997 through 2023. In years where these sources overlap, I averaged their values, though values generally only differed slightly. GDP data are from Johnston and Williamson (2024) for 1850 to 2022 and Federal Reserve Bank of St. Louis (2024) for 2023. As Baily and Bosworth (2014) show, trends in manufacturing’s share of *real* GDP differ from trends in manufacturing’s share of *nominal* GDP because the prices of manufactured goods have increased at a different rate than the prices of non-manufactured goods. However, a separate price index for manufactured goods is not currently available going as far back as 1850, so the figure must be interpreted with this caveat in mind.

**Figure A2: Average Concentration in the Manufacturing Sector by Treatment Status, 1850-1880**

*Panel A: Unweighted Mean HHI*



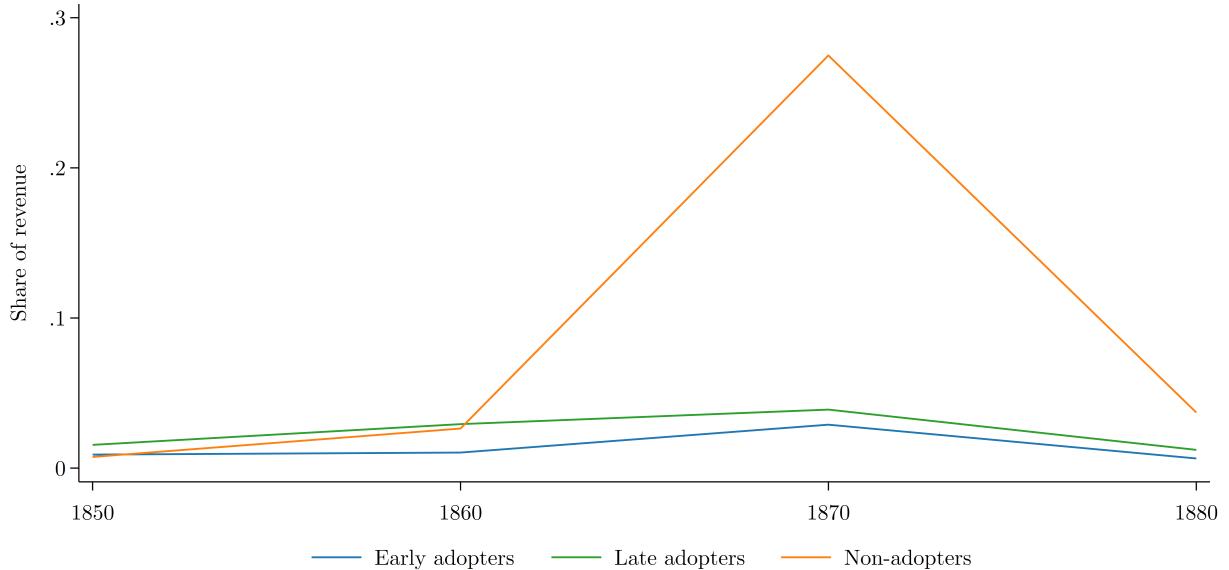
*Panel B: Revenue-Weighted Mean HHI*



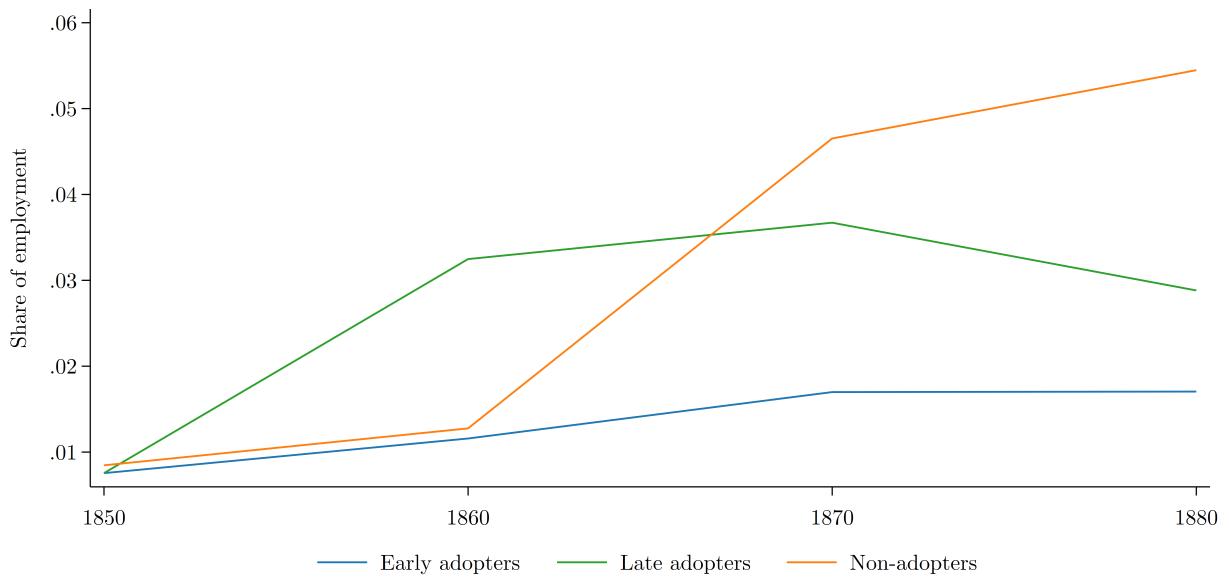
**Notes:** This figure illustrates the mean Herfindahl–Hirschman Index (HHI) by treatment status from 1850 through 1880. “Early adopters” enacted a statute before 1895, while “late adopters” enacted a statute in 1895 or later. In order to compare concentration across states, I computed HHI at the state-by-industry level using the surviving schedules from the 1850–1880 censuses of manufactures (Hornbeck et al. 2024). I do not apply weights in Panel A, while in Panel B, I weight each state-by-industry HHI by its corresponding level of revenue. Schedules for 1880 are missing from industries in which special agents were appointed to collect manufacturing data (Atack and Bateman 1999; Delle Donne 1973). These industries are iron and steel, cotton goods, woolen and worsted goods, silk and silk goods, chemical products and salt, coke and glass, shipbuilding, fisheries, all types of mining, coal, and petroleum (Wright 1900, p. 63). For consistency over time, I removed these industries in the graphs shown above.

**Figure A3: Manufacturing Revenue and Employment in Nationally Monopolized Industries by Treatment Status, 1850-1880**

*Panel A: Manufacturing Revenue*

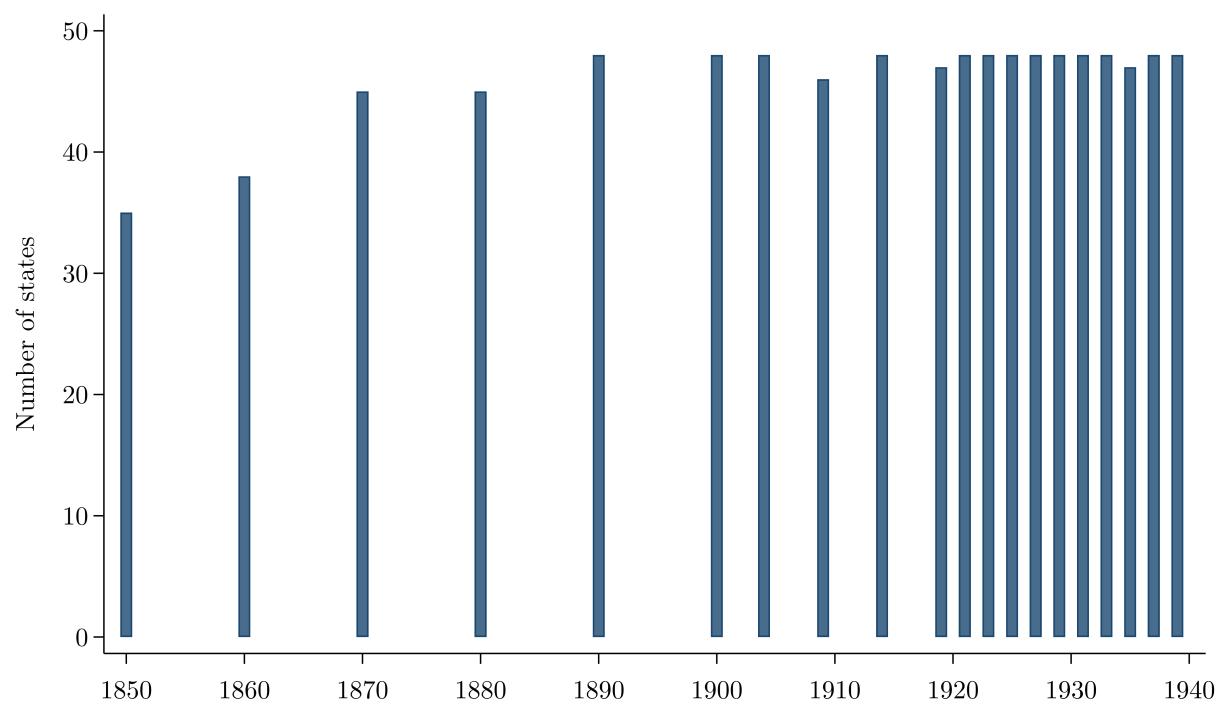


*Panel B: Manufacturing Employment*



**Notes:** This figure illustrates the share of manufacturing revenue (Panel A) and the share of manufacturing employment (Panel B) in “nationally monopolized” industries—i.e., industries with a national HHI over 0.18—from 1850 through 1880, by treatment status. “Early adopters” enacted a statute before 1895, while “late adopters” enacted a statute in 1895 or later. I computed these measures using the surviving schedules from the 1850-1880 censuses of manufactures (Hornbeck et al. 2024). Schedules for 1880 are missing from industries in which special agents were appointed to collect manufacturing data (Attack and Bateman 1999; Delle Donne 1973). These industries are iron and steel, cotton goods, woolen and worsted goods, silk and silk goods, chemical products and salt, coke and glass, shipbuilding, fisheries, all types of mining, coal, and petroleum (Wright 1900, p. 63). For consistency over time, I removed these industries in the graphs shown above.

