Title
Essays on the Economic Effects of Convict Labor in Modern U.S. History

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Publication Date
2018

UNIVERSITY OF CALIFORNIA

Los Angeles

Essays on the Economic Effects of Convict Labor in Modern U.S. History

A dissertation submitted in partial satisfaction

of the requirements for the degree

Doctor of Philosophy in Management

by

Mikhail Poyker

2018

ABSTRACT OF THE DISSERTATION

Essays on the Economic Effects of Convict Labor in Modern U.S. History

by

Mikhail Poyker
Doctor of Philosophy in Management
University of California, Los Angeles, 2018
Professor Paola Giuliano, Co-chair
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My dissertation contributes towards our understanding of effects that convict labor has on economic outcomes. It consists of three chapters. The first, "Economic Consequences of the U.S. Convict Labor System" studies the economic externalities of U.S. convict labor on local labor markets. Using newly collected panel data on U.S. prisons and convict-labor camps from 1886 to 1940, I show that competition from cheap prison-made goods led to higher unemployment, lower labor-force participation, and reduced wages (particularly for women) in counties that housed competing manufacturing industries. At the same time, affected industries had higher patenting rates. I find that the introduction of convict labor accounts for 16% slower growth in U.S. manufacturing wages. The introduction of convict labor also induced technical changes and innovations that account for 6% of growth in U.S. patenting in affected industries.

In my second chapter, "U.S. Convict Labor System and Racial Discrimination" I document that after the demise of the slavery and rise of crime after the end of the Civil War, convict labor system evolved in the United States in order to finance state penitentiary institutions. It provided monetary incentives to the police to arrest more people. Black and other minorities became an easy target for a police that used a variety of minor crime laws to increase the supply of

coerced labor. Using the geographical variation of convict labor camps in the United States in 1886 I show that counties exposed to a more severe exploitation of convict labor experienced higher rates of incarceration among minorities in 1920, and 1930. Moreover, after the abolishment of the old convict labor system in 1941, the racial discrimination in policing remained: the same variation of convict labor camps predicts excessive arrests of Black and Hispanic for non-violent crimes (drugs and vagrancy). To show that the results are causal I use the exogenous shock of first massive expansion of the U.S. convict labor system in 1870 that had happened when the National Prison Association was founded in Cincinnati, Ohio. I use distance to Cincinnati as an instrument for the value of goods produced by convict labor. It correlates with the likelihood of attending the Congress by the wardens of prisons, and cost of getting information about the profitability of convict labor. I perform a series of sensitivity checks and placebo tests to ensure that results are indeed causal.

In the third and last chapter, using historical distribution of the prison and convict labor camps in the United States, I study the long-run effect of convict labor on equality of opportunities. Convict labor negatively affected wages of low-skilled workers and had positive effects on firms in affected industries. I document that this reallocation of welfare from wage earners to capital owners had a long-lasting effect on equality of opportunities: intergenerational mobility of the bottom income quintile got worse, while it improved for the other quintiles.

The dissertation of Mikhail Poyker is approved.

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University of California, Los Angeles

2018

To My Wife and Cat

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Acknowledgments

I am especially indebted to my Committee Chairs Paola Giuliano and Romain Wacziarg, for their invaluable support and guidance throughout my PhD studies. I am extremely thankful for the support and advice from my other committee members Christian Dippel, Daniel Posner, Peter Rossi, and Nico Voigtländer. I am also grateful to Omer Ali, Maxim Ananyev, Sascha Becker, Kirill Borusyak, Dora Costa, Sebastián Edwards, Katherine Eriksson, James Fenske, Stefano Fiorin, Vasily Korovkin, Edward Leamer, Adriana Lleras-Muney, Alexey Makarin, Imil Nurutdinov, Sebastian Ottinger, Bruno Pellegrino, Ricardo Pérez-Truglia, Konstantin Platonov, Yuan Tian, Daniel Treisman, Till von Wachter, Melanie Wasserman, Meng Xu, conference participants at AEA 2017, ALEA 2017, All-California Labor Economics 2017, All-UC Economic History Group 2016, All-UC Economic History Group Graduate Student Workshop 2017, APSA 2016, Cliometric Society 2018, EHS 2018, UC Anderson GEM - BPP Haas Workshop 2017, and seminar participants at CERGE-EI, LSE Economic History, UC Irvine, UCLA, and UNSW for excellent suggestions. The author acknowledges generous financial support from the Economic History Association and the UCLA Ziman Center for Real Estateâs Rosalinde and Arthur Gilbert Program in Real Estate, Finance and Urban Economics. All remaining errors are mine.

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CHAPTER 1

Economic Consequences of the U.S. Convict Labor System

1.1 Introduction

Convict labor is still wide-spread, not only in developing countries but also among the world's most developed countries.[1] In 2005 U.S. convict-labor system employed nearly 1.4 million prisoners, among them 0.6 million worked in manufacturing (constituting 4.2% of total U.S. manufacturing employment).[2] Prisoners work for such companies as Wal-Mart, AT&T, Victoria's Secret, and Whole Foods, and their wages are substantially below the minimum wage, ranging from $0 to $4.90 per hour in state prisons.[3]

Convict labor may impose externalities on local labor markets and firms. Prison-made goods are relatively cheap. Companies that hire free labor find it harder to compete with prisons, especially in industries that rely on low-skilled labor. They face lower demand on their products, pushing down their labor de-

[1] In addition to convict labor, other types of coerced labor such as military labor, peonage, indentured labor, debt bondage, and sharecropping still exist (van der Linden and García (2016)). For example, in Egypt, the army employs conscripted soldiers in factories to produce an array of products, from flat-screen televisions and pasta to refrigerators and cars (See Al Jazeera, (2012)). China and Russia employ up to 2 million and 0.5 million convicts respectively. See Forbes (2013) and Research Foundation, Laogai (2006).

[2] Sources: Census of State and Federal Adult Correctional Facilities, 2005, and FRED.

[3] For more information on wages and companies working with prisons, see `www.prisonpolicy.org/reports/wage_policies.html` and Daily Kos (2010). The situation is not unique to the United States. For example, U.K. prisons "lease out" inmates to local firms allowing them to pay 6% of the minimum wage (The Guardian (2012)).

mand. Excess labor moves to industries not competing with prisons and overall wages decreased. Convict labor affects firms, too. Many (predominantly labor-intensive firms) go out of business, unable to compete with prison-made goods, even when they lower wages.[4] Finally, those affected firms that do not close have to innovate and adopt new technology, either to decrease their production costs, or to produce higher-grade goods that do not compete with prison-made goods.[5]

In this paper, I use a historical setting to evaluate the effect of competition with prison-made goods on firms and free workers. It is challenging to identify causal effects of convict labor in the contemporary setting, since the data on prison output are not available, and due to the embedded endogeneity problem. First, U.S. prisons are built in economically depressed counties under the assumption that they will provide jobs (e.g., guards) in the local labor market (Mattera and Khan (2001)). Second, contemporary convict-labor legislation is endogenous. For these reasons I rely on the historical setting, to identify the effects of convict labor. I digitize a dataset on U.S. convict-labor camps and prisons. Starting in the 1870s, states enacted laws that allowed convict labor, but the timing varied from state to state. Its introduction was unanticipated, both by firms and by prison wardens, who were suddenly in charge of employing prisoners within their institutions. Moreover, as all convict-labor decisions were determined at the prison level, subsequent changes in convict-labor legislation were exogenous to the choices of individual prison wardens. In addition, I use the fact that pre-convict-labor-era prisons were built without any anticipation that they would be used to employ prisoners. In comparison with contemporary prisons, old prisons were built in populated areas with higher wages and employment, which hinders my ability to find negative effects on local labor markets. Finally, the historical setting allows me to document long-run effects of convict labor in a developed country.

[4] Cost of convict labor is lower than reservation/minimum wages of free laborers.

[5] This is consistent with the evidence in Holmes and Stevens (2014), and Bloom et al. (2016).

To elicit the effect of prison-labor competition on the local labor market, I construct a county-decade panel data set spanning 1850 to 1950. I measure the exposure of each county to convict labor as the industry-specific value of convict-made goods in all U.S. prisons weighted by the county's industry labor share and by the distance from those prisons to the county centroid. This imposes two central assumptions: low labor mobility across counties, and iceberg costs of trade.

I estimate the effect of exposure to convict labor on manufacturing wages, employment outcomes, and patenting rates using ordinary-least-squares specification with fixed effects. While the panel dataset allows me to account for time- and county-invariant unobserved heterogeneity and state-specific time trends, three endogeneity concerns remain. First, there is an omitted-variable bias due to the endogenous choice of industry and the amount of goods produced by prisons. Second, prisons could be strategically located to earn higher profits for their states. Third, convict labor was used in industries where local labor unions were stronger and the wage growth rate was higher (Hiller (1915)).

To address these concerns, I employ an instrumental variable estimation. I use state-level variation in the timing of passage of convict-labor laws interacted with the capacity of prisons that existed before convict-labor laws were enacted to construct an instrument for the prevalence of convict labor. Prison production was determined by a prison's warden, and the state-level legislature can be considered exogenous. Old prisons were built without any anticipation that they would be used for production of goods; their locations were determined primarily by population size and urban share of population. Thus, conditional on factors important to the location of the old prisons, the interaction of convict-labor legislation and capacities of old prisons is likely uncorrelated with wardens' activity and possible strategic location of prisons constructed after convict-labor systems were enacted. I find that the introduction of convict labor in 1870-1886 accounts for 16% slower growth in manufacturing wages in 1880-1900, 20% smaller labor-force participation, and 17% smaller manufacturing employment share.[6] Comparing two counties, one at the 25th percentile and the other at the 75th percentile of exposure to convict labor, the more exposed county would on average experience a 2 percentage-point larger decline in mean log annual wages in manufacturing, a 0.9-percentage-point larger fall in manufacturing employment share, and a 0.6-percentage-point larger decline in labor-force participation.

While prison labor was used in quite a few industries, most prisons were producing clothes and shoes. Apparel and shoemaking industries employed mostly women, and they were the most affected by coerced labor. Female wages decreased 3.8 times more than those of men.

I also show that convict-labor shocks affected technology adoption. Comparing two counties, one at the 25th percentile and the other at the 75th percentile of exposure to convict labor, the more exposed county would be expected to experience a 0.6-standard-deviation larger number of registered patents in industries where prisoners were employed. I calculate that the introduction of convict labor accounts for 6% of growth in U.S patenting in affected industries.

Because forms of convict labor differed in the North and South, I analyze subsamples.[7] I show that results are mainly driven by the Northeastern and Midwestern states. For the Southern states, all coefficients remain significant, while the magnitudes of all effects are smaller.

I show that the results are robust to various model specifications and ways I construct the explanatory variable. Results hold if I use exposure to convict labor, weighted only by distance to prison (i.e. disregarding industry shares).

[6]The size of the effects of convict labor shock is comparable to the effects of the "China shock" (Autor et al. (2013)): it is half the effect of China shock in terms of labor-force participation, 1.5 times larger in terms of manufacturing employment share, and 2.5 times larger in terms of mean log wages in manufacturing.

[7]The North and the South differed both in terms of local institutions and industrial composition that resulted in adoption of different systems of convict labor (McKelvey (1936); Wilson (1933)).

I also demonstrate that results are not entirely driven by differences between counties with and without prisons: I find that results hold within the sample of counties with prisons. Then, comparing counties with prisons to counties adjacent to counties with prisons, and to second-order adjacent counties, I find that effects of convict labor decay with distance. Also, I find no effect on manufacturing outcomes when using as a placebo convict-labor output in farming. Further, I find no significant effect of convict labor on the number of patents in industries where prisoners were not employed. Finally, I employ firm-level repeated cross-section data for 1850-1880 from Atack and Bateman (1999) to show that firms in affected industries experienced larger decreases in wages. The firm-level data also suggest a decrease in the number of firms in affected labor-intensive industries.

My results relate to three broad economic literatures. I find that the problem of convict labor is similar to the discussion of low-skilled labor competition related to trade shocks (Autor et al. (2013, 2016a), and Holmes and Stevens (2014)). I find that local labor-market shocks come not only from foreign competition or technological progress but can arise from internal sources. Besides, my findings relate penitentiary policies to patterns of directed technological progress (Acemoğlu (2002, 2007), and Autor et al. (2016b)). I provide evidence in support of findings in Bloom et al. (2016) that firms increase patenting as a way to survive competition. Moreover, in contrast to these recent shocks, I estimate the long-run effects of competition coming from the convict labor system. While sociologists and criminologists thoroughly studied convict labor in the 20th century, only a few qualitative papers raised the topics of competition between prison-made goods and products created by free laborers (Roback (1984), McKelvey (1934), and Wilson (1933)).

The rest of the paper is organized as follows. Section 1.2 reviews the existing literature and relates this paper's contributions to it. Section 1.3 introduces the history of U.S. convict labor and the records of its competition with free labor.

Section 1.4 describes the data. Section 1.5 presents my identification strategy and estimation results. Section 1.8 assesses the possible impact of the contemporary U.S. convict-labor system and concludes.

1.2 Relationship and Contribution to the Literature

The paper contributes to the literature on Labor Competition. Previous work examined the effects of nation wide price shocks due to trade liberalization on wages and unemployment (Autor et al. (2013, 2016a)), the effects of in/out migration on local labor-market outcomes (Borjas (2003, 2015), Card (1990, 2001), Clemens et al. (2017), and Ottaviano and Peri (2012)), and the effects of technology shocks (Acemoğlu and Restrepo (2017)) on local labor-market competition. Here, I show that the penitentiary system can also be a source of local labor-market shocks. I find a significant effect of convict labor on both the county-industry level and the state-industry level. Also, like Holmes and Stevens (2014), I find that firms that relied on low-skilled, labor-intensive production suffered more than those that did not. Moreover, identification comes not only from timing and industrial composition variation but also from the spatial variation in prison locations.

My paper shares several components with Technology Adoption literature (Acemoğlu (2002, 2007), Acemoğlu and Finkelstein (2008), and Lewis (2011)). Previous studies (Aghion et al. (2016), Newell et al. (1999), and Popp (2002)) used energy' price shocks as a driver of energy-saving technological progress. Hanlon (2015) showed how British firms adjusted and evolved when the import of good-quality U.S. cotton stopped during the Civil War. Here, I show how competition with prison labor led to adoption of both new and existing technologies. My findings, however, span a longer time period than previous studies, and my identification comes from competition with prison labor rather than input-factor price shocks. I also show that direct technology change can happen not only due to changes in input factor demand (Acemoğlu (2002)), but due to price shocks. Finally, my paper contributes to the discussion if price shocks due to import competition affect firms patenting and R&D decisions (Autor et al. (2016b)), and supports findings in Bloom et al. (2016).