**Figure A4: Coverage of State-by-Industry Census of Manufactures Data, 1850-1940**



**Notes:** This figure illustrates the number of U.S. states included in the state-by-industry census of manufactures data from 1850 to 1940. Each bar corresponds to a census year.

Figure A5: Examples of Newspaper Articles Mentioning State Antitrust Laws

**IOWA'S ANTI-TRUST LAW.**  
NO PROPER MACHINERY PROVIDED FOR ITS ENFORCEMENT.

DES MOINES, Iowa, June 6.—The Attorney General has given an important opinion in regard to the anti-trust law of the last General Assembly. That law provides that the Secretary of State shall address inquiries to all the corporations organized under the laws of or doing business in the State, as to whether they are violating the provisions of the act by unlawfully combining or merging their corporate existence in any other company. These inquiries the proper officer of the company is compelled to answer under oath. In event of their refusal to comply with the requirements of the statute, the Attorney General, through any County Attorneys of the State, shall direct proceedings, which shall have as their result on conviction, the revocation of the charter of the company.

The State Secretary Jackson sent the proper forms of affidavit to the companies of the State. The results obtained can be tabulated: Corporations complying, 23; corporations as to which the Secretary had information that they had gone out of business, 370; corporations to which letters addressed were returned marked "Uncalled for," 951; corporations from which no reply was received, 1,991. These returns were properly certified to the Attorney General. Before instituting suits he concluded to examine the evidence obtained. As to the first three classes, there is no evidence that they have "refused" to comply with the demands of the State. Large numbers of them are agricultural, charitable, religious, and educational in character, and it is likely that many of the communications did not reach them or reached other parties who might formerly have been officers.

The Attorney General also maintains that it was hardly the intention of the General Assembly that summary measures should be instituted against corporations of this character. After discussing the difficulties of the situation, and what would be proper evidence to enable him to bring suit with any degree of expectancy of success, the Attorney General concludes: "In view of the insufficiency of the machinery provided for the execution of this section of this statute, I think the situation is of so grave a character as to warrant us in calling the attention of the Governor to it for such consideration as he may deem proper in the preparation of his biennial message to the General Assembly."

(a) *New York Times*  
New York, New York  
June 7, 1891

#### THE GEORGIA ANTI-TRUST LAW.

(Atlanta Journal.)

The Journal has received from many States requests for copies of the Georgia anti-trust law. In some instances these requests have come from members of Legislatures who stated that they intended to introduce anti-trust bills. The Georgia law has attracted especial attention because it proved so speedily effective. A bill that will stop the operations of trusts in a State has naturally attracted very general attention. The Georgia bill had been introduced in four or five Legislatures, and it is probable that several other States will soon have anti-trust laws copied after ours. The Alabama House of Representatives last Saturday, without a dissenting voice, passed a bill which copies the Georgia law exactly. It is said that it will certainly pass the Senate and become a law at the present session.

The Georgia Legislature seems to have set the anti-trust ball rolling at a lively rate. Let 'er roll.

(b) *News and Observer*  
Raleigh, North Carolina  
February 10, 1897

## STANDARD OIL FOUND GUILTY

### Combine Convicted of Violating the Anti-Trust Law of Ohio.

*Special Dispatch to the "Chronicle."*

FINDLAY (O.), October 19.—By the verdict of a jury the Standard Oil Company of Ohio is guilty of conspiracy against trade, in violation of the Valentine anti-trust law of Ohio. The penalty is a fine of from \$50 to \$50,000, which may be repeated for each day of the offense, or imprisonment of six to twelve months.

The Standard Oil Company of Ohio has given notice that it will file a motion for a new trial. Under the practice of the Court, the defendant has three days to put this motion in form. The next step will be for the Court to impose the penalty. The defense will then take their bill of exceptions to such rulings of Judge Bunker as they objected to in the Circuit Court of the State. The appeal from this court is to the Supreme Court of the State, by which tribunal there is no doubt the issue will ultimately be decided.

To the State the suit, the verdict and the ultimate appeal is important, particularly because it initiates an entirely new method of proceeding against the alleged trade monopolies—that is, by information and affidavit, instead of by Grand Jury indictment.

The verdict was rendered at 4:30 o'clock this morning, and resulted from a continuous deliberation by the jury during thirty-two consecutive hours. The trial occupied seven days preceding this deliberation. When the case went to the jury at 8:30 o'clock Wednesday night the first ballot of the jurors stood nine for conviction and three for acquittal. As the result of continuous deliberations to 4 o'clock Thursday morning, one of the three for acquittal joined the majority. At 7 o'clock Thursday night one of the two remaining for acquittal went over to the other side, and at 4 o'clock this morning the last of the three gave his assent to the verdict of "guilty."

A touch of the dramatic marked the closing hours of the jury's deliberations. Hymns were sung during all but ten minutes of this time. This ten minutes came at the end and was occupied by the remaining juror who had stood out in explaining his position and surrender to the majority. There was not the slightest levity about this hymnal service. The jurors had then been many hours without sleep. The songs, which were started shortly after 2 o'clock in the morning by about three voices, echoed at first feebly through the spacious courthouse. After one familiar hymn after another was sung, it was evident that the spirit of fraternalism was gaining headway in the small chamber in which the twelve men were locked. The number of voices increased, the hymns gained in volume and enthusiasm. Then "Home, Sweet Home" was sung, the national anthem followed, then more hymns. Laughter was heard between the limited pauses. It bore no tone of derision, but of cordiality. A few minutes later came the announcement that the jury had reached an agreement and Judge Bunker hastily summoned.

(c) *San Francisco Chronicle*  
San Francisco, California  
October 20, 1906

## ANTI-TRUST SUIT SEEKS MILLIONS

LEXINGTON, Miss.—A suit has been entered by the Retail Lumber Dealers Association of Mississippi and Louisiana in the chancery court under the anti-trust statute for the recovery of penalties aggregating \$14,184,000, the minimum under the statute, as the minimum penalty is \$200 a day and the maximum \$50,000.

(d) *Christian Science Monitor*  
Boston, Massachusetts  
July 16, 1909

## MISSOURI COURT OUSTS OPEN PRICE BOARDS

St. Louis Lumber Exchange's Members Fined \$96,000 Under Anti-Trust Laws.

JEFFERSON CITY, Mo., Aug. 31 (Associated Press).—Missouri has scored a complete victory over the new form of business combinations known as "open price associations," in a suit brought by Attorney General Barrett against the St. Louis Lumber Trade Exchange. The Missouri Supreme Court ousted each of the nineteen St. Louis lumber companies composing the exchange, assessed fines totaling \$96,000 and ousted the exchange itself.

The Court sustained the Attorney General in holding that the combination violated the State anti-trust laws, even though it did not directly fix prices. So far as is known it is the first decision of any State court on the points involved and follows closely the decision of the United States Supreme Court last year in the hardwood lumber cases, according to Attorney General Barrett.

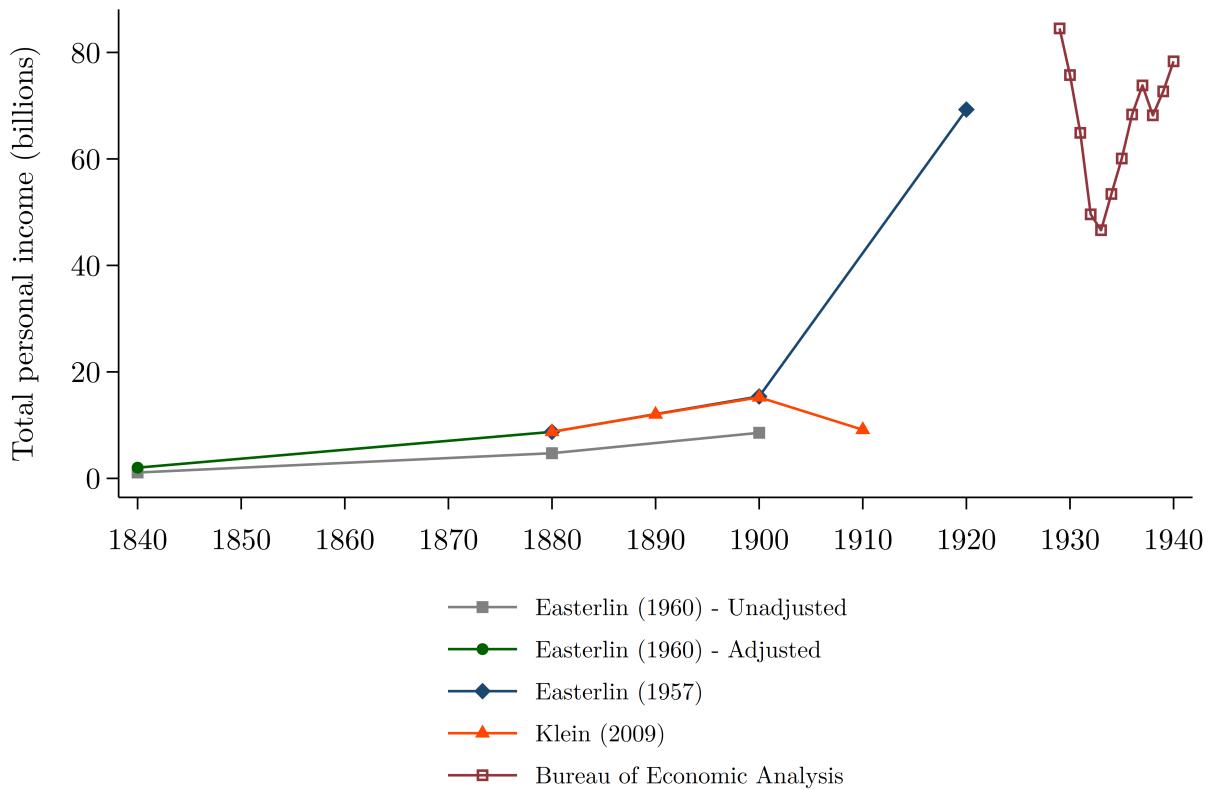
The association regulated the terms of credit and the general business practices of its members, and provided for co-operation that went far, but stopped short of the actual fixing of prices. Books were written describing the plan as "The New Competition," and it was believed, the Attorney General said, that the method would be proof against prosecution by the State or Federal authorities.

The Court in a sweeping decision declared that the Missouri statutes forbid not only agreements to lessen competition, but agreements which tend to lessen it. The decision said that the very creation of a system of machinery which can be improperly used is in itself a violation of the Missouri law.

The Court found that the increased cost of lumber was due to other factors as well, such as higher freight rates and greater labor costs, but condemned the lumber exchange in most positive terms.

(e) *New York Times*  
New York, New York  
September 1, 1923

**Figure A6: State-Level Personal Income Data Sources, 1840-1940**



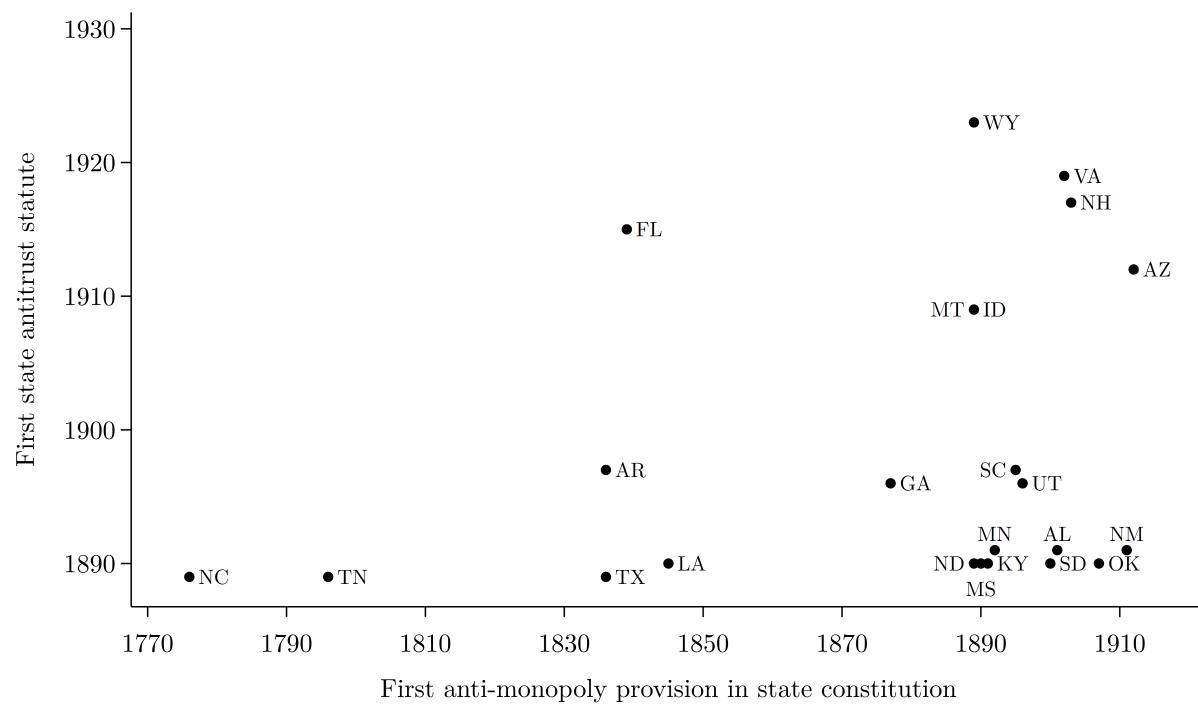
**Notes:** The composite series I construct on state-level personal income comes from four sources. Tables A-1, A-2, and A-3 in Easterlin (1960) provide data for 1840; Table 8 in Klein (2009) provides data for 1880 through 1910; Table Y-1 in Easterlin (1957, p. 753) provides data for 1920; and Bureau of Economic Analysis (2025) provides data for 1929 and later. The data Easterlin (1960) provides for 1840, 1880, and 1900 are adjusted upwards slightly to align with the estimates Easterlin (1957) and Klein (2009) provide for 1880 through 1900.

**Table A1: Pre-Sherman Antitrust Statutes and Constitutional Provisions**

Anti-Monopoly Constitutional Provision			
Antitrust Statute	Yes	No	Total
<b>Yes</b>	4	9	13
<b>No</b>	9	26	35
<b>Total</b>	13	35	48

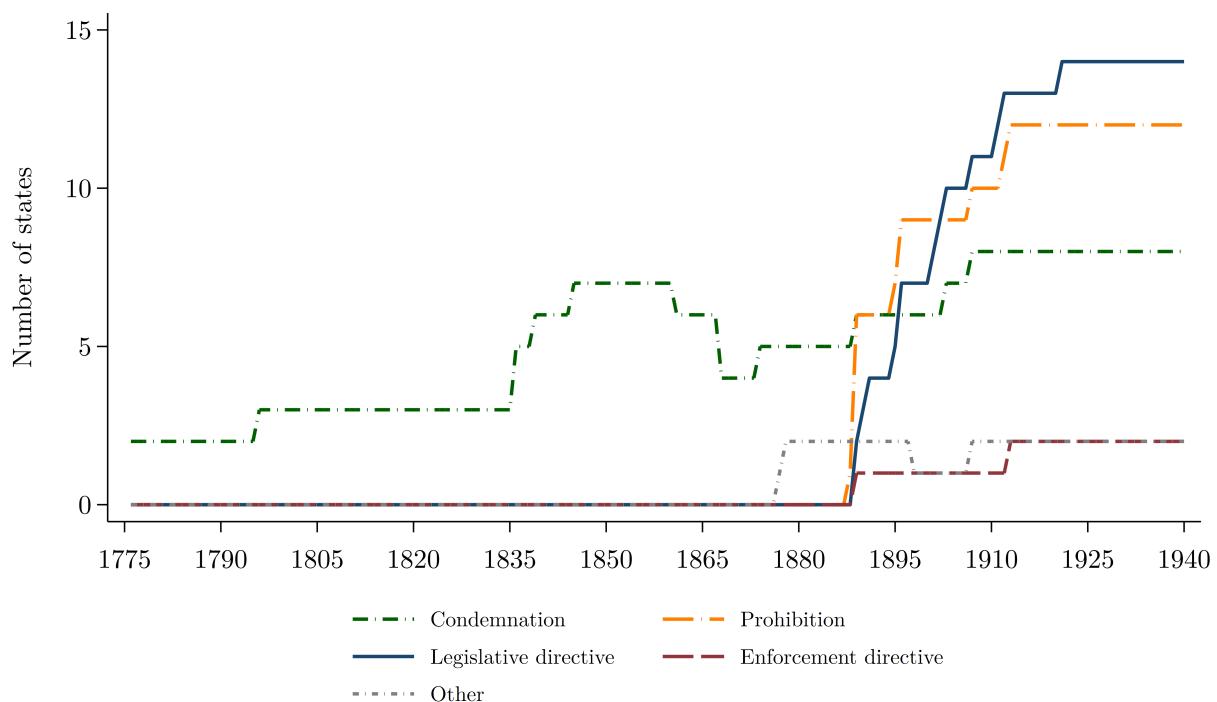
**Notes:** This table tallies the number of states that had antitrust statutes and/or anti-monopoly constitutional provisions before the Sherman Act was enacted on July 2, 1890. The 48 contiguous “states” are included in this tally, though some (Idaho, Wyoming, Utah, Oklahoma, New Mexico, and Arizona) were still territories at the time of the Sherman Act’s passage.

**Figure A7: Adoption of State Antitrust Statutes and Adoption of Anti-Monopoly Provisions in State Constitutions, 1776-1930**



**Notes:** This figure illustrates the relationship between adoption of an antitrust statute and adoption of an anti-monopoly constitutional provision, among states that adopted both of these measures.

**Figure A8: Contents of Anti-Monopoly Provisions in State Constitutions, 1776–1940**



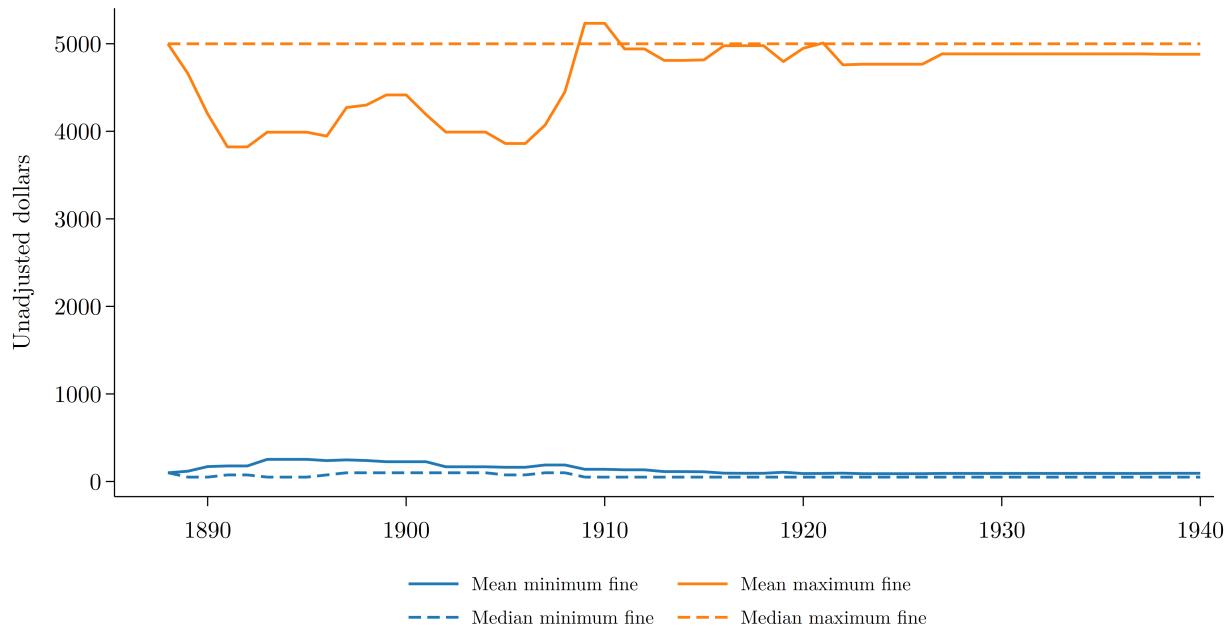
**Notes:** This figure illustrates the number of states with various kinds of anti-monopoly provisions in their state constitutions between 1776 and 1940. See table A2 for definitions of these classes of provisions.