I also contribute to the public policy literature related to Penitentiary Policies. New U.S. prisons are generally located in economically depressed regions under the assumption that they will provide jobs (e.g., guards) in the local labor market (Mattera and Khan (2001)).[8] However, existing evidences suggest that prisons have either no effect or adverse effects on the local labor markets (Genter et al. (2013); Hooks et al. (2010); McElligott (2017), and Oppong et al. (2014)). I find, that convict labor that would be used in those prisons may worsen local labor market outcomes, thus overshadowing any possible positive effects. By providing evidence of adverse externalities that prison labor creates to free labor, I address discussion of mandatory work programs in contemporary prisons (Polinsky (2017), and Zatz (2008)). While convict labor may reduce budgetary burden on state and federal governments (Lynch and Sabol (2000)), and help (or not) rehabilitation of prisoners and their future employment opportunities (Bushway et al. (2003); Gomez et al. (2017); MacKenzie et al. (1995), and Maguire et al. (1988)), working conditions of prisoners and their wages should be more comparable to those of free laborers (Haslam (1994), Western and Beckett (1999), and Zatz (2009)) to prevent unfair competition.

The literature on Coercive Institutions, summarized by (among others) Acemoğlu and Wolitzky (2011) typically focuses on long-run effects. Dell (2010) and Lowes and Montero (2016) examined long-run adverse impacts of the forced labor on contemporary health and institutional outcomes in Peru and Bolivia, and in the Congo, respectively. Others (Acemoğlu et al. (2012), Buggle and Nafziger (2015),

[8] States even provide subsidies for private prison companies that open prisons in depressed counties.

Nunn (2008), and Kapelko et al. (2015)) have studied the economic consequences of coercive institutions on later institutional development. Nunn and Wantchekon (2011) point out their effect on social capital, and trust in particular. Here I contribute to the literature by first estimating short-run effects of convict labor and then estimating the long-run effect. In this sense, my paper mirrors the concept of Markevich and Zhuravskaya (2017) and Nilsson (1994), who looked at the immediate effects of abolishing of slavery/serfdom on contemporaneous outcomes.[9] The effect of coercive institutions is also related to previous studies that highlighted the importance of institutions and differences in the initial factor endowments in explaining the degree of inequality in wealth, human capital, and economic growth (Engerman and Sokoloff (2002, 2005), and Fujiwara et al. (2017)).

1.3 Convict Labor in the United States: Historical Background and Implications

While the history of the U.S. penitentiary system has been described in great detail by historians (Ayers (1984); Johnson and Wolfe (1996); McKelvey (1933, 1935, 1936, 1977); Sellin (1976)), the discrete topic of convict labor has been received less attention.[10] As penology evolved, the goal of prisons became rehabilitation through education (mostly spiritual) and manual labor.[11] Prisons started to ap-

[9]While I do not directly study the reasons behind abolishment of the convict labor system, an emerging set of papers sheds light on the reasons for the evolution and disappearance of coercive labor. Dippel et al. (2015) show that the predisposition in sugar suitability determined how coercive institutions evolved because of changes in sugar prices, while Ashraf et al. (2017) study the emancipation of Prussian serfs during the industrialization.

[10]Most of the literature is concentrated on the issue of convict labor in the Southern states (Blackmon (2009); Browning (1930); Carper (1966); Cohen (1976); Green (1949); Ledbetter (1993); Lichtenstein (1993, 1996); Oshinsky (1997); Pruitt (2001); Roback (1984); Shelden (1979); Taylor (1942); Walker (1988); Williams and Collins (1995); Worger (2004)). Only a few address more widespread Northern convict labor (Gildemeister (1978); Hiller (1915); Jackson (1927); McKelvey (1934)).

[11]New England settlers wanted to remedy moral failures of criminals by forcing them to perform hard labor, and since the creation of the first U.S. prison, East Penitentiary in Philadelphia, prisoners were employed. In the Pennsylvanian system, prisoners were confined to solitary cells

pear across the United States replacing jails as the primary location for confining criminals.

Prison labor was meant to become a source of income to offset states' expenditures for corrections. However, neither profits from non congregated labor under the Pennsylvanian system nor work in quarries near prisons contributed sufficiently to the states' budgets (Gildemeister (1978) pp. 16, 29). In 1818 a new type of penitentiary appeared in Auburn, New York, where prisoners were gathered during the day in a workshop and worked together. However, this system of labor (the Auburn system) required the presence of an outside contractor who would provide tools and equipment as well as foremen who would supervise and teach inmates the required skills.[12]

Nevertheless, prisons operated with massive losses. By 1870, only eight prisons across the U.S. (all in New York) operated with a modest net profit (Department of Labor (1900)). Historians of the penitentiaries are unanimous about the reasons behind this failure (Gildemeister (1978); McKelvey (1936)). First and foremost was the prisoners' lack of skill. Most were uneducated and lacked experience in manufacturing jobs. It took years to teach them a skill, and often by the time they learned it, they were already subject to release. Thus quarrying or masonry were the most popular occupations for convicts before the Civil War. The second reason was the small number of prisoners: prison maintenance costs were low, and states did not have strong incentives to employ them.

This situation changed after the Civil War. The prison populations soared.[13] In Ohio, New Jersey, and the Eastern Penitentiary of Pennsylvania, they tripled

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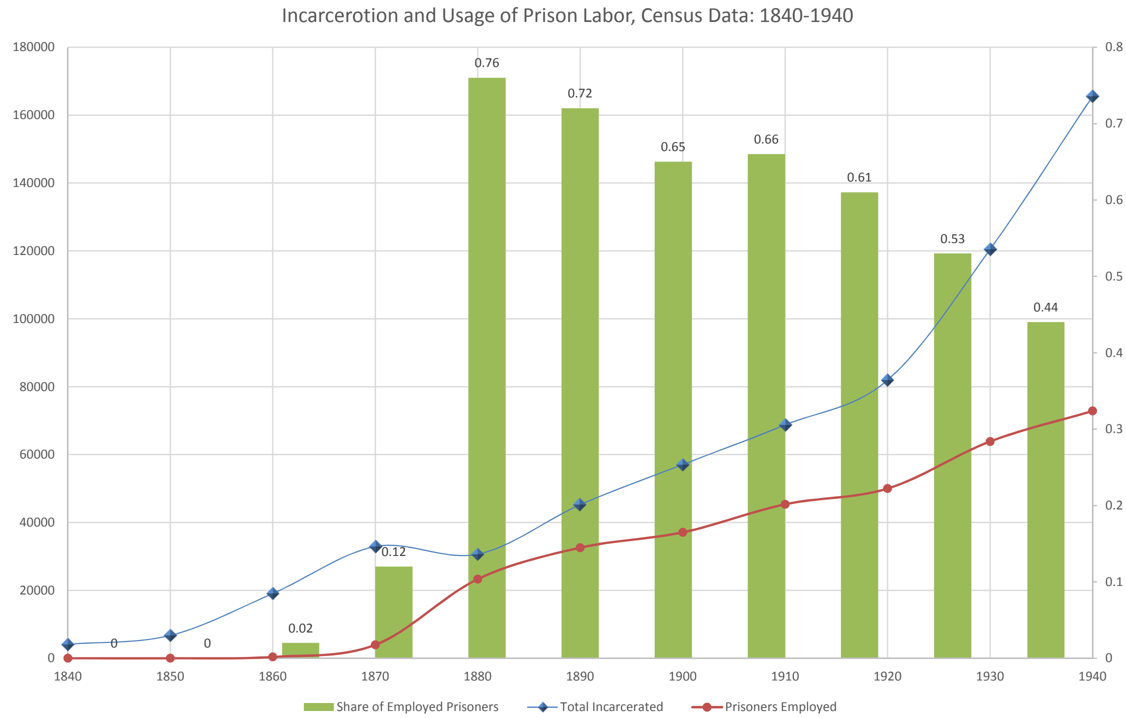
and were given an opportunity to work while inside (Lewis (1922) pp. 68-70).

[12] Auburn prison was profitable most of the time. It was followed by Sing Sing Correctional Facility in 1828. The profitability of the Auburn prisons (and in fact all prisons) depended highly on the warden. One of Auburn's directors tried to make a full cycle of silk production there but failed in growing silkworms, creating huge losses for the state. More in Barnes (1918) p. 260, Gildemeister (1978) p. 17, and Hiller (1915) p. 248.

[13] More in Gildemeister (1978) pp. 19-40.

Figure 1.1: Incarceration and Convict Labor



from 1856 to 1886, compared to population growth of 75.3%.[14] More prisons were needed.[15] In the wake of the Civil War, states had other budgetary problems that made them more eager to find ways for their penitentiaries to fund themselves (Wilson (1933)).

New types of industrialized machinery were replacing many of the manual skills needed to produce particular goods, making some industries increasingly vulnerable to cheap labor. And while unionization could protect such industries as coopers, hatters, molders, and shoemakers at the beginning, it couldn't help against the introduction of prison labor. In particular, mechanization enabled prisons to teach convicts one particular task instead of the whole set of skills

[14]Prison data is from the prisons' annual reports; population growth is based on changes between 1860 and 1890 from the decennial population census.

[15]Moreover, due to slavery and specific "honor" cultural norms (Grosjean (2014)), ex-Confederate states only had three prisons throughout their territory. Although, the one in Atlanta, Georgia was destroyed during the city siege.

necessary to manufacture certain goods.[16]

1.3.1 Economics of Convict Labor

The use of convict labor was clearly controversial. Most firms that were using free labor complained that they suffered unfair competition because of prison-made goods (Department of Labor (1887, 1925)). This section contains factual records and examples of this competition and explains why introduction of convict labor was a price shock.

One may think of convict labor as a labor supply shock. However, this would be only partially true. Only convict leasing system allowed firms to freely employ prisoners, and the predominantly Southern convict leasing system only employed 20% of prisoners at its prime in 1886 (9,104 inmates), and only 3% in 1914 (1,431 inmates).[17]

The majority of prisoners were employed within the walls of their prisons, and regular firms could not hire convicts directly. The exception to some extent was the contract system, as it allowed one (or few in rare cases) firms to employ prisoners within prisons premises. However, those firms were often connected to

[16] A colorful case of the hatters industry's struggle with prison labor is described in Weiss and Weiss (1961) (pp. 9, 28, 56). Created in colonial times, the industry was highly concentrated in Connecticut, New Jersey, New York, and Pennsylvania. In response to mechanization begun in the 1840-1850s, hatters created a union in 1854 to protect the vitality of their trade. However, in the 1870s prisons in those locations began to produce and sell cheaper hats: in Connecticut, prison-made hats sold at "$1.00 to $2.00 per dozen less than goods of similar qualities would cost regular manufacturers." The competition essentially halved the population of hatters (CT Contract Convict Labor Commission (1880), pp. 103-4, 115.). In 1878, hatters in New Jersey lobbied successfully for abolishment of hat production in the New Jersey Penitentiary. However, this provided only marginal relief from convict production in New York, Massachusetts, and Rhode Island. By 1882, the daily wage of hatters in New Jersey was $1.84 — four times the cost of prison labor — making the competition troublesome (NJ BSLI (1883)). By 1884, hatters had effectively curtailed or abolished prison production of hats in all states but Massachusetts and were able to moderately raise wages and decrease unemployment by 70% (NJ BSLI (1888)). A similar case of iron molders' competition with prison labor in the 1870s is described in Gildemeister (1978), pp. 175-180.

[17] More about the systems of convict labor in the next Section. Details of the Southern convict labor system are described in Poyker (2018a).

the prison warden either through collusion or (in most cases) they were affiliated to his relatives (Gildemeister (1978); McKelvey (1934)). Thus, prison was producing goods on the open market by itself, or through one affiliated to the prison firm (as in contract system).

Convict labor camps started to employ prisoners in low-skilled intensive industries and sell final low-quality goods on the open market. Prices of prison-made goods were very low and local firms had to wait until prisons sold everything before they could start to sell the same product themselves: "Our minimum price of bungalow aprons is about one-third higher than the prison-made goods. We can compete with them only because they do not produce enough to supply the market and then only by selling as close as possible to their price on a small margin of profit." Losing money, they had to try to decrease the wages: "I cut the wages of the girls. ... Under ordinary circumstances our girls make from $18 to $20 a week. ... If we keep the cost down to a figure that will make it possible to make goods, a girl can not make more than $2 a day."[18]

Lower prices were possible mainly because prison labor was cheaper than free labor. Prisons either paid too little or nothing at all to the inmates.[19] As a result, some employers noted, "... [the wage of prison labor] is one-sixth of the wage rate paid by those employing free labor."[20] In most cases, such unfair competition meant that firms using free labor had to "let them sell their products before we can begin," a twine manufacturer from Minnesota noted.[21]

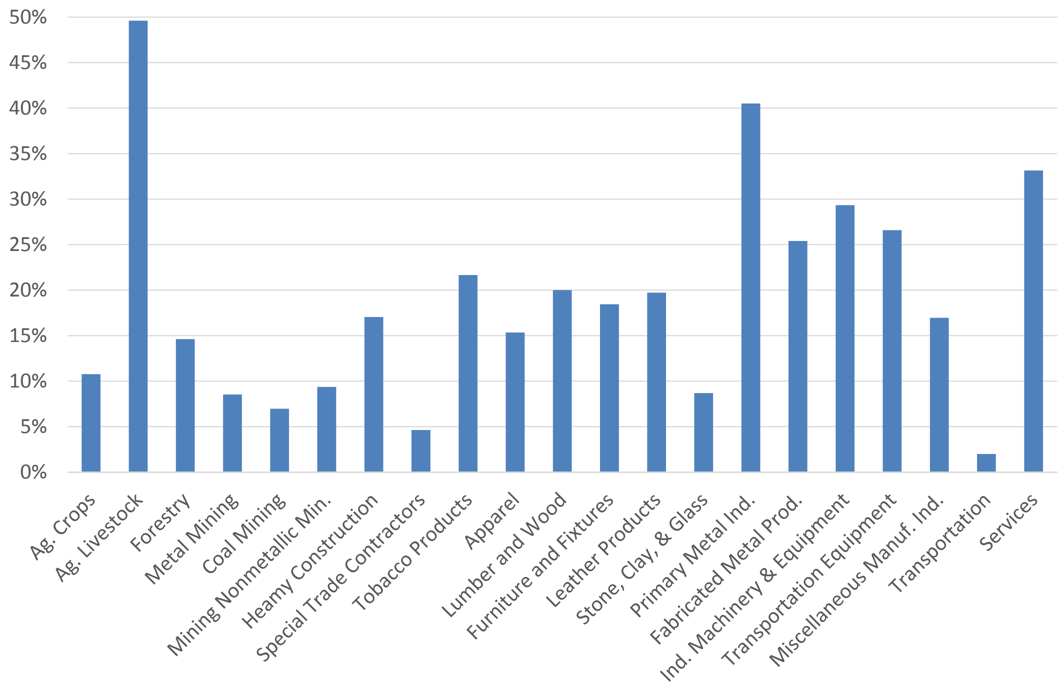
The evidence above indicates that the price shock created by prison-made

[18]Department of Labor (1925), pp.111-112. In some states, prison output constituted up to 80 percent of all output in its industry. In the most popular among prisons apparel industry, for example, prisons produced from 3 to 35 percent of state's apparel-firms output.

[19]In some states prisoners were eligible for earlier release as a result of working records. In other states prisons were obligated to pay lump-sum payments equal to the accumulated wages of the inmates. However, in practice, prisoners were underpaid or received nothing at all (Department of Labor (1887, 1906, 1925)).

[20]Even in the countryside, labor costs were too high: for example, a manufacturer based in the rural area in Ohio said: "Even in a country factory such as this, it is impossible to compete with prison products. ... I have tried having the goods made up in the country, but you cannot

Figure 1.2: Ratio of Cost of Convicts to Cost of Free Laborers, 1886



goods increased competition in final goods markets for certain industries and adversely affected labor demand in those industries. Displaced free laborers flew to other industries, pushing down the average wage on the local labor market. Thus the introduction of convict labor was an adverse-labor demand shock to local free workers.[22]

To show that the convict labor shock was an industry specific, I discuss the case of coopers in Chicago who faced competition from Joliet State Prison in Illinois

get the cost down enough to meet prison competition."

[21]And this, from a shirt maker in the Midwest: "Our work-shirt department has been shut down for two months. For over three months ... we operated at a loss. ... Until the prison contractors have sold out we cannot sell." A small Indiana apron maker suggested that "if someone business was large enough, it might be possible to fight prison competition." However, even big companies could not compete with it: for example, a large plant in the same city in Indiana producing $3 million of merchandise annually had to close.

[22]I also assume that incarceration rates did not have an effect on local labor supply. As convicts left their county of residence and send to one of the prisons in their states, they could decrease labor supply in counties without prisons relative to county with prison, and make me overestimate the effect of convict labor. However, the average number of people taken from the labor force was small. The earliest available data is from 1920 census: on average, 62 people were incarcerated in each U.S. county (st.dev. = 229).

and Indiana's Michigan City Prison, which started to produce barrels for meat in 1885. Cooperage was an important industry, producing containers in which to ship consumer goods. Before industrialization, coopers commanded a fairly high level of skill, as they used a small array of hand tools to fashion barrels. Coopers were producing two types of barrels: "slack" and "tight." The former was used for dry products like meat and vegetables, nails and machine parts, while the later was used for liquids or heavy solids (e.g., flour, rum, and naval stores) and required the highest mastery of the craft.

It was not until the 1870s that technology and steam-powered machinery revolutionized the craft in response to major new demands in meat-packing and oil.[23] Meat-packing demands shifted toward lower quality and higher quantity with new processes and rapid market expansion. At the time Joliet State Prison and Michigan City Prison starting to produce "pork barrels," "lard tierces," "lard kegs," and "beef tierces" (IL BLS (1886)).[24] Since prisoners could be contracted at less than one third the price of Chicago coopers, the operation (even conditional on lower productivity) became profitable. The share of prison-made meat packages sold in Chicago increased by 56% from 43% to 67% of the market, while their average price decreased by 33% (Figure ??). Of the 65 Chicago shops employing 686 coopers operating in 1880, 16 shops (235 coopers) had closed by 1885. From 1875 to 1885, average annual wages for coopers dropped by 30%, from \$613 to \$432. At the same time, the salaries of coopers employed in the production of tight (beer) barrels (not competing with prison labor) decreased only by 8.6% (Panel A of Figure 1.3).

At the same time, due to transportation costs the adverse effect on wages of coopers was smaller farther away from the prisons that produced those barrels.

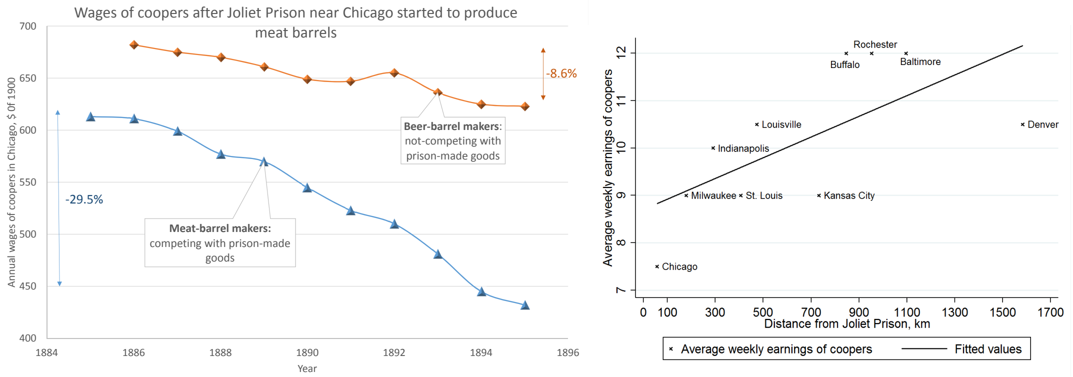
[23] More about industrialization in the cooperage industry can be found in Coyne (1940), pp. 23-24; Hankerson et al. (1947), pp. 147-151; and Wagner (1910), pp. 306-325.

[24] These goods produced by so-called provision coopers required less skill than the traditional mainstays of the cooper craft, tight and slack barrels. In fact, the new products were called packages, not barrels.

The Illinois and Indiana prisons depressed wages as far as Milwaukee to the north and Kansas City to the west. In Panel B of Figure 1.3 I plot wages of coopers in cities where prison-produced barrels were found by the investigators of the Industrial Commission on Prison Labor (Department of Labor (1900)) and distance from those cities to the Joliet State Prison.

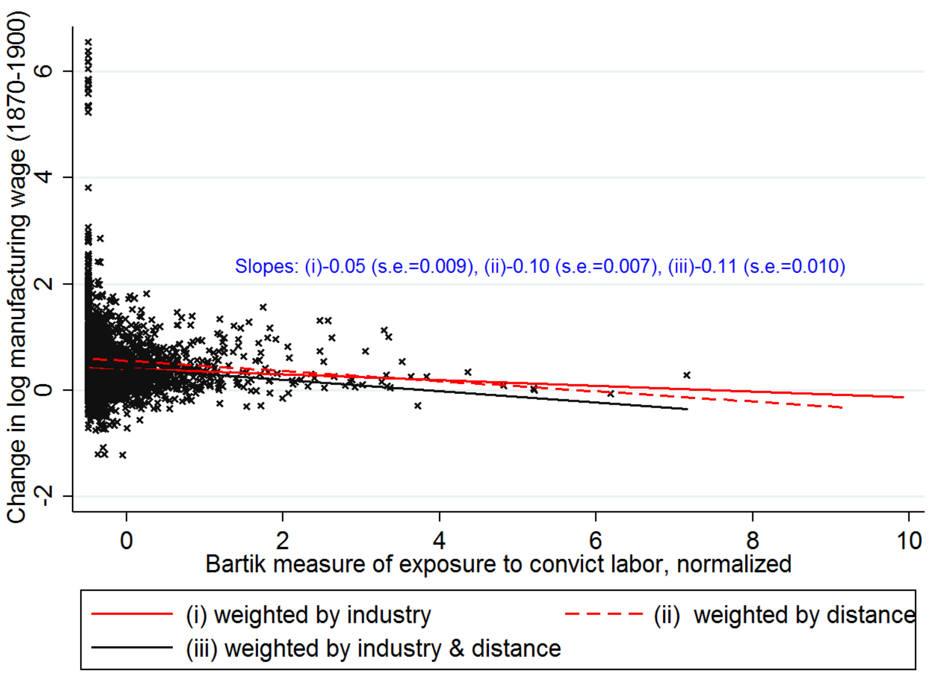
Figure 1.3: The Case of Chicago's Coopers: Wages of Coopers Producing Meat and Beer Barrels

Panel A　　　　Panel B



Annual wages in 1895 dollars. Source: Department of Labor (1900), pp. 38-39.

Figure 1.4: Convict Labor and Changes in Manufacturing Wages



Each cross is a county. Source: U.S. Department of Labor, and Haines (2004).

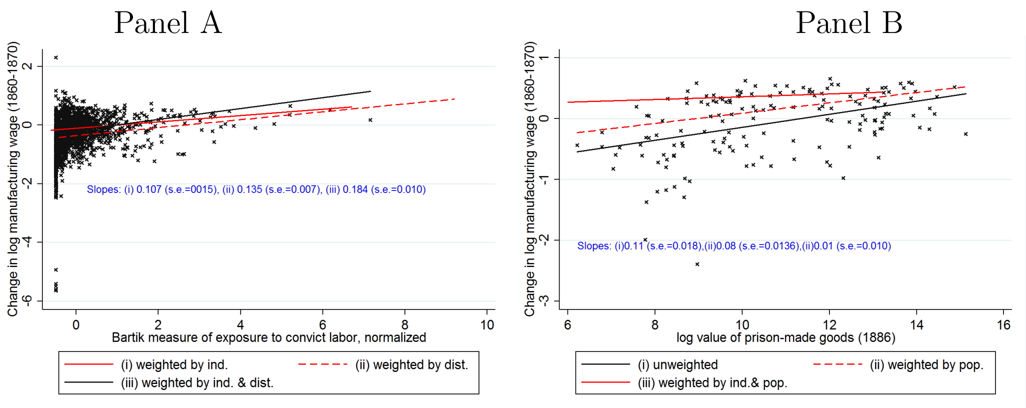
The last point raises the question whether convict labor was a pure industry shock similar to the setting of Autor et al. (2013), or whether it was more local because transportation costs were larger at that time. In Figure 1.4 I plot changes in log manufacturing wages between 1870 and 1900 on the vertical axis, and the Bartik measure on the horizontal axis. The solid red line represents the measure of the convict labor shock as the total value of prison-made goods in each industry weighted by county's industry composition (pure Bartik measure).[25] The slope is indeed negative. However, as we saw in the previous figure, both distance and industry matter. Thus I assume iceberg transportation costs, and weight the value of goods produced in each U.S. prison in each industry by distance to that prison. The dashed red line shows the slope for the Bartik measure where I weight by distance to prisons instead of industry compositions. The slope of the

[25] I assume, that the change was from zero-level in 1870 to the level of 1886, when the first data is available.

line is steeper than for the measure with industry weights. Thus the convict labor shock was local, and counties located closer to prisons were more affected than those located farther away.[26] Finally, I construct Bartik measure by weighting both by industry and distance to prisons and plot the linear fit with the black solid line.[27] Its gradient is even more negative, suggesting that both industry and distance were important for the convict labor shock. With respect to different outcomes, in Figure A1.12 I show negative relationship between convict labor and employment share in manufacturing (Panel A) and labor-force participation (Panel B).

Finally, I provide a placebo test, and show the relationship between convict labor output and changes in wages a decade before convict labor was adopted. In Panel A of Figure ?? I plot exposure of each U.S. county to convict labor weighted by distance to prisons in 1886 and changes in log wages in 1860-1870. If some correlation is present, it is positive, such that counties with larger convict labor output experienced faster wage growth.[28]

Figure 1.5: Convict Labor and Changes in Manufacturing Wages: Placebo



Each cross is a county. Source: U.S. Department of Labor, and Haines (2004).

[26] This is consistent with Donaldson and Hornbeck (2016) and Rhode and Strumpf (2003) who argue that transportation costs were large at that time. All results hold if I exclude outliers.

[27] This measure is the baseline measure in my analysis and will be described in grated details in Section 1.5.

[28] I present placebo results for employment share in manufacturing and labor-force participation in Figure A1.13.

1.3.2 Types of Convict Labor Systems in the United States

U.S. convict labor systems have seen many changes over the past 150 years. The development of the penitentiary was integrally related to rapid industrialization, and convict labor become widespread only after the Civil War (Wilson (1933)). At first, hard labor was seen as more humane and efficient than physical punishment (a belief influenced by Quakers and Protestants), but over time convict labor also became a major source of income for state governments.

After the Civil War, states started to enact convict labor laws that allowed prisoners to be employed in productive labor. Legislation varied a lot regarding both profitability for the state and other parties involved, and the working conditions of prisoners. There were six systems of convict labor: "contract," "piece-price," "state-account," "state-use," "public works and ways," and "convict leasing".

The "private" systems:

- Under the contract system, prison officers, under legal instruction, advertised for bids for the employment of the convicts of their respective institutions, the highest responsible bidder secure the contract. The contractor employed a certain number of convicts at a certain price per day. The prison or the state furnished power, and sometimes machinery, but rarely tools. All convicts were employed within the walls of the prison.

- The piece-price system was similar to the contract system, except that the contractor had nothing to do with the convicts. The contractor furnished the prison officers with material ready for manufacturing, and the prison officers agreed to return the completed work, for which the government received an agreed price per piece. Under this system the contractor had no position at the prison.

- Under the convict leasing system prisons and local sheriffs had the right to "lease" convicts to private individuals, firms, or farms and plantations. The lessee paid to the prison and various public officials involved and was responsible for feeding, clothing, and housing the prisoners (Sellin (1976)).[29]

The "public" systems:

- Under the state-account system, the prison acted as a firm and sold goods on the market, thus assuming all business risks. All profit went directly to the states. However, this system had two major problems. The first problem was managerial: wardens were often bad businessmen. Second, prisons needed to employ convicts even if there was no demand for the goods produced.

- The state-use system is similar to the state-account system, except that the sale of goods was limited exclusively to state departments and agencies.

- The public works and ways (PWW) system: as is evident from the name, prisoners constructed and repaired roads rather than producing goods for consumption.[30]

Contract, piece-price, and convict leasing systems were sufficiently similar, and I will refer to them as private systems. They assumed private operation of convict labor and were producing goods sold on the open market (often interstate), thus competing with free labor.[31] This proved disruptive for two reasons. First, convict labor was significantly cheaper than the free labor. Second, firms that took advantage of the contract system were criticized for undercutting their prices

[29] Convict leasing was the most profitable system of convict labor (Department of Labor (1887, 1914)) for the states.

[30] The PWW system shares similarities with the convict leasing system, namely that prisoners did not occupy prisons' capacities. However, as they were working on public projects, states were not entirely free from housing and guarding duties.

[31] The labor cost of prison-made goods was fixed for convict leasing and contract systems since contractor/lessee paid to the prison a lump sum payment only. Under piece-price system labor costs were variable.

below the market price of their goods.[32]

The state-account system produced goods for open sale and thus competed with goods produced by free labor. The simplest way to describe it is to say that the prison was a firm, and its warden a manager. Prison operated under this system were less efficient than the private systems: they often had to produce goods for which the state provided machinery but not the one that were most profitable. Moreover, wardens were often bad entrepreneurs (Gildemeister (1978); Hiller (1915)). At the same time, the state-use and PWW systems intended to produce goods (construction services in case of the PWW) that would be consumed by its state agencies. The Department of Labor (1914) considered them less dangerous for free labor regarding competition. As convict labor under these types was entirely under the prison's supervision, I refer to them as public systems.

The division to private and public system is important, because I later use the time variation in state-level laws that allowed private system vs. laws that did not allow convict labor or only allowed public systems (see Section 1.5). Here, I make an assumption that private systems were more harmful than the public. I also assume that state-level enactment of first private convict labor laws and following changes in convict labor legislation were unexpected by wardens of counties' prisons.