**Table A2: Contents of Anti-Monopoly Provisions in State Constitutions**

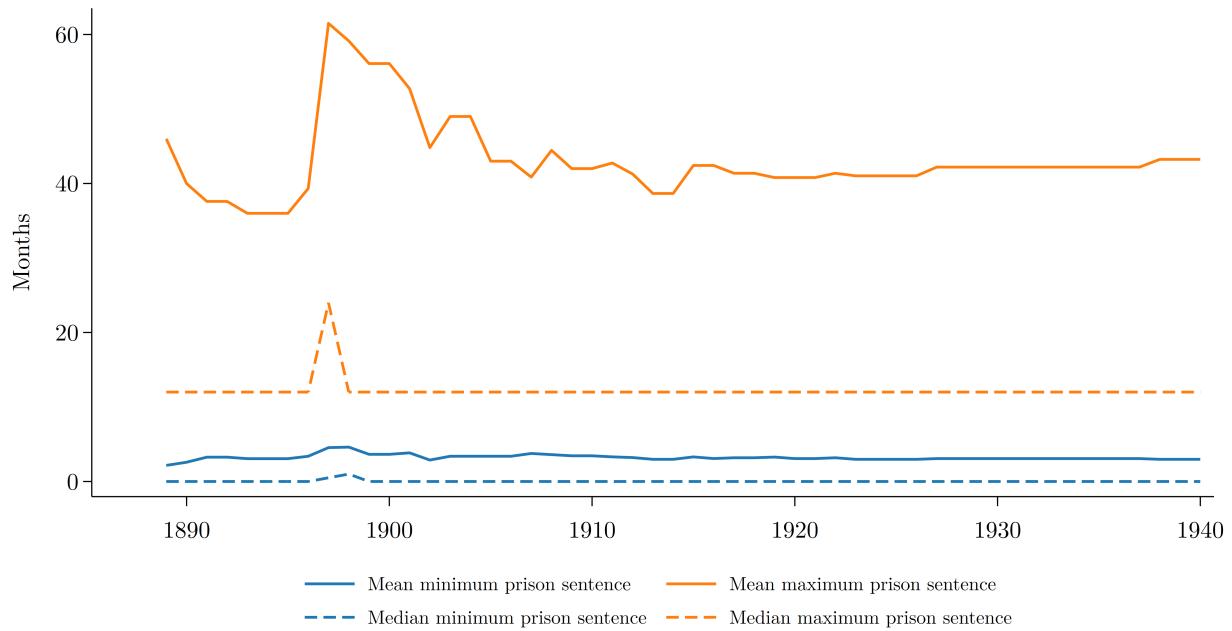
Type	Definition
Condemnation	Decries the existence of monopolies.
Prohibition	Declares that combinations, trusts, and/or other restraints of trade are illegal.
Legislative directive	Orders the state's legislature to pass laws to regulate monopolies.
Enforcement directive	Designates a state officer, such as the Attorney General, to enforce the constitutional provision against monopolies.
Other	Deals with competition policy in a manner not covered by the above categories.

**Figure A9: Fines and Prison Sentences Under State Antitrust Laws, 1888-1940**

*Panel A: Fines*



*Panel B: Prison Sentences*



**Notes:** This figure illustrates the fines (Panel A) and prison sentences (Panel B) states described in their antitrust statutes. Among states with statutory minimum and maximum fines, mean and median values of these minimum and maximum fines are provided in unadjusted dollars in Panel A. Among states with statutory minimum and maximum prison sentences, mean and median values of these sentences are provided in months in Panel B.

**Table A3: Descriptive Statistics for Ever- and Never-Treated States, 1880**

	(1) Ever-Treated	(2) Never-Treated	(3) Difference
Population (thousands)	1192.75 (1038.23)	767.10 (1348.72)	425.657 (410.928)
Urban share of population	0.216 (0.166)	0.301 (0.236)	-0.086 (0.067)
Male share of population	0.536 (0.065)	0.541 (0.068)	-0.005 (0.025)
Foreign-born share of population	0.151 (0.120)	0.168 (0.117)	-0.016 (0.045)
Agricultural revenue per capita	42.80 (16.83)	43.66 (19.77)	-0.859 (6.489)
School expenditures per capita	1.768 (1.153)	1.935 (0.973)	-0.167 (0.418)
Railroad miles per 1,000 square miles	57.33 (62.07)	77.70 (70.45)	-20.371 (23.745)
Average value of farmland (millions)	18.39 (15.36)	26.00 (15.99)	-7.611 (5.768)
Newspapers per 10,000 population	2.43 (1.19)	2.83 (1.53)	-0.393 (0.471)
Personal income per capita	187.87 (115.03)	241.61 (142.49)	-53.737 (44.951)
Grange chapters per 1,000,000 population (1887)	591.47 (369.20)	398.73 (448.36)	192.745 (145.280)
Greenback vote share (1884)	0.033 (0.030)	0.011 (0.009)	0.021 (0.018)
Mean state HHI (unweighted)	0.418 (0.109)	0.476 (0.115)	-0.059 (0.041)
Mean state HHI (weighted by value added)	0.119 (0.157)	0.239 (0.156)	-0.121** (0.059)
Mean state HHI (weighted by revenue)	0.134 (0.098)	0.226 (0.163)	-0.093** (0.042)
Share of manufacturing employment in nationally monopolized industries	0.015 (0.021)	0.091 (0.175)	-0.077*** (0.029)
Observations	36	9	45

**Notes:** Columns (1) and (2) report means and standard deviations for ever-treated and never-treated states, respectively (standard deviations in parentheses). Column (3) shows the difference in means, with standard errors in parentheses. Some variables are only consistently available at the state level in 1880, so each observation represents a single state-level mean; the sample size thus differs from that in the regression analysis, which uses disaggregated data. HHI is the Herfindahl–Hirschman Index and “nationally monopolized” industries are those with a national HHI over 0.18. The source for the first nine measures is the National Historical Geographic Information System (Manson et al. 2022). State-level income estimates are from Easterlin 1960. Grange chapter data are courtesy of the National Grange. Greenback vote shares are from Inter-University Consortium for Political and Social Research (1999). I computed the last four measures using data from the 1880 census of manufactures (Hornbeck et al. 2024). Oklahoma, North Dakota, and South Dakota are excluded due to data limitations. \*\*\*, \*\*, and \* denote statistical significance at the 1, 5, and 10 percent levels, respectively.

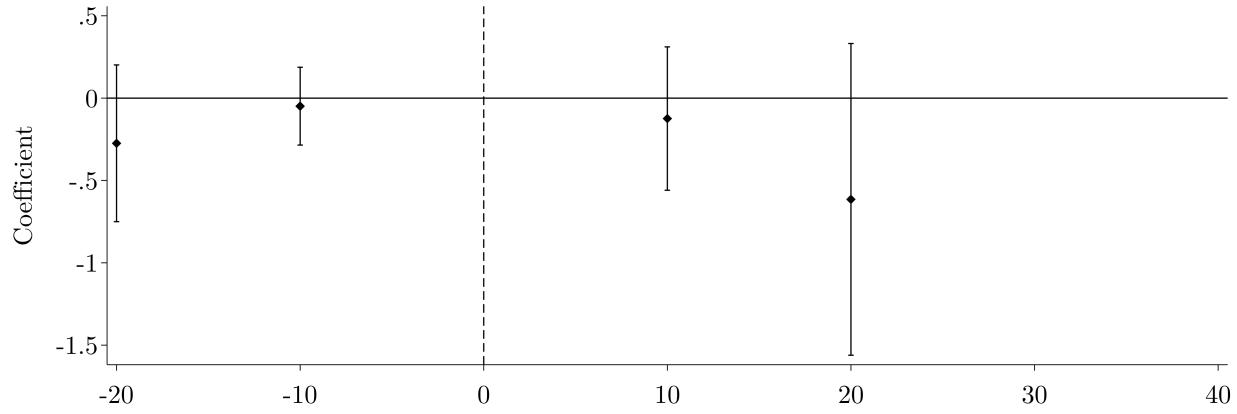
**Table A4: Descriptive Statistics for Early- and Late-Treated States, 1880**

	(1) Early-Treated	(2) Late-Treated	(3) Difference
Population (thousands)	1523.36 (1108.22)	862.14 (871.57)	661.220* (332.315)
Urban share of population	0.176 (0.130)	0.255 (0.191)	-0.078 (0.054)
Male share of population	0.515 (0.019)	0.557 (0.087)	-0.042* (0.021)
Foreign-born share of population	0.125 (0.107)	0.177 (0.130)	-0.052 (0.040)
Agricultural revenue per capita	48.88 (15.85)	36.73 (15.92)	12.147** (5.296)
School expenditures per capita	1.597 (1.111)	1.940 (1.200)	-0.342 (0.385)
Railroad miles per 1,000 square miles	47.39 (36.24)	67.28 (80.06)	-19.889 (20.714)
Average value of farmland (millions)	15.78 (11.47)	21.00 (18.43)	-5.222 (5.116)
Newspapers per 10,000 population	2.217 (1.016)	2.649 (1.344)	-0.432 (0.397)
Personal income per capita	139.10 (53.00)	236.64 (139.28)	-97.533** (35.124)
Grange chapters per 1,000,000 population (1887)	725.13 (402.58)	449.46 (275.96)	275.669** (120.911)
Greenback vote share (1884)	0.034 (0.033)	0.031 (0.028)	0.003 (0.018)
Mean state HHI (unweighted)	0.390 (0.064)	0.447 (0.138)	-0.057 (0.036)
Mean state HHI (weighted by value added)	0.077 (0.176)	0.163 (0.125)	-0.087 (0.052)
Mean state HHI (weighted by revenue)	0.103 (0.061)	0.166 (0.119)	-0.063* (0.032)
Share of manufacturing employment in nationally monopolized industries	0.010 (0.011)	0.020 (0.027)	-0.011 (0.007)
Observations	18	18	36