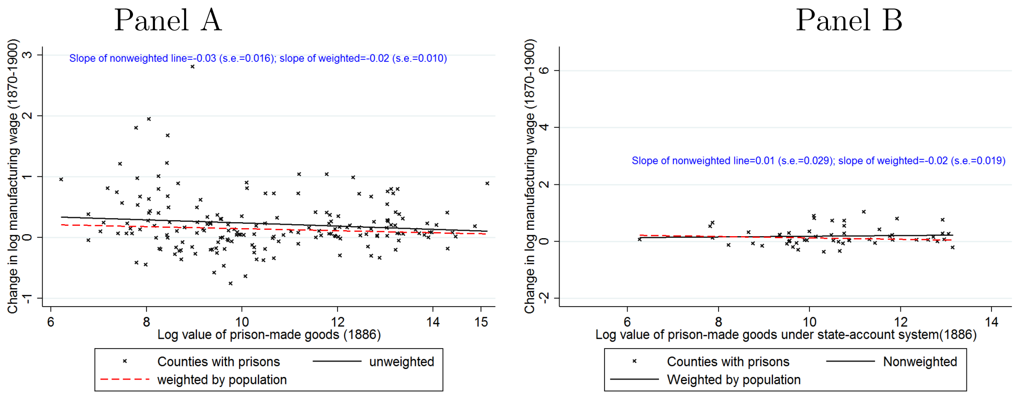
In Panel A of Figure 1.6, I plot changes in log manufacturing wages between 1870 and 1900 on the horizontal axis, and log value of prison-made goods in 1886 on the vertical axis. We can see a negative slope (approximately -0.03 (s.e.=0.016)). It suggests that counties in which prisons produced more prison-made goods experienced lower wage growth. In Panel B, I use the value of goods produced only under the public systems on the horizontal axis instead. The slope

[32] All states used convict labor, and all but five states (Colorado, Idaho, Montana, North Dakota, and Utah) involved private contractors.

is statistically not different from zero. It suggests that public systems were less harmful (if harmful at all) for free labor. I also present a placebo test in Panel B of Figure ?? where I use convict labor output in counties that had prisons in 1886 and changes in log wages in 1860-1870. The relationship is positive.

As can be seen in Table 1.1, private system experienced a tremendous growth since 1870 both in shares and the number of employed prisoners.[33] Private forms of convict labor were initially more popular than public ones.[34] But they were gradually replaced with allegedly less harmful public systems due to increasing social pressure (Department of Labor (1914); Sharkey and Patterson (1933)).[35]

Figure 1.6: Convict Labor and Changes in Manufacturing Wages



Each cross is a county. Source: U.S. Department of Labor, and Haines (2004).

[33]Most of convicts were employed in New York under the contract system, and few were leased out in the Southern states.

[34]Similar trends can be observed for the value of goods produced under each system of convict labor in Table 1.15.

[35]According to Department of Labor (1925) convict leasing, which existed mostly in the Southern states, has dissipated by 1923. However, convicts were redirected to work under PWW system (by constructing highways and railroads) or to harvest cotton on penal state farms under the state-use system (Shichor (1995)).

Table 1.1: Evolution of Convict Labor: Share of Employed Convicts

System	1870	1886	1895	1905	1914	1923	1932	1940
Convict leasing	1	20	14	6	3	0	0	0
Contract	6	30	24	23	16	7	3	0
Piece-price	0	6	10	5	4	4	6	0
State-account				14	20	16	10	5
State-Use	5	20	24	12	14	22	22	26
PWW				5	7	12	12	13
Not employed	87	24	28	35	36	39	47	56
	100	100	100	100	100	100	100	100

State-account, state-use, and public works and ways systems were reported together as the public-account system before 1905. Shares for 1870 are the upper bounds, as there are no data on how many inmates actually worked, only the total prison population of the prisons that employed prisoners. Source: U.S. Department of Labor data.

The struggle against convict labor continued despite the shift from private to public forms. No matter which system was used, prison labor competed with free labor to some extent. And since approximately 60% (Sharkey and Patterson (1933)) of all prison-made goods were sold in states other than their state of origin firms using free labor opposed convict labor anywhere in the country. However, states could only pass legislation regulating production of convict-made goods in their state; they could not regulate interstate trade. Congress has attempted to enact laws prohibiting the use of prison labor since the beginning of the 1900s; however, the first anti-penal labor law (Hawes-Cooper Convict Labor Act) wasn't signed until 1929 and enacted until 1934. That act allowed states to prohibit sales of convict-made goods produced in other states. Two years later, in 1936, two more federal laws (the Ashurst-Sumners and Walsh-Healey Public Contracts Acts, 1936) were enact to prohibit any interstate trade with prison-made goods and have any contracts with private contractors.[36] As a result, by 1940, all convict labor was concentrated in the public systems, either producing goods for consumption by its state or employing prisoners in chain gangs. The latter was abolished in 1941 by President Roosevelt's Circular 3591. State-use system of convict labor remained

[36] Although it allowed to sign contracts not exceeding $10,000 annually.

the only form of convict labor afterward, and the problem of competition with convict labor was quieted until 1979, when Congress revived the private system of convict labor by establishing the Prison Industry Enhancement Certification Program.

1.4 Data

1.4.1 Novel Data on U.S. Convict Labor

In this Section, I describe the new dataset of convict labor in the United States collected for this paper. The rest of the data that appears in this paper was used previously by other researchers; it is described in great detail in Appendix 1.8. The construction of the variables used in the empirical specifications will be introduced in the corresponding sections containing the results.

The primary source of the data for this paper is a set of U.S. Department of Labor reports devoted to convict labor. As competition between convict labor and free labor was a widely discussed topic at the time, the Bureau of Labor decided to inspect all penitentiary facilities to determine the level of competition between goods produced under different convict labor systems and goods produced by free laborers. Approximately every ten years, the Department of Labor was issuing special reports devoted to convict labor, containing meticulously collected information about employed prisoners, and output of U.S. correctional facilities.

I collected and digitized seven reports for the following years: 1886, 1895, 1905, 1914, 1923, 1932, and 1940. Then, I matched all prisons and convict labor camps across years by name and assigned a FIPS code and GPS coordinates for each one of them. Overall, the dataset contains 464 different locations with correctional facilities.[37]

[37]GPS coordinates of a prison are difficult to determine since most of them do not exist now; instead I use coordinates of the location of a town where it was located. In the few cases where

The data were collected by Bureau of Labor employees who traveled directly to prisons and filled out the surveys according to the accounting books provided by prisons.[38] The data includes all prisons and convict labor camps, as well as juvenile reformatories and industrial schools. It includes prisons that host prisoners but do not employ them. However, the data does not include local county jails, unless they employed prisoners.

The data does not contain industry codes but does include specific articles of produced goods (e.g., "Cane-seating Chairs" and "Clothing, Men, and Boys"). I assign them to the two-digit SIC codes from the 1987 SIC manual. For further analysis, I aggregate the value of goods produced by convicts (Value of goods $\text{produced}_{i,p,t}$) in prison (p), industry (i), and year level (t).

Finally, I construct a dataset of prisons existing before convict labor was imposed in each state with their GPS coordinates.[39] Most of the data come from the North American Review (1866) and Wines (1871).[40] As total institution population of prisons was often above design capacity, I assign the maximum of the prison capacity or actual prison population from the 1870 or 1880 U.S. population censi[41].

1.4.2 Selection into Having a Prison and Summary Statistics

Prison location is endogenous to local economic conditions, even disregarding the dimension of convict labor. To understand the sorts of selection bias that might plague an evaluation of the effect of convict labor, one must consider how geo-

several prisons are located in the same town, I aggregate it to the GPS coordinates of the town.

[38] The data for the 1895 report was obtained not in person but through the mail: prison wardens filled out the survey themselves and sent it to the Bureau of Labor.

[39] All early legislation related to convict labor is available in Department of Labor (1887).

[40] I supplement that data with state-level (or prison-level) official reports related to correctional facilities.

[41] Table XIX of Volume I, of the 1870 census, and Tables CLII-CLV of the Compendium of the 1880 census.

graphic placement of prisons was determined. Historians (Lewis (1922); McKelvey (1936)) and contemporaneous sources (North American Review (1866)) list several criteria that were used to determine prison location from 1850 to the 1930s: (i) proximity to large urban centers; (ii) proximity to a railroad or navigable river; (iii) proximity of materials suitable for the construction of a prison.[42] The high cost to transport prisoners and materials clearly influenced the location of prisons.[43] Based on these criteria, it is reasonable to expect counties with prisons to have a higher urban share of population, and higher wages than other parts of the country.[44]

Based on 1870 levels, counties with prisons appear to be more populous and more urban, as expected; however, market access, which is a proxy for the proximity to railroad and navigable rivers is similar to those in the rest of the sample.[45] Treated counties are more educated, have more churches, have higher manufacturing output, are wealthier, generate more taxes, and also have higher debt. The sample of all prisons is more similar to the rest of the sample than to the sample of pre-convict-labor-era prisons. If I exclude former Confederate states, groups become more evenly balanced.

Overall, treated counties had much higher wages in manufacturing, suggesting that I can have strong upward bias in my point-estimates of wages in the OLS

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[42]For example, Sing Sing is located atop of a massive stone deposit. In most cases, prisons were built with convict labor (Lewis (1922), p. 113).

[43]More on this topic in Gildemeister (1978) p. 22.

[44]An example of such thinking by state legislators can be found in Wisconsin, Legislature (1850) (p. 132), where the location of Wisconsin's first prison is discussed. After some discussion they chose to build it in the north-central woods, to use local timber, and because nearby rail access to the Great Lakes would help minimize the cost to transport convicts. Similar discussion took place in 1857 in Illinois, when the location for the Joliet Penitentiary was chosen (Illinois State Penitentiary (1857), p.450).

[45]In Table A1.16, I compare the average mean county characteristics in 1870 (i.e., before the start of the convict labor era) for counties with pre-convict-labor-era prisons (Column I), for counties that had a prison between 1850 and 1950 (Column II), all counties without pre-convict-labor-era prisons (Column III), all counties without any prisons (Column IV), and counties without any prison in the sample without ex-Confederate states (Column V).

specification.

As counties with prisons differed markedly from other counties, I choose to trim the sample by omitting "worse possible" control counties (e.g., as in Kline and Moretti (2014)). To do so, I employ propensity score matching on covariates for the pre-treatment year of 1870. As there could be other important unobservables not mentioned by the historians, it could render the propensity score estimation incorrect if I do not include them. Thus I follow an idea mentioned in Chernozhukov et al. (2016) and use all possible covariates from the cross-section of county-level 1870 data and run LASSO. Then I use the most important covariates to estimation the propensity score. Finally, I drop all counties whose propensity score is below an arbitrary threshold (25%).

The trimmed sample creates a better control group (Column VI), especially when ex-Confederate states are dropped (Column VII), while counties in the resulting control group still have smaller wages.

Overall, Table A1.16 confirms that counties with prisons were more urban and populous, and had higher wages relative to the rest of the nation's counties and, to a lesser extent, the Northeast and Midwest regions.

1.5 The Effect of Convict Labor on Wages and Firms: Introduction of Convict Labor in 1870-1886

1.5.1 Convict Labor and Local Labor Outcomes

1.5.1.1 Empirical Specification and Results

As evident from the Table 1.1 identifying variation come from the massive unexpected expansion of private forms of convict labor from 1870 to 1886. While few new states continued to switch toward private system after 1886, there was a gradual decreasing trend in the prevalence of private forms convict labor. In this Section I exploit only the introduction of convict labor in 1870-1886 as the national exogenous shock caused by competition with prison-made goods.

I use a first differences specification:[46]

$$\Delta y_{c,1880/1900} = \alpha + \gamma \Delta CL_{c,1886} + \Pi \Delta \mathbb{X}_{c,1880/1900} + \eta y_{c,1880} + \varepsilon_c, \tag{1.1}$$

where $\Delta CL_{c,1886}$ is a change of exposure to convict labor from the zero-level of 1870 to the level of 1886, $\Delta \mathbb{X}_{c,1880/1900}$ is a matrix of changes in control variables, and $y_{c,1880}$ is a pre-treatment level of the dependent variable.[47] I only use a Bartik-style continuous measure of exposure to convict labor. As convict labor was subject to state-level legislation I cluster standard errors by state.[48]

As convict labor was virtually non-existing at 1870, the change ($\Delta CL_{c,1886}$) is actually a level ($CL_{c,1886}$) of exposure to convict labor in 1886. I define two measures of exposure to convict labor to which I later refer to as "continuous" ($CL_{c,t}^{\text{continuous}}$) and "discrete" ($CL_{c,t}^{\text{discrete}}$). In the former, I allow all counties to be treated by convict labor. However, as I measure effects on the local labor markets, I weigh the effects of each prison by the distance between it and a given county and by counties industrial composition:

$$CL_{c,1886}^{\text{continuous}} = \sum_{i \in I} \left(\lambda_{i,c} \times \sum_{p \in P_{1886}} \frac{\ln\left(\text{Value of goods produced}_{i,p,1886}\right)}{\text{Distance}_{c,p}} \right) \tag{1.2}$$

[46]This specification is similar to the one in Autor et al. (2013).

[47]Following Wooldridge (2015), I add the constant as a difference of the intercepts between decades; however, results are robust to specification without the constant.

[48]While the number of clusters is below the "rule of 42" (Angrist and Pischke (2008)), Hansen (2007) finds that Stata's cluster command is reasonably good at correcting for serial correlation in panels, even in 10 clusters scenario. Although OLS specifications remains significant if I use wild bootstrapping (Cameron et al. (2008)).

where P_t is the set of all prisons at year $t = 1886$, $\text{Distance}_{c,p}$ is a distance between prison p and county's c centroid (in km), and $\lambda_{i,c}$ is a value share of industry i in county c in 1870.[49] In the discrete measure, only counties that had prisons are considered as treated:

$$CL_{c,1886}^{\text{discrete}} = \sum_{i \in I} \left(\lambda_{i,c} \times \ln \left(\sum_{p \in P_{c,1886}} \left(\text{Value of goods produces}_{i,p,1886} \right) \right) \right) \quad (1.3)$$

where $P_{c,t}$ is the set of all prisons in county c at at year $t = 1886$, and $\lambda_{i,c}$ is the same. As transportation of goods was costly, and historical literature emphasized that the competition mattered on the local labor markets I use these two measures as the baseline measures.[50]

My main identifying assumption is that there are no other variables that are correlated with exposure to convict labor and have effects on manufacturing outcomes. This first-differences model cannot account for unobserved factors that vary by county and over time and are correlated with the prevalence of convict labor. For example, if a prison site was chosen in a place with cheap land with a decreasing wage trend, I will overestimate the effect of convict labor. Conversely, if prisons started to produce more goods in a location where wages tended to increase, I will underestimate the effect of convict labor. As discussed in Section 1.4.2, convict labor mostly thrived in locations where wages were high, and wardens chose to produce those goods whose price was increasing, and/or if that

[49]I discuss alternative iceberg costs in Appendix 1.8. As a robustness check I report results with 1880 industry weighting (based on Atack and Bateman (1999) state-representative sample of manufacturing firms) in Table A1.22. If I use spatial HAC errors to correct for spatial and temporal autocorrelation in panel data (Conley (1999, 2010)) in specifications with continuous treatment, resulting standard errors are always smaller or similar to those clustered by state, and I do not report them.

[50]I show that results are robust to the specification with weighting only by distance (without industry weighting) in Robustness Section. For completeness, in Appendix I also show that the specification with only industry weights (à la Bartik) works too.

industry's unions were becoming stronger. This also would cause downward bias. Similarly, as prisons were more likely to be located in urban counties with higher population growth, wages tended to rise, and consequently I underestimate the effect of convict labor. I add controls for the changes in shares of African-American population to control for possible institutional omitted variables, correlated with convict labor and wages, and I add changes in shares of women and foreign-born population as proxies for the changes in crime rate. An important source of the omitted variable can come from counties' economies, thus I add changes in values of manufacturing and farm output. I control for changes in total population, urban share, and population growth to address the fact that most prisons were located in urban settings.

Measurement error could be a potentially crucial source of bias in my analysis. There are two possible sources of this bias. First, wardens often did not write down in their books all the output that the prisoners produced, or through collusion with the contractor artificially decreased the value of goods produced (Gildemeister (1978); McKelvey (1934, 1936)). The second possible source is the cost of convict labor: in many cases prisons were employing all their prisoners, while on paper some of them were ill or handicapped or working half a day. In addition, noone controlled the working hours of prisoners; thus, potentially, inmates could make more goods than were reported.[51] Thus in addition to classical measurement error I may have upward bias due to under-reporting. However, it will cause only scaling upward bias and will not affect the significance of point-estimates. Moreover, assuming, that every warden reports only a quarter of the true value of prison-made goods the evaluation of the overall effect of convict labor (e.g. comparison of counties in 25th and 75th percentile of exposure to convict labor) won't be affected.[52]

[51] Gildemeister (1978), pp. 34-37 and fn. 21,22.

[52] Measurement error will be a problem if there is a differential trend in under-reporting.

The main source of endogeneity that I cannot control for is related to the fact that wardens chose to employ prisoners in industries where unions were maintaining high wages, or, in some cases, because convict labor catalyzed unionization of local affected industries (Hiller (1915); Gildemeister (1978)). This would cause a downward bias of my OLS estimates.

Overall, OLS estimates will be biased because of endogeneity in the amount of prison-made goods, and endogeneity in selection into building a prison when convict labor is already allowed. Thus concerns about omitted variable bias suggest that I will more likely underestimate the effect of convict labor on wages.

To deal with the embedded endogeneity problem, I use IV estimation. I use plausibly exogenous cross-sectional variation of the state prisons that existed before the years when convict labor was allowed in corresponding states (hereafter old prisons). Old prisons were built without consideration of manufacturing goods for profit, and their locations can be considered exogenous, conditional on population and urban share.[53] To conclude, I assume that preexisting prisons are uncorrelated with the error term, are good predictors of the usage of convict labor, and do not directly affect wages in manufacturing.[54]

The first stage of the 2SLS specification can be written as:

$$CL_{c,1886} = \alpha + \gamma \text{Old Prisons}_{c,1870} + \Pi \Delta \mathbb{X}_{c,1880/1900} + \eta y_{c,1880} + \varepsilon_c, \qquad (1.4)$$

[53] Because old prisons were concentrated in the Northern states (see Figure A1.9), the instrument rather identifies LATE of competition with prison-made goods produced in the Northern states under private systems. To estimate the overall effect of convict labor on manufacturing wages in the whole U.S. and for the whole spectrum of convict labor systems, I use the actual number of prisoners (both employed and idle) as an instrument for value of goods produced. However, identification comes with a cost of relaxing the assumption that location of new prisons cannot be strategically chosen for the sake of maximizing profits from convict labor. Results of this specification are provided in Appendix 1.8. While resulting coefficients are comparable in magnitude, and, the first-stage relationship is stronger, I choose not to use it as a baseline specification.

[54] Following Conley et al. (2012), I check the sensitivity of the identifying exclusion-restriction assumption in Appendix 1.8.

And the second stage can be written as:

$$\Delta y_{c,1880/1900} = \alpha + \gamma \widehat{CL_{c,1886}} + +\Pi\Delta\mathbb{X}_{c,1880/1900} + \eta y_{c,1880} + \varepsilon_c, \tag{1.5}$$

where the variable Old Prisons$_{c,1870}$ measures exposure of each county by the old prisons around it: Old Prisons$_{c,1870} = \sum_{p \in P} \left(\frac{\ln\left(\text{Old prison capacity}_p\right)}{\text{Distance}_{c,p}} \right)$, Old prisoncapacity$_p$ equal to actual time-invariant old prisons capacities.[55]

[55]Results are robust to the specification with a "discrete" instrument (Appendix 1.8). In this case, only counties with old prisons have nonzero values. In the previous version of this paper I used prison capacities just before the state has accepted convict labor legislation. However, in case prison can expand their capacities in expectation of new convict labor legislation (if they see nearby states adopted them early) I chose to use prison capacities at 1870. Nevertheless, results hold.

Table 1.2: Convict Labor and Labor Market Outcomes: Introduction of Convict Labor

Panel A	Introduction of Convict Labor (1870-1886)					
	I	II	III	IV	V	VI
Outcome (1880-1900):	Δ log Wage in Manufacturing		Δ Labor-Force Participation		Δ Employment Share in Manufacturing	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict Labor (Continuous)	-0.013**	-0.030***	-0.004***	-0.005*	-0.008***	-0.014**
	(0.005)	(0.012)	(0.001)	(0.003)	(0.002)	(0.006)
R-squared	0.197	0.188	0.032	0.032	0.113	0.105
Kleibergen-Paap F-stat		13.29		16.27		13.7
Observations	1,954	1,954	2,122	2,122	2,226	2,226
Panel B	**Introduction of Convict Labor (1870-1886): Placebo**					
	I	II	III	IV	V	VI
Outcome (1860-1870):	Δ log Wage in Manufacturing		Δ Labor-Force Participation		Δ Employment Share in Manufacturing	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict Labor (Continuous)	0.102***	0.194***	0.000	0.000	0.018***	0.044***
	(0.019)	(0.033)	(0.004)	(0.005)	(0.005)	(0.009)
R-squared	0.309	0.258	0.521	0.521	0.218	0.147
Kleibergen-Paap F-stat		9.57		10.46		9.82
Observations	1,709	1,709	1,929	1,929	2,034	2,034

Exposure to convict labor is normalized. All columns contain a constant. The following variables are used as controls (in changes): ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, ln of value of farm products, log of number of slaves in 1860 (level), and log of market access (the change and the base level of 1870). All columns have corresponding lagged outcome variable (level) as a control. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Result for the continuous specification are presented in Panel A of Table 1.2.[56] Columns I and II show OLS and 2SLS of regression of convict labor on changes in log manufacturing wages. Both estimates are significant, and the IV estimate is approximately twice as large than the OLS one. The difference between the county-level convict labor change in counties at the 25th and 75th percentiles was a 0.66 standard deviation. Evaluated using the Column II estimate, a county at the 25th percentile experienced an 2.0 percentage-point larger wage decline (or smaller

[56] I present first-stage residual plot in Figure A1.15: the relationship between the endogenous explanatory variable and the instrument are clearly linear. First stage and reduced form results are presented in Tables A1.17 and A1.18.

wage increase) than a county at the 75th percentile. Columns III and IV report result for changes in labor-force participation. Evaluated using the Column IV estimate, a county at the 25th percentile experienced an 0.3 percentage-point larger fall in labor-force participation than a county at the 75th percentile. Columns V and VI show effect of convict labor on employment share in manufacturing. Using estimate from Column VI, a county at the 25th percentile experienced an 0.9 percentage-point larger fall in manufacturing employment share than a county at the 75th percentile.[57]

Finally in Panel B I repeat the same specifications as in Panel A, but with pre-treatment changes of the dependent variables. In particular I count changes between 1860 and 1870, while keeping the right-hand side of the equation the same.[58]

The discrete specification yield similar results (see Table A). This specification also alleviates the concern that distances to prisons correlate with manufacturing outcomes. While all OLS and 2SLS estimates remain significant all magnitudes experience slight decrease. The most plausible explanation for this effect is that the effect of competition with prison-made goods exceeded the boundary of a county, and thus in the discrete specification, I count partially treated counties (close to counties with prisons) as control counties. Thus the differences in wages between them is smaller, and I underestimate the effect of convict labor.

[57]In Table A1.24 I show that my results are robust to usage of the measure of exposure to convict labor similar to the one in Autor et al. (2013): $\widetilde{CL}^{\text{continuous}}_{c,t} = \sum_{i\in I}\left(\frac{L_{c,i,t}}{L_{i,t}} \times \left(\sum_{p\in P_t} \frac{\ln\left(\text{Value of goods produced}_{i,p,t}\right)}{\text{Distance}_{c,p}}\right)/L_{c,t}\right)$, where convict labor shock is scaled by county's c's labor force ($L_{c,t}$), and share of county c in U.S. employment in industry i $\left(\frac{L_{c,i,t}}{L_{i,t}}\right)$ at year $t = 1870$.

[58]The only difference on the right-hand side, is that I use new pre-treatment dependent variable $y_{c,1860}$ instead of $y_{c,1860}$.

1.5.1.2 Measuring the Effect of Convict Labor

To gauge the economic magnitude of these effects I compare the estimated reduction in wages and employment with the observed changes during 1880 to 1900. Here I make and assumption, that exposure to prison-made goods affected absolute level, and not just a relative level of manufacturing employment, wages, and labor-force participation across U.S. counties. Given the magnitude of the convict labor output (for each manufacturing worker with average annual wage of $242 there were at least $18 per worker of prison-made goods) it seems plausible that competition with prison-made goods had an absolute impact on U.S. manufacturing.

My IV specification in Panel A of Table 1.2 uses normalized explanatory variable, however for the purpose of evaluating the effect of the introduction of convict labor I use estimates from non-normalized explanatory variable. During the 1870-1886 the log of value of prison-made goods grew by 0.11.[59] Applying this value to the non-normalized estimate for the specification in Column II (-0.102), I calculate, that exposure to convict labor decreased growth in manufacturing wages by 1.1 percentage-points. Wages in manufacturing were growing at that time 7.2% on average, thus in absence of convict labor, manufacturing wage growth would be 15.8% higher. Similarly I calculate what would be the labor-force participation and manufacturing employment share without exposure to convict labor. Over 1880-1900s labor-force participation grew by 1 percentage-point, however, using coefficient from Columns IV (-0.018), I find that exposure to convict labor caused differential decrease in labor-force participation by 0.2 percentage-point. Manufacturing employment share grew by 3.3 percentage-points, and using coefficient from Columns VI (-0.049) I calculate, that exposure to convict labor decreased

[59]To make growth in value of prison-made goods comparable to my measure, I compute it as $\sum_{p\in P}\sum_{i\in I}\left(\ln\left(\text{value of goods produces}_{i,p}\right)\right)/(\text{mean distance to prison}\times\#\text{industries})$. For comparison, the mean value of the explanatory variable is 0.143.

growth in manufacturing employment share by 0.6 percentage-point. Thus without convict labor labor-force participation and manufacturing employment share would be 20.3% and 16.9% larger correspondingly.[60]

1.5.1.3 Heterogeneous Effects of Convict Labor on Female Labor Market Outcomes

In the previous section, firms affected by competition with prison labor were trying to decrease wages to keep up with the prison-made goods, and the well-being of low-skilled laborers deteriorated. Surprisingly, the group that experienced the most intense competition was not Black or foreign-born men — it was unskilled women. The number of female-labor-intensive industries was limited, and prisons were heavily involved in all of them (See Figure A1.10). The following quote describes how the demand for low-skilled laborers was affected by this competition:

> "We have been forced to go into higher line. One of the worst elements in the situation is the difficulty in training girls. When we had a large output of lower grade goods we put new hands on them. They could turn out the dresses rapidly, make better money and have enough showing in quality to hold their interest until they were expert enough to do the fancier garments. Now we cannot afford to produce enough of this class of merchandise for training purposes. Instead, men are being trained to do it in prisons. They can never use this training after their discharge as this kind of work is wholly monopolized by women. A new girl put on the higher grade stuff in the factory can not make more than one garment a day and then it is not well done

[60] Another way to indicate significance of the introduction of convict labor is to compare its effects to those of China shock (Autor et al. (2013)). Comparing two counties one at 25th percentile and the other at 75th percentile of exposure to convict labor, the more exposed county suffered 43% of the size of China shock in terms of labor-force participation, 1.5 times larger shock in terms of manufacturing employment share, and 2.5 times larger in terms of mean log wages in manufacturing.

and she is under severe nervous strain. The girls become discouraged and quit and we have it all over again. We have girls crying around here all the time because they can not handle the only work we have for them. ... We have closed one plant with 40 machines, employing 50 girls, where we produced only the cheap goods. It was closed two years ago and we do not expect to operate it again. Prison labor has shot this industry to pieces."[61]

The garment and shoemaking industries were hammered the hardest by prison-made goods. The share of the value of prison-made products in these industries was around 45% percent in 1886 and reached 75% by 1940. Two reasons distinguish why those industries were overcome by convict labor: the relative simplicity of the production process and the relative weakness of women's labor rights. Male laborers and their unions fought fiercely against employment of convicts in their industries (see Section 1.3 for examples), but women could not fight back against prison labor in the same way. Thus over time prisons shifted their production toward less-protected female-labor-intensive industries. And later, as the state-use system came into vogue (again, due to anti-convict-labor campaigns), prisons could sell their goods only to state or federal agencies (e.g., the Army), which had a large demand for clothes.

Labor demand decreased in apparel and shoemaking industries decreased, but if men could move to another industry, there was often no alternative for women. A quote from a Michigan clothing manufacturer illustrates the point:

"There are lots of girls who can't do higher grade work, who never become skilled enough to get on better goods. The incompetent girls are the victims of the criminals in prisons. The unskillful girl is simply out of luck. We used to use this low-end stuff to keep busy in slack

[61] Department of Labor (1925), pp. 112-113.

times and stock up on them. Now we have to close since we can not stock up in this line and can not keep expensive street dresses in stock."[62]

In many cases the situation was exacerbated on account of minimum wage laws pertaining for women: "Under our [Illinois] minimum wage laws we must pay a beginner $9 per week. She earns about $4 the first week. Instead of the $1 we figure for labor cost, her work cost us $1.50. It takes four weeks before she earns what she is paid and she never makes up the difference because she goes onto piece rates and is paid for what she does. The prison has no labor laws and under their contracts, the amount the contractor pays is reduced in proportion if the output does not measure up to the contract terms." [63]

I expect that wages will decrease more for women than for men, because there were few industries were women could find a job, and in most of them they competed with prison-made goods. If we see a drop in labor-force participation after the introduction of convict labor, it should also be larger for women than for men.