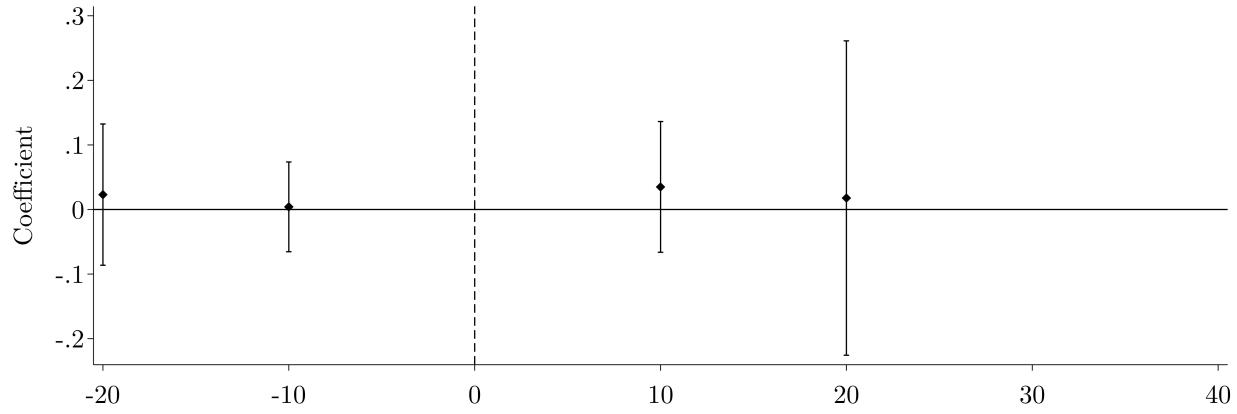
**Notes:** Columns (1) and (2) report means and standard deviations for “early-treated” states, which adopted a statute before 1895, and “late-treated” states, which adopted a statute in 1895 or later, respectively (standard deviations in parentheses). Column (3) shows the difference in means, with standard errors in parentheses. Some variables are only consistently available at the state level in 1880, so each observation represents a single state-level mean; the sample size thus differs from that in the regression analysis, which uses disaggregated data. HHI is the Herfindahl-Hirschman Index and “nationally monopolized” industries are those with a national HHI over 0.18. The source for the first nine measures is the National Historical Geographic Information System (Manson et al. 2022). State-level income estimates are from Easterlin 1960. Grange chapter data are courtesy of the National Grange. Greenback vote shares are from Inter-University Consortium for Political and Social Research (1999). I computed the last four measures using data from the 1880 census of manufactures (Hornbeck et al. 2024). Oklahoma, North Dakota, and South Dakota are excluded due to data limitations. \*\*\*, \*\*, and \* denote statistical significance at the 1, 5, and 10 percent levels, respectively.

**Figure A10: Event Study Results for Productivity Outcomes**

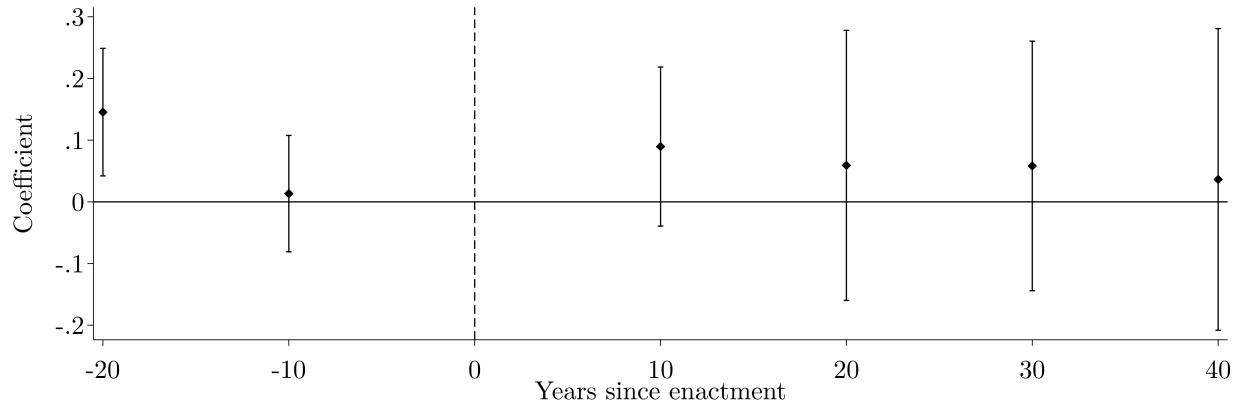
*Panel A: Log Residual Value-Added TFP with Capital*



*Panel B: Log Levinsohn and Petrin (2003) TFP with Capital*



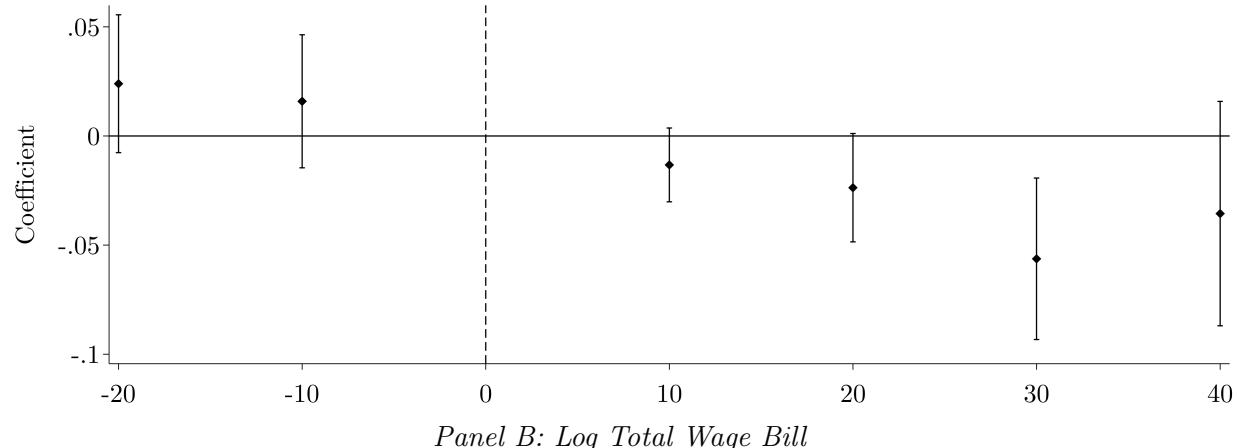
*Panel C: Log Levinsohn and Petrin (2003) TFP with Establishments*



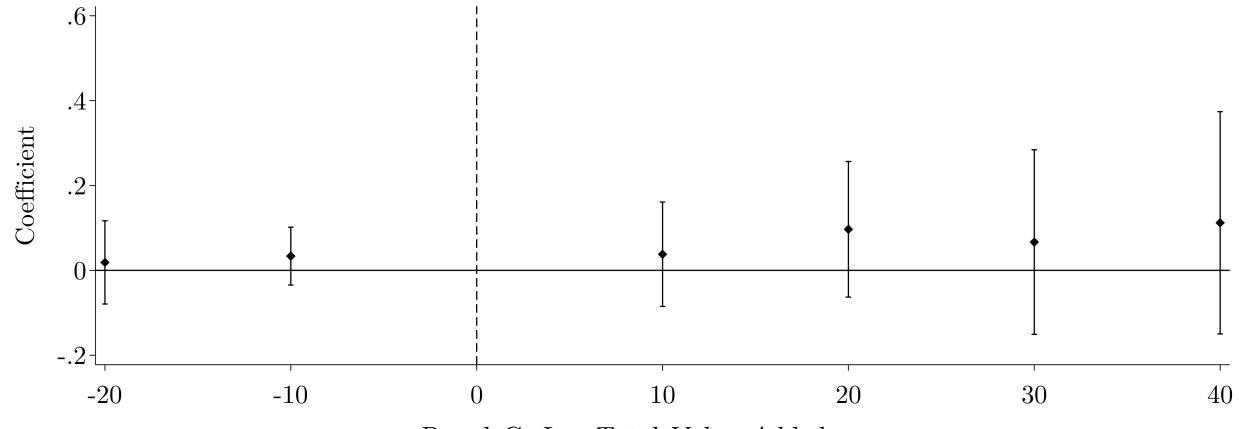
**Notes:** Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

**Figure A11: Event Study Results for Labor Share of Value Added and Components**

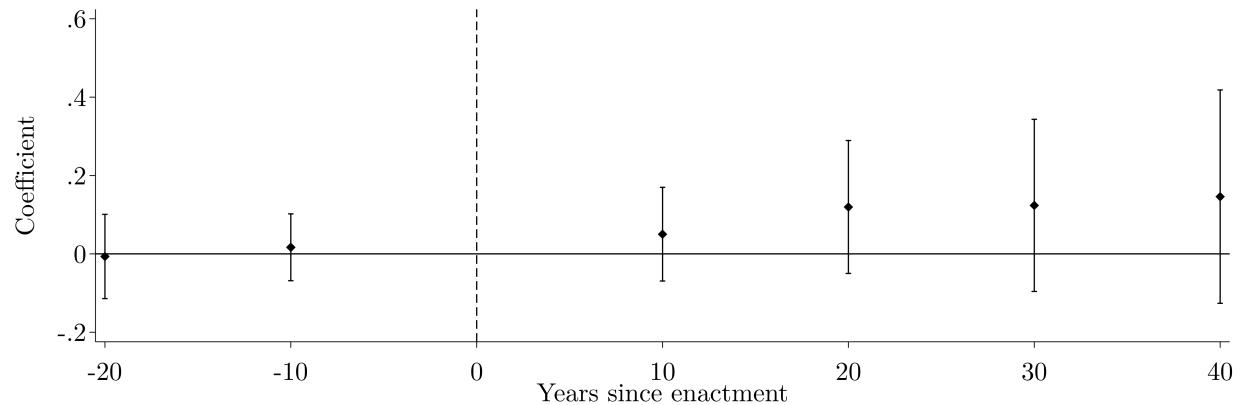
*Panel A: Log Labor Share (of Value Added)*



*Panel B: Log Total Wage Bill*

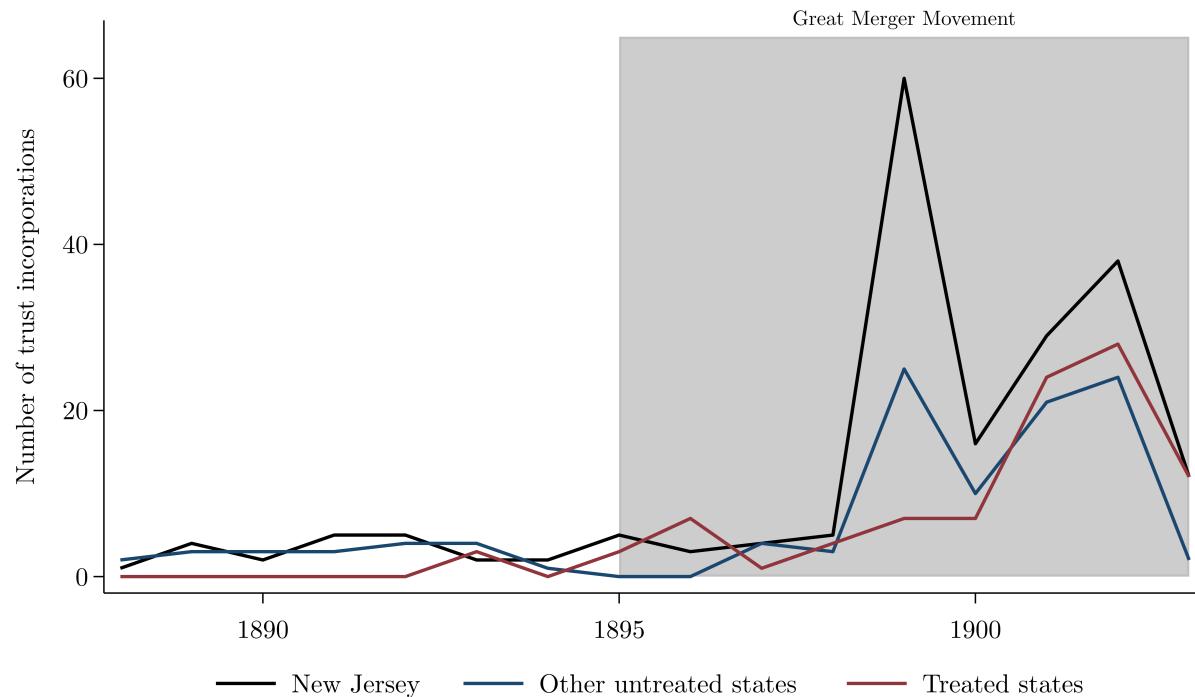


*Panel C: Log Total Value Added*



**Notes:** Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state. State-by-industry and year-by-industry fixed effects are also included.

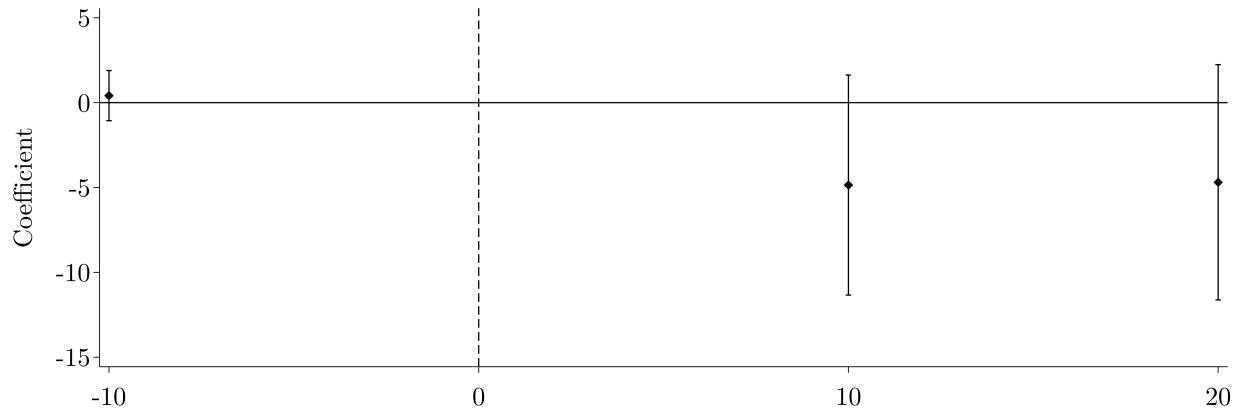
**Figure A12: Trust Incorporations by Treatment Status, 1888-1903**



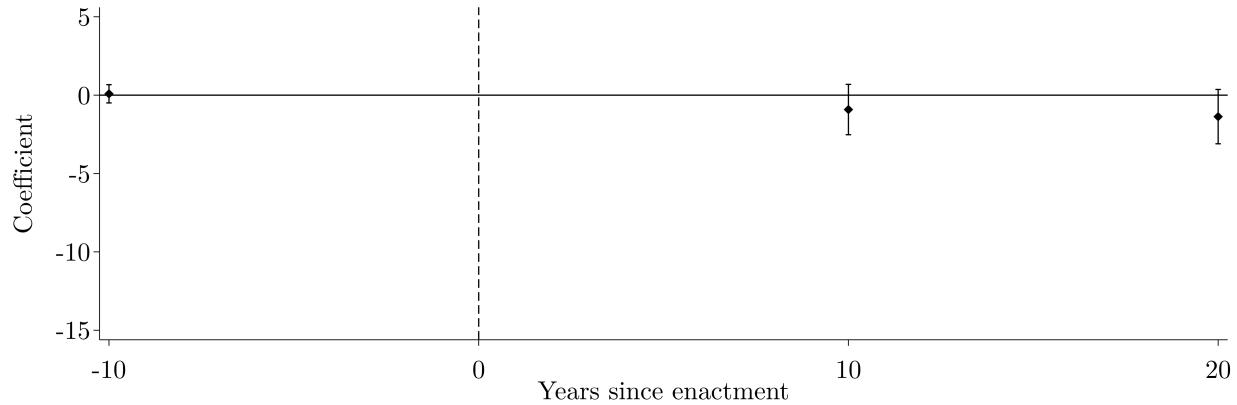
**Notes:** This figure illustrates the number of trusts incorporating over time, by the contemporaneous treatment status of the state where they chose to incorporate. Data are from Moody (1904).

**Figure A13: Effect of Enactment on Number of Trust Incorporations**

*Panel A: All states*



*Panel B: All states except New Jersey*



**Notes:** The dependent variable is the number of trust incorporations by state and decade. Data are from Moody (1904). Estimates are obtained using the stacked difference-in-differences method. Bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. States whose antitrust laws were overturned or repealed are excluded. Controls capture the population, median occupational score, estimated personal income, and literacy rate of each state.

## B Methodology for Identifying and Coding State Antitrust Laws

To identify all state antitrust statutes in effect between 1860 and 1940, I developed a list of common words and phrases appearing in state antitrust statutes other scholars had already identified.<sup>44</sup> Then, I used HeinOnline, a popular online database for legal research, to search state session laws and state legal codes for these terms. The specific search terms I used are **anti-trust, antitrust, unfair trade practices, restraint of trade, pools, pooling, monopoly, monopolies, conspiracy against trade, combination, price fixing, fix the price, substantially lessen competition, and unreasonably restrain trade.** I used the Boolean operator “or” in my searches to ensure that session laws and historical codes containing *any* of these terms would be returned. Despite returning many false positives, which I tossed out upon inspection, this approach allowed me to cast a wide net and reduce the chance of missing any relevant laws.

I considered any state statute outlawing restraints against trade, monopolization, horizontal price fixing, horizontal output restrictions, trustee control of corporations, anticompetitve stock purchases, refusals to deal, or some combination thereof to be a state antitrust statute. Table B1 defines each of these acts. Price discrimination, tying, predatory pricing, and retail price maintenance are notably excluded from the list of infractions I counted. In my review, I found that state statutes prohibiting these forms of anticompetitive conduct were often enacted independently of a general antitrust statute prohibiting more basic forms of anticompetitive conduct, such as price fixing. As a result, I excluded price discrimination, tying, predatory pricing, and retail price maintenance from my analysis to avoid characterizing, for example, a state that did not prohibit price fixing—but did prohibit price discrimination—as having an antitrust statute. Further, to avoid counting some sections of a chapter or title but not others, I regarded whole chapters or titles as antitrust statutes as long as they contained at least one eligible element (e.g., a prohibition of monopolization), even if they contained one or more sections with ineligible elements (e.g., a prohibition of price discrimination). I also did not consider statutes prohibiting price gouging or profiteering to be antitrust laws. Further, I did not count statutes clarifying that certain groups (e.g., labor unions or agricultural associations) did not constitute trusts. As described in Section 3, I do not regard statutes applying to a single industry, rather than to commerce more generally, as antitrust statutes in this paper. However, I do consider statutes that apply to “necessities of life” to be antitrust statutes given the breadth of this term.<sup>45</sup> I coded laws as in effect the year they were enacted, regardless of the exact enactment date. Further, in cases where a law was repealed or overturned, I coded the law as inactive in the year it was repealed or overturned.<sup>46</sup>

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<sup>44</sup>For example, Forrest (1896) and Halle (1899) are two early works on state and federal antitrust laws that served as a helpful starting point.

<sup>45</sup>Connecticut’s 1911 antitrust law is one example of a law containing this language. Act of August 15, 1911, Chapter 185, 1911 Connecticut Acts 1461.