I have to use first-differences specification instead of the panel IV pursued thus far for two reasons. First, I have wage data by gender for 1890 and 1900 from Haines (2004), and for 1940 and 1950 from the Population Census. Second, due to the plausibly exogenous nature of the timing of the shock, I can use OLS with first-differences.

To elicit a causal effect on wages and labor-force participation of men and women, I employ two plausibly exogenous shocks of convict labor output: the introduction of convict labor in the 1870s-1880s, and its demise in 1936. Thus I use the changes in convict labor output between 1870 and 1886 $\Delta CL_{c,1870/1886} =$

[62]Ibid, p. 116.

[63]Ibid, p. 110. State-level minimum wage laws related to women, children and Black started to appeared in the earlier 20th century long before first federal-level minimum wage laws (Thies (1990)). Their effects on labor-force participation, and women-men and Black-White wage gaps is an important topic, that can be studied in the future.

$CL_{c,1886}$ to identify the changes in wages and labor-force participation in 1890 and 1900. Similarly, I use the fall in convict labor output between 1932 and 1940 $(\Delta CL_{c,1932/1940})$ due to the anti-convict labor legislature in 1936 on the changes in wages and labor-force participation between 1940 and 1950, as they should bounce back after distortion is removed. While, I can report 2SLS coefficients using identification from the previous section, I choose to report OLS estimates, since they are more conservative. Hence, I estimate the same specification as in Table 1.2.

Results for the continuous and discrete specifications are presented in Table 1.3. Each column contain results of two separate regressions: one with outcome for females, and (shaded in gray) – for males. The table also contains p-values of the test if the differences between point-estimates for men and women are statistically significant.

In Panel A, I study the shock of the introduction of convict labor. As expected, the relationship between wage changes and introduction of convict labor is negative. This implies that counties facing the largest increase in competition with prison-made goods experienced slower wage growth than counties facing smaller increase in exposure to convict labor. The estimate for women in Column I of -0.119 implies that a county facing a one standard deviation larger increase in exposure to convict labor experienced a 11.9 percentage-point larger female-manufacturing-wage decline (or smaller wage increase) relative to other counties. The difference between the county-level convict labor change in counties at the 25th and 75th percentiles was a 0.66 standard deviation. Evaluated using the Column I estimate, a county at the 25th percentile experienced an 7.4 percentage-point larger wage decline (or smaller wage increase) than a county at the 75th percentile. The estimate for the manufacturing wages of men in Column I is 3.8 times smaller in magnitude than the one for women, and the difference is statistically significant at the 99% level. I found similar results for the discrete measure in Column II.

Table 1.3: Heterogeneous Effects of Convict Labor on Female and Male Labor Market Outcomes

Panel A		Introduction of Convict Labor (1870-1886)					
		I	II	III	IV	V	VI
Outcome:		Δ log Wage in Manufacturing		Δ Labor-Force Participation		Δ Employment Share in Manufacturing	
Convict Labor (Continuous)	Female	-0.119***		-0.005*		-0.007*	
		(0.028)		(0.002)		(0.004)	
	Male	-0.031***		-0.009**		-0.013***	
		(0.007)		(0.004)		(0.002)	
Convict Labor (Discrete)	Female		-0.028***		-0.002***		-0.004*
			(0.010)		(0.001)		(0.002)
	Male		-0.011***		-0.004***		-0.004***
			(0.002)		(0.001)		(0.001)
$\gamma_{Male}-\gamma_{Female}=0$, p-value		0.00	0.06	0.02	0.01	0.07	0.83
Panel B		**AS and WH Public Contracts Acts (1936)**					
		I	II	III	IV	V	VI
Outcome:		Δ log Wage in Manufacturing		Δ Labor-Force Participation		Δ Employment Share in Manufacturing	
Convict Labor (Continuous)	Female	0.088***		0.018***		-0.008	
		(0.021)		(0.004)		(0.005)	
	Male	0.044**		0.021***		-0.001	
		(0.019)		(0.005)		(0.004)	
Convict Labor (Discrete)	Female		0.010*		-0.001		-0.003*
			(0.006)		(0.001)		(0.002)
	Male		0.010*		-0.001		0.000
			(0.005)		(0.002)		(0.001)
$\gamma_{Male}-\gamma_{Female}=0$, p-value		0.5292	0.4082	0.4	0.5134	0.4542	0.2029

Both values of exposure to convict labor are normalized. Each row contains results from two differen regressions: one for female outcomes, and one for outcomes of males. Coefficients in Panel B are multiplied by -1 to show the reduction in convict labor output. All columns contain OLS in first differences. All columns contain a constant. The following variables are used as controls (in changes): ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, ln of value of farm products, log of number of slaves in 1860 (level), and log of market access (the change and the base level of 1870). Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In Panel B, I show the growth of wages and labor-force shares after the enactment of anti-convict labor legislation in 1936. Estimates for the wage effect are comparable in magnitude; however, the effect of the anti-convict labor legislation is quite large. The difference between the county-level convict labor change in counties at the 25th and 75th percentiles was 0.70 standard deviations. Thus a county at the 75th percentile experienced a 6.2 (3.1) percentage-point larger female (male) wage increase (or smaller wage decrease) than a county at the 25th percentile. I also find that a positive effect for the discrete measure in Column II. However, male-female estimates in both columns do not differ statistically from each other.

In Columns III-IV of Panel A, I find an adverse effect on labor-force participation of both men and women. One standard deviation in convict labor output decreased labor-force participation by 0.5 percentage-point for women, and 0.9 percentage-point for men, and the difference of the effect between genders is statistically significant. These effects are small: a county at the 5th percentile experienced a 0.3 (0.6) percentage-point larger female (male) labor-force participation decline (or smaller labor-force participation increase) than a county at the 75th percentile. Male-female differences between the continuous and discrete measures are statistically significant, however it suggest that labor-force participation for decreased more than women. This may partially explain the fact that wages of males experienced slower growth than wages of women: labor supply of men adjusted and pushed wages upward.

In comparison with the introduction of convict labor, the labor-force participation results for the period of its abolishment in Column III of Panel B are much larger. Evaluated using the Column I estimate, a county at the 75th percentile experienced an 1.3 (1.5) percentage-point larger female (male) labor-force participation growth in (or smaller labor-force participation decrease) than a county at the 25th percentile and the difference of the effect between genders is statistically insignificant. However, estimates are only significant for the continuous measure. As transportation costs decreased substantively by 1930-1940s, the discrete measure is less informative, as convict labor shock became rather nation-wide, than local competition.

In Columns V and VI of Panel A I check if introduction of convict labor also affected employment share in manufacturing. One standard deviation in convict labor output decreased labor-force participation by 0.7 percentage-point for women, and 1.3 percentage-point for men. Evaluated using the Column V estimate, a county at the 25th percentile experienced an 0.5 (0.9) percentage-point larger fall in women's (men's) manufacturing employment share than a county at the 75th percentile. However, I find no evidence of increase in manufacturing employment share after convict labor was abolished in Panel B.

Thus, competition with prison-made goods indeed had a larger effect on female wages, at least during the introduction of convict labor. This table also suggests that public forms of convict labor were also deleterious to low-skilled workers. Because the legislation not only decimated private systems but also prohibited interstate trade of prison-made goods, prisons operated under the public-account system also decreased their output.[64] Moreover, as convicts remained employed in the clothing industry under the state-use system, women's wages did not fully adjust; thus in Panel B I don't find a statistically significant difference in wage estimates.

Finally, in Table A1.27 I check whether the effect of convict labor was driven only by Northern states. Each row contains estimates from a separate regression with continuous and discrete measures of convict labor. I present results for the changes in wages in Columns I-VI. The resulting coefficient is robust to the exclusion of Southern or Western states in Columns I-IV. However, in Panel A when we exclude Northern states, the estimate becomes insignificant for women but not for men. According to the Department of Labor (1887), less than 1% of convict labor output was in the clothing or shoemaking industries in non-Northern states, thus the whole effect on women's wages comes from the North and become insignificant when Northern states are omitted. Nevertheless, in Column VI for the continuous specification, the coefficient is negative and significant. While its

[64] Output of prison-made goods increased in the clothing industry under the state-use system from 1932 to 1940. This increase was most likely driven by the increase in military contracts, as WWII had already started.

magnitude is smaller than of the estimate on the full sample, it suggests that there was also a wage effect although less pronounced in the South. In Panel B, however, the exclusion of Northern states led to the opposite situation. We see a significant increase in women's wages following the decrease of convict labor in Column V and zero effect in Column VI. This result is in line with the fact that clothes and shoes became dominant industries for prisons. Columns VII-XII suggest that labor-force participation was indeed hindered by convict labor, but it was mostly a Northern thing.

1.5.2 Convict Labor and Technology Adoption

As competition with prison-made goods was tough, and despite the decrease in wages, firms could not employ free laborers for (near) zero wage.[65]Some firms had to close, partially or entirely; some survived. However, the prisons were producing low-quality goods and high-end markets were less affected.[66] Thus firms could "innovate away" from the competition with prison-made goods. The first option was to switch to production of high-quality goods ("We are trying to meet the situation by producing a better garment that will command a higher price." and "We have found it impossible to compete in price with prison-made stoves. Our only method is to produce a higher grade article.") or buy higher-quality materials

[65]In addition, some manufacturers were arguing that states did not tax prisons, and often bought new equipment for the prison using taxpayers money, thus making competition to be unfair not only in terms of the cost of labor. For example, all binder twine machinery in Minnesota, Kansas, North Dakota, Missouri, Indiana, Michigan, South Dakota, Wisconsin, and Oklahoma was bought by the governments of their states. Prison industries were exempt from paying federal, state, county, and municipal taxes (Department of Labor (1925); Sharkey and Patterson (1933)). Moreover, "a prison plant pays freight, and it may pay insurance, but its books show no payment for interest, depreciation, or carrying charges. These costs exist, nevertheless, and become a burden to the taxpayers."

[66]Quality was not only low in the clothing industry but also in industries that required standardized quality. For example, one of the largest free-labor manufacturers of twine in Minnesota noted: "The most popular twine is "Standard" twine which is supposed to run, and is labeled to run, 500 feet to the pound. The free-labor twine is made under laws that require it to fulfill its guarantee, but State owned and operated plant is not amenable to its own State and can not be made to live up to honest mercantile standards, and, in fact, in a great many cases does not."

that require less labor input ("When poorer material or less trimming is used, more work is done."). The second option was to improve their technology to make it less labor-intensive and allow them to create the same type of good cheaper or with better quality ("We have to be constantly producing new styles and each new style makes additional expense.").

I show graphical evidence of the positive effect of the introduction of convict labor on changes in patenting in Figure 1.16. I plot the changes in the number of patents in industries competing with prison-made goods in 1880-1900s on the vertical axis against the value of goods produced in prisons in 1886 on the horizontal axis. I find that counties that were more exposed to convict labor had experienced larger increase in the number of registered patents.

Table 1.4: Convict Labor and Technology Adoption: Introduction of Convict Labor

	Introduction of Convict Labor (1870-1886)							
	I	II	III	IV	V	VI	VII	VIII
Outcome (1880-1900):	Δ log Patents in Competing Industries		Δ log Patents in Noncompeting Industries		Δ Capital-Labor Ratio		Δ log Patents in Competing Industries	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
							Placebo (1860-1870)	
Convict Labor (Continuous)	0.030	0.071*	-0.006	-0.095*	50.354***	147.448***	-0.127***	-0.293***
	(0.020)	(0.043)	(0.014)	(0.054)	(16.230)	(39.661)	(0.029)	(0.090)
R-squared	0.242	0.235	0.162	0.131	0.255	0.217	0.105	0.037
Kleibergen-Paap F-stat		6.25		6.25		17.17		5.72
Observations	2,356	2,356	2,356	2,356	2,230	2,230	2,034	2,034

Exposure to convict labor is normalized. All columns contain a constant. The following variables are used as controls (in changes): ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, ln of value of farm products, log of number of slaves in 1860 (level), and log of market access (the change and the base level of 1870). All columns have corresponding lagged outcome variable (level) as a control. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

I start by replicating the specification from Section 1.5.1.1 and study the effect of introduction of convict labor on patenting. Results are presented in Table 1.4. I regress change in log of patents in affected industries on exposure to convict labor in Column I. While OLS coefficient is positive, it is not significant. In Column II I employ IV, and the estimates become significant: comparing county at 25th percentile of exposure to convict labor and 75th percentile, the more exposed one would have experience 6.6 percentage-points larger growth in patents in competing industries. In Columns III and IV I show results for falsification test, where I estimate the effect of convict labor on patents in industries that did not employ prisoners: both resulting coefficients are negative. I also show supporting evidence for my findings related to changes in industry composition in Columns V and VI. In Columns VII and VIII, I do a placebo regression for number of registered patents and regress changes in log patents in 1860-1870 on the exposure to convict labor in 1886. However, if any effect is present, it is negative. Benchmarking these results I find, that introduction of convict labor resulted in 6% higher growth rates in patents in industries that competed with convict labor, and 9.8% higher increase in capital-labor ratio.

In Table 1.5, I try to unveil the mechanism behind the increase in capital-labor ratio by using firm-level data. I aggregate firm-level data on industry-state-decade level in order to be see the effect of convict labor on industries. I consider the following specification:

$$y_{i,s,t} = \alpha_s + \beta_t + \xi_i + \gamma CL_{i,s,t} + \Pi \mathbb{X}_{i,s,t} + t\delta_s + \varepsilon_{i,s,t}. \tag{1.6}$$

Unit observation is the firm industry i in state s at decade t, and $y_{i,s,t}$ is a dependent variable. Similarly to the previous firm-level specifications I only use discrete measure of convict labor in industry i in state s at decade t. I cluster standard errors on state level.[67]

In Column I-III, I present OLS results of regression of exposure to convict labor on share of firms of industry i in state s on the total number of firms in state s. Column I reports specification with industry, state and decade fixed

[67] Two-way clustering by industry and state yields similar or smaller standard errors, and I don't report them.

effects. One percent increase in convict labor output decreases number of firms in that industry by 0.7%. In Columns II and III I add industry-year fixed effects, and state-specific trends, respectively. The magnitude of the coefficients increase and remain significant. It suggests, that firms in affected industries run out of business. Columns IV-VI contain similar specification. The dependent variable is the average capital per firm in industry i in state s. I find, that 1% increase in convict labor output is associated with (at least) $720 increase in average capital. At the same time, I also observe increase in capital-labor ratio (Columns VII-IX). These findings suggest, that capital labor increased not only because firms in affected industries shifted to better machinery to compete with prison labor, but also because more labor-intensive firms died out thus changing industrial compositions in their states and counties.