<sup>46</sup>For example, if a law had been passed in 1900 and overturned in 1903, I would have coded the law as in effect

Figure B1: Bureau of Corporations Internal Memorandum on State Antitrust Laws

	I.	II.	III.	IV.	V.
Alabama	a,b,c,d,e.		a.		f
Alaska	i,k.	b.	b,e.	a,b,e	
Arizona	i,k.	b.	b,e.	a,b,e	
Arkansas	b,c,d.	f	a,c,d,g,b,i	a,b,e	a
California			Legislation against control of live stock business only.		
Colorado	No legislation.				
Connecticut	No legislation.				
Delaware	No legislation.				
Dist. Columbia	i,k.	b.	b,e.	b.	
Florida			Legislation against control of live stock and fresh meat only.		
Georgia	<del>1. b,c,d,e,f,g,h,i,j,k,l,m,n,p,q,r,s,t,u,v,w,x,y,z</del> no legislation				
Hawaii	i,k.	b.	b,e.	b.	
Idaho	b,c,(constitution)				
Illinois	b,c,e.	a,d,g	a,b,f,g	a,b,d	a,b,d
Indiana	<del>s,p,b,e,f,g,h,i,j,k,l,m,n,p,q,r,s,t,u,v,w,x,y,z</del>	a,b,d,f	a,b,c,d,g	a,b	f
Ind. Ter.	i,k.	b.	a,b,e.	b.	
Iowa	a,b,c,d,e.	a,d.	a,b,c,f	a,b	f
Kansas	<del>a,b,c,d,e,f,g,h,i,j,k,l,m,n,p,q,r,s,t,u,v,w,x,y,z</del>	a,b,c,d	a,b,c,d,f	a,b	b,d
Kentucky	b,c,e.	a,d.	a,b,c,f	b	b
Louisiana	<del>a,k,o,p,q,r,s,t,u,v,w,x,y,z</del>	a	a,b,c,f,g		
Maine	p. l,p		a,e.		a
Maryland	No legislation.				
Massachusetts	f.		a,b,f		
Michigan	a,b,c,f,l,p	a,b	a,b,c,d,f	a,b,c,d	
Minnesota	<del>a,b,c,f,l,m,n,p,q,r,s,t,u,v,w,x,y,z</del>	a,b,f,h	a,b,c,f,g	a,b,c,d	e
Mississippi	a,b,c,e,f,g,h,i,j,k,l,q,r	a,b,f,h,k	a,b,c,g	a,b	<del>h,v,d</del>
Missouri	<del>a,b,c,d,e,f,g,h,i,j,k,l,m,n,p,q,r,s,t,u,v,w,x,y,z</del>	a,b,d,f	a,b,c,d	a,b	a,b,d,l,m,n
Montana	b,g,e,a,b,f,i,k				
Nebraska			Laws against combinations controlling grain, stock, lumber and coal.		
Nevada	No legislation.				
New Hampshire	No legislation.				
New Jersey	No legislation.				
New Mexico	b,c,i,k.	a,b,d	a,b,e.	b	
New York	b,c,f,k	<del>a,b</del>	a,b,c,g	a	
No. Carolina	a,b,f,g	<del>a,b</del>	a,b,c,g	a	
No. Dakota	b,c,f,p	<del>a,b</del>	a,b,c,f	a	
Ohio	a,b,c,f,i,p	<del>a,b</del>	a,b,c,d	a,b,c,d	IC
Oklahoma	b,f,h,i,k.	<del>a,b</del>	a,c,b,e,f	b	
Oregon	a.				
Pennsylvania	No legislation.				
Phil. Isds.	No legislation.				
Porto Rico(?)	i,k.	b.	b,e.	b.	
Rhode Island	No legislation.				
So. Carolina	a,b,c,d,e,f,l,g,h	<del>a,b</del>	a,b,c,d,f	a,b	b
So. Dakota	<del>a,b,c,f,h,i,j,m,y,g</del>	<del>b,h</del>	a,b,f,g	a,b,c	
Tennessee	b,c,f,k	<del>a,b,c,g</del>	a,b,c,g	a,b,c	
Texas	<del>a,b,c,d,e,f,i,l,m,n</del>	<del>a,b</del>	a,b,c,f	a,b	c
Utah	<del>a,b,c,g,e,g</del>	<del>b</del>	a,b,c,f,g	a,b	
Vermont	No legislation.				
Virginia	No legislation.				
Washington	<del>a,b,c,(const.)</del>	Legislation against combinations to control market			
West Va	<del>a,b,c,f,g,h,i,j,k,l,m,n,p,q,r,s,t,u,v,w,x,y,z</del>	<del>a,b</del>	<del>a,c</del>	<del>a,f</del>	<del>a,h,i</del>
Wisconsin	No legislation.				
Wyoming	No legislation.				
West Virginia	No legislation.				

**Notes:** This photograph depicts a page from a 1903 internal memorandum on state antitrust laws authored by staff at the Bureau of Corporations. Each column represents some aspect of the state's antitrust law. For example, column I tallies the acts declared illegal under each state's antitrust law, where each letter represents a different illegal act and the meaning of each letter is defined elsewhere in the document. I took this photograph during a visit to the National Archives.

**Table B1: Illegal Acts Under State Antitrust Laws**

Illegal Act	Definition
Restraint of trade	To restrict competition or restrain trade in the production, manufacture, or sale of an article or commodity.
Monopolization	To monopolize or to attempt to monopolize the production, manufacture, or sale of an article or commodity.
Horizontal price fixing	To form a combination to control the price of an article or commodity.
Horizontal output restriction	To form a combination to limit the quantity of any article or commodity to be produced, manufactured, or sold.
Trustee control of corporations	To sell trust certificates establishing trustee control of several firms that would otherwise compete.
Anticompetitive stock purchases	When one firm buys shares or stock in another firm for the purpose of restricting competition.
Refusal to deal	To refuse to sell because purchaser is not a combination member or to refuse to deal with customers or suppliers who transact with a competitor.

I also compared my database to other scholars' compilations of state antitrust laws to ensure accuracy. For example, Forrest (1896), Halle (1899), Bureau of Corporations (1915), and Works Progress Administration (1940) are contemporaneous works on state and federal antitrust laws. Any time I encountered an accounting of state antitrust statutes that disagreed with my own, I reviewed the differences in detail to confirm my understanding was correct. I also compared my database to internal memoranda on state antitrust laws from the now-defunct Bureau of Corporations. Figure B1 provides an example of one such memorandum. These documents were particularly helpful because they illustrated how contemporaneous regulators at the Bureau of Corporations viewed state antitrust law. I obtained copies of these documents during a visit to the National Archives in August 2022.

After identifying state antitrust statutes in effect between 1860 and 1940, I analyzed the content of these laws. For example, I coded damages authorized by state antitrust statutes as actual damages, double damages, or treble damages. In cases where the damages plaintiffs could seek differed according to the antitrust violation, I coded the larger allowable damages.<sup>47</sup> I also only

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in 1900, 1901, and 1902. In 1903, I would have again coded the law as inactive.

<sup>47</sup>For example, a 1907 amendment to Indiana's antitrust statute allowed treble damages to be sought for injury due to restraint of trade, horizontal price fixing, horizontal output restriction, or monopolization. Meanwhile, an 1899 Indiana law continued to set maximum damages equal to the "costs [accrued to injured persons] and a reasonable attorney's fee" for injury due to refusals to deal. In 1907 and later, I code the damages authorized by Indiana's antitrust statute as treble damages, even though the 1899 statute continued to set damages for refusals to deal at a lower amount. Act of March 3, 1899, Chapter 148, §4, 1899 Indiana Acts 257. Act of March 11, 1907, Chapter 243, §7, 1907 Indiana Acts 490.

coded statutes as declaring antitrust violations to be torts when the statute empowered individual people or corporations harmed by anticompetitive conduct to sue under the antitrust statute. If the statute, for example, only empowered the state's Attorney General to sue, I did not count the statute as declaring antitrust violations to be torts. One special case comes from Alabama's 1919 antitrust law, which allowed "such damages [to] be recovered as the jury [saw] fit to assess."<sup>48</sup> This language suggests damages could theoretically attain any value. However, since I encountered no other instances of states allowing effectively unlimited damages, I coded this statute as allowing the next-highest level of damages I coded (treble damages).

Coding the criminal penalties described in state antitrust statutes was mostly straightforward, but some rules I applied are as follows. In cases where a law described one set of minimum and maximum fines for an offender's first violation, and a different set of minimum and maximum fines for an offender's second violation, I counted the first set of fines in my coding. Further, though some states set different minimum and maximum fines for corporate offenders and individual offenders, I did not distinguish between the two fine types in my coding. Instead, I coded minima as `min {minimum fine for corporations, minimum fine for individuals}` and maxima as `max {maximum fine for corporations, maximum fine for individuals}`.<sup>49</sup> Similarly, in cases where fines differed according to the antitrust violation, I determined minima and maxima across all violations.<sup>50</sup> Since I counted prison sentences in months, I coded 30-day minimum prison sentences as one month and one-year minimum prison sentences as 12 months. I also coded 24-hour minimum prison sentences as 1/30 or 0.0333 months.<sup>51</sup>

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<sup>48</sup> Act of September 30, 1919, Chapter 741, 1919 Alabama Acts 1088.

<sup>49</sup> One exception to this rule comes from an 1890 Iowa law. In this case, I coded the individual fines rather than the corporate fines because the statute sets corporate fines as between 1 percent and 20 percent of the firm's capital stock. Whether this amounted to more or less than the individual fine would have depended on the firm. Act of May 6, 1890, Chapter 28, 1890 Iowa Acts 41.

<sup>50</sup> For example, an 1897 Indiana law established a minimum fine of \$100 for price fixing, while an 1899 Indiana law established a minimum fine of \$50 for refusing to deal. I thus coded Indiana's minimum fine for antitrust violations as \$100 in 1897 and 1898 and \$50 in 1899 and the years thereafter (until 1907, when the Indiana General Assembly adopted a wholly new antitrust statute). Act of March 5, 1897, Chapter 104, 1897 Indiana Acts 159. Act of March 3, 1899, Chapter 148, 1899 Indiana Acts 257. Act of March 11, 1907, Chapter 243, 1907 Indiana Acts 490.

<sup>51</sup> For example, Montana's 1909 antitrust law describes a 24-hour minimum prison sentence. Act of March 6, 1909, Chapter 97, 1909 Montana Acts 127.

## C Food Retail Price Analysis

Although not the central focus of the paper, this appendix presents a supplementary analysis of how state antitrust laws affected retail food prices. To facilitate this analysis, I digitized and assembled a panel of data on the prices consumers paid at retail stores for various foods in cities across the United States. The data span 1851 to 1911, with some gaps, and are drawn from Weeks (1886) and three subsequent reports published by the Department of Commerce and Labor.<sup>52</sup> The foods covered in this dataset (see Figure C1) reflect the consumption patterns of typical households during the study period and serve as a meaningful proxy for the cost of living in the late nineteenth and early twentieth centuries; Weeks (1886, p. 1) aptly describes them as “the chief necessities of life.”