Table 1.5: Convict Labor and Wages: Firm-Level Data

	I	II	III	IV	V	VI	VII	VIII	IX
Dependent Variable:	Share of Firms in the Industry			Capital-per-Firm			Capital-Labor Ratio (K/L)		
Convict Labor	-0.007	-0.009*	-0.010**	719.6**	882.4**	935.5**	138.9*	195.2*	237.2*
	(0.005)	(0.004)	(0.004)	(334)	(364)	(375)	(78)	(111)	(140)
Industry x Year FE		X	X		X	X		X	X
State-specific trends			X			X			X
R-squared	0.901	0.927	0.933	0.381	0.701	0.736	0.28	0.32	0.35
Observations	293	293	293	293	293	293	293	293	293

Both values of exposure to convict labor are normalized. All columns contain constant and log value of manufacturing output. Robust clustered by state standard errors in parentheses. Standard errors clustered by state and SIC two digit industry codes are in square brackets. *** p < 0.01, ** p < 0.05, * p < 0.1

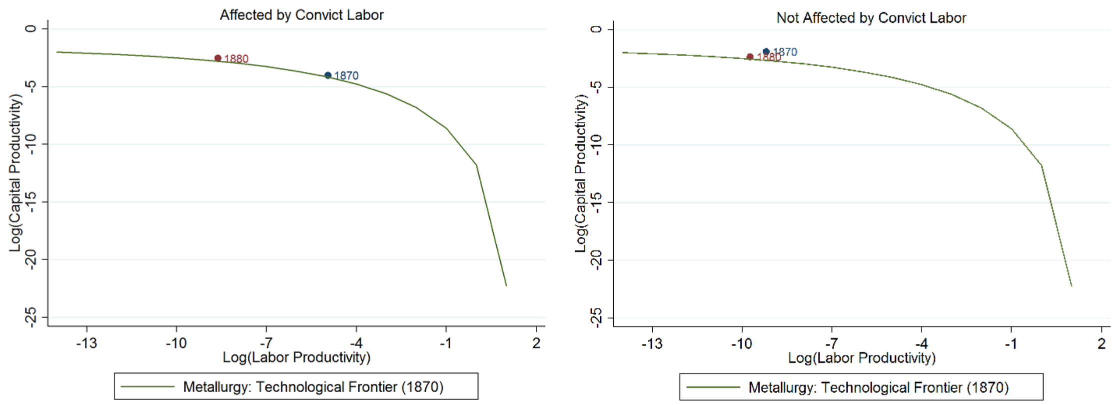
Thus, I find that convict labor increased aggregate county-level capital-labor ratio. Firms started to buy better machinery to produce higher-grade goods ("We have put in every modern machinery and process that we know of to produce our goods at a minimum cost.") (Department of Labor (1925), pp. 111-113). Possible explanation behind counter-intuitive result (from a 2-factor model perspective) that convict labor mostly affected firms that were producing low-quality goods, which could be replaced by low-skilled laborers with the help of necessary machinery, while firms producing higher grade goods were less likely to suffer (e.g., the

Amish shops in Holmes and Stevens (2014)). High-labor intensive firms in affected industries did not survive. Moreover, because surviving firms had to switch their production line to better quality goods, that compete less with prison products, I expect them to be more capital-intensive, or substitute their low-skilled laborers with capital.[68]

Another way to address the increase in patenting and shift to more capital-intensive technologies is to use methodology developed in Caselli and Coleman (2006). They assume that there is a trade-off between technologies favoring productivity of capital and productivity of labor, and that the same output can be achieved by choosing various combinations of both values. The output is maximized if chosen productivity of labor and capital are on the technological frontier — the "budget constraint" for firms' productivity choice set. Using authors' methodology, I document that firms located in counties affected by competition (in the same industry) with prison-made goods invested in technologies associated with the increase in productivity of capital, and disinvested in the technologies that improve productivity of labor. At the same time firms located far from the competition with prison-made goods (or firms in non-affected industries) did not experience much changes. I provide an example in Figure 1.7. On the left side I depict the technological frontier for metallurgic firms in 1870 located in areas that were subject to high competition with prison-made goods in 1886. On the horizontal axis I depict log of productivity of labor, and log of capital productivity is plotted on the vertical axis. Clearly, after convict labor was enacted, affected firms shift to more capital-intensive technologies. Moreover, the technological frontier of affected firms moved slightly upward. On the right side I show firms

[68] The latter also indicates possible directed technological change, as return to capital become larger relative to return to labor. In vein of Acemoğlu (2002), and Acemoğlu and Autor (2011) return to skills/college premium, in Panel A of Figure 1.14 I show that counties more exposed to competition with prison-made goods had higher return to capital. Finally, following Hanlon (2015) I show, that those counties also had larger number of registered patents in industries in which convicts used to be employed in 1886. Thus convict labor boosted technology adoption by forcing firms to invent or adopt new technologies that could make them more competitive.

Figure 1.7: Technological Frontier in Metallurgy: Affected vs. Not Affected Firms



in metallurgy industry that were not affected by the competition. Their capital and labor productivity did not change much from 1870 to 1880, and, if so, the technological frontier moved inward.[69]

Overall, the technology-adoption hypothesis is confirmed: counties more exposed to competition from prison-made goods either adopt existing technologies or contribute to new technologies, resulting in substitution of labor with capital.

In Appendix 1.8, I present the same robustness checks as I do in previous sections. In addition, as a falsification test in Table A1.31, I present regression of convict labor on patents in areas where prisoners were not employed. While the resulting coefficients are not precisely estimated zeroes, they are insignificant on conventional 90% level, supporting the hypothesis that patenting was a reaction on competition with prison-made goods.

[69] More detailed description of Caselli and Coleman (2006) model and its implementation in my setting is presented in Appendix 1.8. I provide results for other industries in Figures A1.17 and A1.18.

1.6 The Effect of Convict Labor on Wages and Firms: Panel Specification (1850-1940)

In the previous section, I studied effects of introduction of convict labor system in the United States. Using the it as a nation-wide movement uncorrelated with the local economic conditions, and addressing endogeneity concerns with the IV strategy based on the pre-convict-labor era prison capacities I showed that competition with prison-made goods adversely affected wages in manufacturing and manufacturing employment, while increased patenting in the affected industries. In this section I study the effect of convict labor in 1850-1940 using cross-sectional and temporal variation in convict labor.

1.6.1 Convict Labor and Local-Labor Market Outcomes

I start my analysis with the OLS regression of wage on convict labor output:

$$\ln\left(\text{Wage}\right)_{c,t} = \alpha_c + \beta_t + \gamma CL_{c,t} + \Psi \mathbb{X}_{c,t} + t\delta_s + \varepsilon_{c,t}. \tag{1.7}$$

The dependent variable $\ln\left(\text{Wage}\right)_{c,t}$ is the log of the average annual wage (nominal) in manufacturing in county c at decade t, $t \in (1860, 1940)$. Variable $CL_{c,t}$ is the exposure of county c at decade t to convict labor; $\mathbb{X}_{c,t}$ is a matrix of county-level controls; α_c, and β_t are county and decade fixed effects; and $t\delta_s$ are state-specific time trends. I weight by population in 1890. As convict labor is a state policy I cluster errors on the state level.[70]

I use the same continuous and discrete measures of exposure to convict labor as in the previous section; Both measures are equal to zero for $t = 1860$ and

[70] All results hold if I use weighting by population in other years, if I use weighting by market access in 1870 or 1890, or do not weight at all.

$t = 1870$ and equal to the non-zero for other decades:

$$CL_{c,t}^{\text{continuous}} = \sum_{i \in I} \left(\lambda_{i,c} \times \sum_{p \in P_t} \frac{\ln\left(\text{Value of goods produced}_{i,p,t}\right)}{\text{Distance}_{c,p}} \right), \quad (1.8)$$

$$CL_{c,t}^{\text{discrete}} = \sum_{i \in I} \left(\lambda_{i,c} \times \ln \left(\sum_{p \in P_{c,t}} \left(\text{Value of goods produces}_{i,p,t}\right) \right) \right). \quad (1.9)$$

My main identifying assumption is that there are no other variables that are correlated with exposure to convict labor and have effects on manufacturing outcomes. This model cannot account for unobserved factors that vary by county and over time and are correlated with the prevalence of convict labor.[71] This model has the same endogeneity concerns as the cross-section of first differences. The main difference of this model is that I use more variation in dependent and explanatory variables, and that I can add state-specific time trends. It helps to control for the fact that some states chose to close some prisons and open new ones, and to control for changing state-level legislation on prisons and convict labor.[72]

The model also helps with the scaling measurement error concern related to under-reporting: as county fixed effects and state linear trend allow this concern to be valid if there are differential trend in under-reporting.

Overall, OLS estimates will be also biased because of endogeneity in the amount of prison-made goods, and endogeneity in selection into building a prison when convict labor is already allowed. Thus concerns about omitted variable bias

[71] The two-way fixed-effect approach fails if I have an omitted variable whose differential trend is correlated with the differential trend in the dependent and explanatory variables.

[72] For example, changing systems of convict labor, laws requiring all prison output to be labeled "Made in Prison," or other state-specific legislation (e.g., prohibiting prisoners from working as hatters in New Jersey or prohibiting prisons in Massachusetts from buying new machinery).

suggest that I will more likely underestimate the effect of convict labor on wages.

To deal with the embedded endogeneity problem, I use IV estimation. The panel nature allows me to use the interaction of two different sources of plausibly exogenous variation. First, cross-sectional variation comes from the state prisons that existed before the years when convict labor was allowed in corresponding states (hereafter old prisons). Old prisons were built without consideration of manufacturing goods for profit, and their locations can be considered exogenous, conditional on population and urban share.

Second, as a source of time variation, I use the timing of adoption of private systems that authorized the use of convict labor for manufacturing goods on the open market. In particular, I use the fact that after 1870 there was a differential change in the amount of prison-made goods: from near zero dollar amount, mostly stone production, to at least 0.5% of GDP in 1886. Private systems enabled prisons to buy machinery, and provided foremen who organized prisons into firms. At the same time, the replacement of private systems with public ones followed the enactment of local anti-convict-labor legislation, which made convict labor less effective and less destructive to free labor, which led to worse managerial practices and less control of inmates. Moreover, prisons were operated by their wardens; all contracts and decisions about the employment of prisoners were made at the prison level, and the timing of state-level laws was plausibly exogenous.[73]

To conclude, I assume that the interaction of preexisting prisons and the timing of adoption of private systems are uncorrelated with the error term, are good

[73]Because old prisons were concentrated in the Northern states (see Figure A1.9), the instrument rather identifies LATE of competition with prison-made goods produced in the Northern states under private systems. To estimate the overall effect of convict labor on manufacturing wages in the whole U.S. and for the whole spectrum of convict labor systems, I use the actual number of prisoners (both employed and idle) as an instrument for value of goods produced. However, identification comes with a cost of relaxing the assumption that location of new prisons cannot be strategically chosen for the sake of maximizing profits from convict labor. Results of this specification are provided in Appendix 1.8. While resulting coefficients are comparable in magnitude, and, the first-stage relationship is stronger, I choose not to use it as a baseline specification.

predictors of the usage of convict labor, and do not directly affect wages in manufacturing.[74]

The first stage of the 2SLS specification can be written as:

$$CL_{c,t} = \tilde{\alpha}_c + \tilde{\beta}_t + \tilde{\gamma}\text{Old Prisons}_{c,t} + \tilde{\Psi}\mathbb{X}_{c,t} + t\tilde{\delta}_s + \epsilon_{c,t}. \tag{1.10}$$

And the second stage can be written as:

$$\ln\left(\text{Wage}\right)_{c,t} = \alpha_c + \beta_t + \gamma\widehat{CL_{c,t}} + \Psi\mathbb{X}_{c,t} + t\delta_s + \varepsilon_{c,t}, \tag{1.11}$$

where the variable Old $\text{Prisons}_{c,t}$ measures exposure of each county by the <u>old</u> prisons around it: Old $\text{Prisons}_{c,t} = \sum_{p\in P}\left(\frac{\ln\left(\text{Old prison capacity}_p\right)}{\text{Distance}_{c,p}}\right)\cdot\mathbb{D}\left(\text{private}=1\right)_{s,t}$, Old prisoncapacity_p equal to actual time-invariant <u>old</u> prisons capacities, and $\mathbb{D}\left(\text{private}=1\right)_{s,t}$ is an indicator function that is equal to one if state s that contained prison p had already adopted private system at decade t, and zero if state's did not or had already completely switched to public systems.[75]

Results are shown in Table 1.6. The first four columns contain results for the full sample of states, while the last six contain results for the subsamples of states. For the full sample, in Columns I and III, I present an OLS regression,

[74] Following Conley et al. (2012), I check the sensitivity of the identifying exclusion-restriction assumption in Appendix 1.8. In Appendix 1.8 I also use a difference-in-differences specification based on the same source of identifying variation but different identifying assumptions, and I show that results are comparable across the models.

[75] Results are robust to the specification with a "discrete" instrument (Appendix 1.8). In this case, if a state had adopted a private system only counties with <u>old</u> prisons have nonzero values. The timing of laws can be plausibly endogenous if prison wardens can lobby convict-labor related legislation in their state. However, historical literature does not provide evidence that it took place. It also would be unlikely due to partisan composition of state legislators and even if they can pass such law, the timing would be relativly unpredictable. I address this concern in the next Section where I use only cross-sectional component of my instrument in the first-differences specification.

and I show second-stage results of the IV specification in Columns II and IV. Columns with second-stage include the first-stage coefficient of the instrument on the explanatory variable. For the subsamples, I only provide results of the second stages.

The OLS point-estimate of wage elasticity in Column I is negative and significant. So is the IV coefficient in Column II. One standard deviation in exposure to prison-made goods decreases wages by 20%.[76] The difference between the county-level convict labor change in counties at the 25th and 75th percentiles was a 0.63 standard deviation. Evaluated using the Column II estimate, a county at the 25th percentile experienced an 12.6 percent larger manufacturing wage decrease (or 2 percent larger decline in mean log annual wages in manufacturing) than a county at the 75th percentile. The direction of IV bias supports the selection concerns raised above: prisons were built in areas where wage increases faster, and the fact, that measurement error bias is present. The estimated effect may be smaller than the true effect since I use average wages in manufacturing, which include wages of white-collar workers and high-skilled laborers.[77]

In Columns III and IV, I use the discrete measure of exposure to convict labor. This specification also alleviates the concern that distances to prisons correlate with manufacturing outcomes.[78] The OLS coefficient becomes smaller while significant, but the second stage coefficient increases slightly in magnitude. One standard deviation in exposure to prison-made goods decreases wages by 24%. The most plausible explanation for this effect is that the effect of competition with

[76]The first-stage F-statistics is 18, and the Anderson-Rubin test is rejected, suggesting that the instrument is strong. Partial R^2 is equal to 0.06, indicating that the first-stage power is driven by the instrument, rather than the variety of fixed effects, trends, and controls.

[77]In Appendix (see Table A1.28) I also use specifications with county-specific trends. While they are very restrictive, they alleviate some concerns about differential pretrends due to overall industrialization. Resulting coefficients and first-stage F-statistics do not differ from those obtain in specifications with state-specific trends.

[78]It also helps if spatial patterns in adoption of convict labor laws bias my results (Dube et al. (2016)).