To estimate the average effect of state antitrust law enactment on food prices, I use a stacked difference-in-differences design that accommodates variation in treatment timing across states.<sup>53</sup> For each of the 30 food items, I construct a separate dataset that stacks treated states around their year of adoption and includes both never-treated and not-yet-treated states as controls. The estimating equation is:

$$\ln(P_{st}) = \beta_0 + \beta_1(statelaw_s \times post_{st}) + \Gamma X_{st} + \delta_s + \delta_t + \varepsilon_{st} \quad (5)$$

where  $s$  denotes a state and  $t$  denotes the year. The log retail price of a given food item is denoted by  $\ln(P_{st})$ , whether state  $s$  adopted an antitrust law is indicated by  $statelaw_s$ , and the time period after which a state antitrust law was adopted is denoted by  $post_{st}$ . The model also includes state fixed effects  $\delta_s$  and year fixed effects  $\delta_t$ , as well as a vector of time-varying controls  $X_{st}$ . Specifically,  $X_{st}$  includes state-level personal income (to account for changes in consumer purchasing power), and county-level population (to capture demand and urbanization), median occupational score (a proxy for local economic development), and the share of employment in agriculture (to account for variation in food production and consumption patterns). Standard errors are clustered at the state level. The coefficient  $\beta_1$  captures the average treatment effect on the treated for each food item, netting out time-invariant state characteristics, national trends, and key observable covariates.

The estimated effects of state antitrust laws on retail food prices are mixed overall, but the results suggest meaningful price declines for meat products and point to a substantial reduction in the cost of overall household expenditures on food. Of the 30 foods shown in Figure C1, six exhibit statistically significant effects at the 95 percent confidence level. Four of these six—bacon, fresh beef roast, fresh lamb or mutton, and fresh veal—show negative and significant coefficients, suggesting that the enactment of state antitrust laws was associated with lower prices for these products. Notably, all four are meat products. Two additional meats—fresh beef steak and fresh chicken—

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<sup>52</sup>The sources for this dataset are Weeks (1886) and Department of Commerce and Labor (1904, 1906, 1912). These reports provide annual price data for the years 1851–1880, 1890–1903, 1904–1905, and 1907–1911, respectively. I digitized the latter three reports myself and thank Michael Haines for sharing digitized data from Weeks (1886).

<sup>53</sup>See Section 4 for more information about the stacked difference-in-differences design I employ.

also have negative coefficients that fall just short of statistical significance. Taken together, these results suggest that state antitrust laws may have been effective in reducing the prices of meat. This effect is plausibly linked to the intense public and political scrutiny of the Chicago meat packers in the late nineteenth century, which may have placed pressure on the leading firms to scale back price-fixing and other anticompetitive practices in response to the enactment of state antitrust laws.<sup>54</sup> Consistent with this pattern, a “representative food basket”—i.e., a weighted index of food retail prices based on household expenditure shares circa 1890, as documented by Haines (1989)—became approximately 27 percent cheaper following the enactment of a state antitrust law. While most individual food price effects are imprecisely estimated, the index-level result points to a potentially broad decline in consumer food spending.

These results should be interpreted with some caution. As discussed in Section 2.3, the impact of an antitrust law depends in part on pre-existing market conditions. If the market for a given food was already competitive before the enactment of a state antitrust law, substantial price effects would not be expected. Accordingly, the many null results in Figure C1 is in line with the view that antitrust laws had limited scope to reduce prices in already competitive markets. Two results—positive coefficients for beans and for salted fish—run counter to this pattern, though a small number of counterintuitive results may arise by chance given the number of outcomes tested. On the other hand, laws introduced in markets that were initially cartelized or otherwise uncompetitive are more likely to generate price reductions through increased competition. One notable case is sugar, a product from a notoriously concentrated industry. The Sugar Trust, formed in 1887, controlled a dominant share of national refining capacity and was a frequent target of criticism and regulatory scrutiny.<sup>55</sup> Despite its notoriety, no state successfully sued the Sugar Trust under their antitrust statute.<sup>56</sup> Although the coefficient for sugar is negative, it is not statistically significant, offering a cautionary example of a highly concentrated industry where antitrust law may not have translated into measurable benefits for consumers—perhaps due to a lack of enforcement.

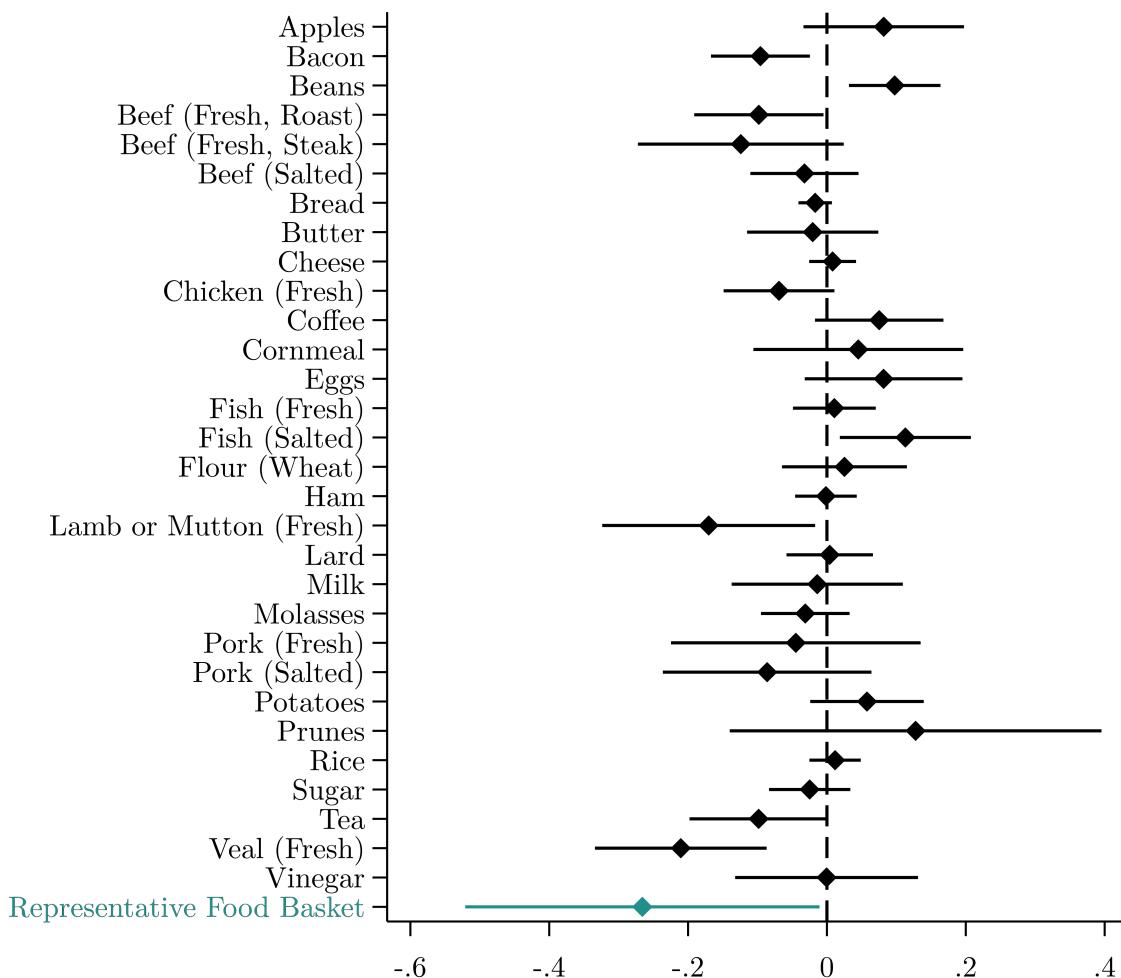
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<sup>54</sup>In the late nineteenth century, meatpacking was dominated by a few large Chicago-based firms. By 1890, the four largest—Armour, Swift, Morris, and Hammond—accounted for about 89 percent of the national supply of dressed beef (Yeager 1981, p. 67). Known collectively as the “Beef Trust,” these firms processed a wide range of meats, not just beef, and were often accused of price-fixing, market division, and collusion with railroads. Public outrage over the Beef Trust made meatpacking a target of early state antitrust enforcement efforts. For example, in 1903, the Missouri Supreme Court found five major meat packers guilty of violating the state’s antitrust statute and ordered them to cease operations in the state, pay \$5,000 each in fines, and cover court costs. *State ex rel. Crow v. Armour Packing Co.*, 173 Mo. 356, 73 S.W. 645 (1903). Arkansas also brought several suits of a similar nature against the Beef Trust, including a notable case against Hammond that the U.S. Supreme Court decided in the state’s favor. *Hammond Packing Company v. State of Arkansas*, 212 U.S. 322 (1909). See Willis (2021) for a detailed account of Arkansas’s antitrust enforcement efforts during the Progressive Era. State-level enforcement actions, such as those by Missouri and Arkansas, may help explain the strong price effects observed for meat products.

<sup>55</sup>Henry Osborne Havemeyer, founder of the Sugar Trust, testified that 17 or 18 of 21 refineries belonged to the Trust, with the remainder accounting for about 30 percent of the national market (Industrial Commission 1900).

<sup>56</sup>New York did famously dissolve a sugar refining company for joining the Sugar Trust. This action, upheld in *People v. North River Sugar Refining Company*, was brought as a *quo warranto* proceeding under corporate law rather than using antitrust law. 121 N.Y. 582 (1890).

**Figure C1: Difference-in-Differences Results for Food Retail Prices**



**Notes:** Each coefficient in this figure represents a separate estimation of equation 5, where the dependent variable is the log retail price of the listed good. Estimated values of  $\beta_1$  from equation 5 are shown, and bars indicate 95 percent confidence intervals. Robust standard errors are clustered at the state level. Estimates are obtained using the stacked difference-in-differences method. The “representative food basket” refers to a price index constructed using the items consumed by a typical household around 1890, as documented by Haines (1989), where each food item is weighted by its share of household food expenditures. States whose antitrust laws were overturned or repealed are excluded. Controls capture state-level personal income and the county-level population, median occupational score, and share of employment in agriculture.