Table 1.6: Convict Labor and Wages: Panel Specification

	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent Variable: ln of Wage in Manufacturing									
Sample	Full Sample				w/o South		w/o West		w/o North	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	-0.06***	-0.20***			-0.18***		-0.20***		-0.05**	
	(0.008)	(0.040)			(0.039)		(0.042)		(0.022)	
Convict Labor (Discrete)			-0.02***	-0.24***		-0.19**		-0.24***		-0.14**
			(0.005)	(0.088)		(0.076)		(0.088)		(0.055)
R-squared	0.841	0.774	0.838	0.601	0.794	0.632	0.776	0.589	0.796	0.794
Kleibergen-Paap F-stat		17.84		7.203	15.06	6.075	16.49	7.007	86.85	17.63
Instrument's coefficient		0.44***		0.43***	0.41***	0.48***	0.45***	0.44***	0.59***	0.17***
		(0.106)		(0.130)	(0.114)	(0.158)	(0.110)	(0.134)	(0.086)	(0.026)
# States	41				29		30		22	
Observations	15,366	15,364	15,366	15,364	8,685	8,685	13,180	13,180	8,863	8,863

Both values of exposure to convict labor are normalized. All columns contain constant, county, and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, employment share in manufacturing, employment share in agriculture, ln of value of farm products, number of dwellings, and state-specific linear trends. Columns with second-stage include the first-stage coefficient of instrument on the explanatory variable. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

prison-made goods exceeded the boundary of a county, and thus in the discrete specification, I count partially treated counties (close to counties with prisons) as control counties. Thus the differences in wages between them is smaller, and I underestimate the effect of convict labor.

Regressions on the subsamples help us to identify where the effects on wages derive from.[79] In Columns V and VI of Table 1.6, I drop the Southern states; the resulting second-stage coefficients are not statistically different from those in the full sample, while standard errors increase slightly.[80] In Columns VII and VIII, where I omit Western states coefficients increase slightly in magnitude. Finally, in Columns IX and X, I omit Northern states; negative effect of wages remains significant, but the magnitude of the effect become much smaller. It suggests that convict labor affected wages in manufacturing mostly in the Northern states. The following explanation could support this finding. Most convicts in

[79] I use U.S. Census Bureau definitions of the U.S. regions. Western states include Great Plains and the Far West. Northern states include the Midwest and Northeast.

[80] While the number of clusters is below the "rule of 42" (Angrist and Pischke (2008)), Hansen (2007) finds that Stata's cluster command is reasonably good at correcting for serial correlation in panels, even in 10 clusters scenario. Although OLS specifications remains significant if I use wild bootstrapping (Cameron et al. (2008)).

the Southern and Western states were employed in farming, timber production, mining/quarrying, or road construction sectors. None of these sectors counts as manufacturing, thus my dependent variable excludes them by construction.[81] Border Southern states, however, could still be affected by the goods produced by nearby Northern prisons.[82]

While in Figure 1.12 I graphically demonstrated that the introduction of convict labor decreased labor-force participation manufacturing employment share in Table A1.29, I do not find evidence that convict labor caused unemployment and decreased the labor-force participation rate in the panel specification. One plausible explanation is that convict labor may have affected labor-force participation only at the time it was introduced (and abolished). Thus in the next Section I show the effect of introduction of convict labor labor market outcomes.

1.6.2 Convict Labor and Firms

I use the same specification to estimate the effect of convict labor on patenting. I expect that with rapid growth of population and urbanization, the growth rate in the number of patents will be higher outside of preexisting urban centers, where most of old prisons were located. Thus, I will also most likely underestimate the effect of convict labor on patents. Results are shown in Table 1.7. The first four columns contain results for the full sample of states, while the last six columns contain results for the subsamples of states. For the full sample, in Columns I and III, I present OLS regression; I show second-stage results of the IV specification in Columns II and IV. Columns with second-stage results include the first-stage

[81]The share of farming and road construction rose over time in the South. Unions were fighting against the use of convict labor in manufacturing, and states were leasing prisoners to plantations or chain gangs instead of employing them in factories.

[82]First stage F-statistics in Columns IX and X are higher than in Columns II and IV, respectively, since old prisons existed there for decades. At the same time prisons in the North were more likely to be closed and new prisons were more likely to appear in other locations as a replacement.

coefficient of the instrument on the explanatory variable. For the subsamples, I only provide results of the second stages.

In Panel A, I report results for the county-level number of patents registered in the next decade. Counties more exposed to prisons had more patents registered. The difference between the county-level convict labor change in counties at the 25th and 75th percentiles was a 0.6 standard deviation. Using estimate from Column II, the more exposed county would be expected to experience 0.8 standard deviation larger number of registered patents per 10,000 people. Results of the discrete measure yield similar results, though the magnitude of the coefficient drops.

In Panel B I test if convict labor affected industry composition, and capital-labor ratio in particular. Both OLS and IV coefficients are positive and significant. Column II shows that one standard deviation in exposure to convict labor increases the capital-labor ratio by 67.6, or 7% of its standard deviation. Evaluated using the Column II estimate, a county at the 75th percentile experienced a 3.4 percent larger increase in mean capital-labor ratio than a county at the 25th percentile. Similarly, Columns III and IV show results for the sharp measure of convict labor. Their magnitude is smaller than that of continuous measure, similar to the results for wages in Section 1.5.

Table 1.7: Convict Labor and Technology Adoption: Panel Specification

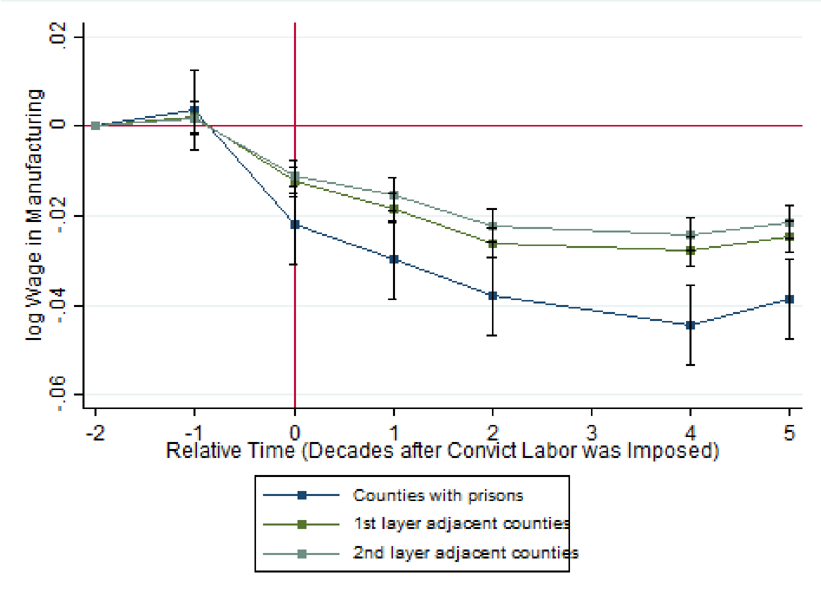
Panel A	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent Variable: Number of Patents in Competing Industries per 10,000 people									
Sample	Full Sample				w/o South		w/o West		w/o North	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	0.01***	0.03**			0.03***		0.03**		0.00	
	(0.002)	(0.012)			(0.011)		(0.012)		(0.000)	
Convict Labor (Discrete)			0.00***	0.02**		0.02***		0.02**		0.00
			(0.001)	(0.007)		(0.006)		(0.007)		(0.000)
R-squared	0.991	0.953	0.991	0.939	0.961	0.948	0.955	0.942	0.983	0.983
Kleibergen-Paap F-stat		16.52		13.87	13.91	11.41	15.53	13.29	40.11	25.10
Instrument's coefficient		0.46***		0.36**	0.43***	0.39**	0.45***	0.37**	0.60***	0.19***
		(0.114)		(0.144)	(0.121)	(0.167)	(0.117)	(0.148)	(0.096)	(0.059)
Observations	16,371	16,366	16,371	16,366	10,073	10,073	13,930	13,930	8,729	8,729
Panel B	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent Variable: Capital-Labor Ratio (K/L)									
Sample	Full Sample				w/o South		w/o West		w/o North	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	17.85*	67.64***			81.96***		67.64***		74.55**	
	(10.611)	(24.958)			(29.081)		(25.668)		(36.665)	
Convict Labor (Discrete)			14.48*	27.02**		28.85**		27.22**		77.16**
			(8.168)	(12.354)		(12.529)		(12.759)		(36.687)
R-squared	0.615	0.412	0.616	0.416	0.479	0.484	0.415	0.419	0.307	0.303
Kleibergen-Paap F-stat		11.01		33.92	7.255	26.38	10.49	32.24	4.909	15.63
Instrument's coefficient		0.45***		0.43***	0.41***	0.47***	0.45***	0.43***	0.59***	0.17***
		(0.105)		(0.133)	(0.111)	(0.161)	(0.108)	(0.137)	(0.086)	(0.026)
Observations	7,859	7,722	7,859	7,722	4,243	4,243	6,974	6,974	4,227	4,227

Both values of exposure to convict labor are normalized. All columns contain constant, county, and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, employment share in manufacturing, employment share in agriculture, ln of value of farm products, number of dwellings, number of slaves (for 1850 and 1860), and state-specific linear trends. Columns with second-stage results include first-stage coefficient of the instrument on the explanatory variable. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

1.7 Robustness and Sensitivity Checks

To be sure that counties located closer to prison were more severely affected than those that are farther away, I use an event-study design. I run a regression of the log value of goods produced in 1886, on log wages in manufacturing, controlling only for decade, and state fixed effects and log manufacturing output. Resulting coefficients are presented in Figure 1.8. I present the result for the counties that had prisons with the blue line. The OLS coefficient becomes negative and significant as soon as convict labor laws was enacted, and the effect persisted afterwards. Then I repeat the same regression, but I treat counties adjacent to counties with prison. Resulting coefficients are smaller in magnitude that those for counties with prison. Thus, the effect was smaller in nearby counties. Finally, I show that estimates become even smaller, when I use adjacent counties to counties that are adjacent to counties with prisons.

Figure 1.8: Convict Labor and Log Manufacturing Wages: Event Study



Each square is the coefficient of the event-study regression of the log wages in manufacturing on time-invariant log value of convict labor output in 1886 in a county, interacted with decade dummies. Relative time (in decades) is plotted on the horizontal axis, such as 1880 is counted as 0 – first decade when convict labor is imposed. Wages data for 1910 is not available. I omit dummy, but results are unaffected if I use other as the baseline. I use state, and decade fixed effects, and log manufacturing output in a county as controls. Dark blue line corresponds for regression where I treat counties that had convict labor in it as treated. Green line treats counties that are adjacent to counties that had prisons. Gray line assumes that counties that are adjacent to counties that adjacent to prisons as treated. Results hold if I double-count adjacent counties that are adjacent to more than one county with prison. 95% confidence intervals are depicted.

My main dependent variable depends both on distance and on industry composition. In Columns I-IV of Table 1.8 I show that my results do not depend on industry weighting. I use the following measures of exposure to convict labor:

$$CL^{\text{'continuous}}_{c,t} = \left(\sum_{p \in P_t} \frac{\ln\left(\sum_{i \in I} \text{Value of goods produced}_{i,p,t}\right)}{\text{Distance}_{c,p}} \right), \quad \text{and}$$

$$CL^{\text{'discrete}}_{c,t} = \ln\left(\sum_{p \in P_{c,t}} \left(\sum_{i \in I} \text{Value of goods produces}_{i,p,t} \right) \right) \tag{1.12}$$

In Columns I and II, I show OLS and 2SLS for the continuous measure of exposure to convict labor without industry weighting. Both coefficients statistically don't differ from those in Columns I and II in Table 1.6. I present results for the discrete measure in Columns III and IV. While the OLS coefficient is the same as in Column III of Table 1.6, the 2SLS estimate in Column IV is 3 times smaller. This is intuitive since the variable takes into account both products that were competing with local firms and products that did not.[83]

In Columns V and VI, I restrict the sample to counties that had a prison at least in one year in my panel. The OLS coefficient essentially becomes zero, but the second stage coefficient remains significant. Thus the effect of convict labor is not only the extensive margin effect of comparison counties with prisons with counties without prisons, but also exists on the intensive margin. Similarly, in Columns VII and VIII, I show the same specification, but add industry weighting. Results remain significant, while 2SLS estimate is almost twice smaller than one in the baseline specification (Column IV of Table 1.6).

Finally, in Columns IX -XII, I show results of a placebo regressions. I assume

[83]In Table A1.30 also I report results of the specification with only industry weighting: $CL^{\text{"continuous}}_{c,t} = \sum_{i \in I} \left(\lambda_{i,c} \times \sum_{p \in P_t} \ln\left(\text{Value of goods produced}_{i,p,t}\right) \right)$. Since in this specification the total value of prison-made goods is a nation-wide shock, my instrument (exposure to capacities of old prisons) is not meaningful. Thus I only use OLS specification. Resulting estimates are significant and slightly smaller in magnitude in comparison to corresponding OLS coefficients in Tables 1.6 and 1.8. These results are also in line with slopes in Figure 1.4.

that convicts employed in farming did not compete with manufacturing workers. Thus I expect, that the value of convict labor output in farming to have no adverse effect on manufacturing wages. Neither continuous nor discrete specification result in significant estimates. However, the first-stage F-statistics is also low for placebo specifications. A possible explanation is that most of farming was done under convict leasing system, which did not rely on preexisting prison capacities.

Table 1.8: Convict Labor and Wages: Robustness Checks

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
	Dependent Variable: ln of Wage in Manufacturing											
	Robustness Checks								Placebo			
	w/o Industry Weights						w Industry Weights		w/o Industry Weights			
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict Labor (Continuous)	-0.07***	-0.22***										
	(0.015)	(0.027)										
Convict Labor (Discrete)			-0.02***	-0.08***	-0.00	-0.06***	-0.00	-0.13**				
			(0.003)	(0.015)	(0.003)	(0.023)	(0.003)	(0.065)				
Convict Labor in Farming (Continuous)									-0.01	0.05		
									(0.016)	(0.155)		
Convict Labor in Farming (Discrete)											-0.01	3.14
											(0.009)	(4.108)
Kleibergen-Paap F-stat		43.6		18.4		10.3		4.9		2.4		0.5
Sample	Full				Counties with Prisons				Full			
Observations	15,366	15,364	15,366	15,364	2,985	2,985	2,985	2,985	10,561	10,555	13,862	13,859

Both values of exposure to convict labor are normalized. All columns contain constant, county, and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, employment share in manufacturing, employment share in agriculture, ln of value of farm products, number of dwellings. Columns I-IV include state-specific linear trends. Columns V-VIII contain state-specific linear trends. Columns with second-stage include the first-stage coefficient of instrument on the explanatory variable. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

1.8 Discussion of the Contemporary U.S. Convict Labor System and Concluding Remarks

Convict labor has always been controversial topic riven with acrimony. New England settlers wanted to remedy the supposed moral failures of criminals by forcing them to perform hard labor, and today its proponents argue that in-prison labor creates skills needed the after-release employment and saves the state money. However, while the debate about whether convict labor is the best way to rehabilitate inmates is ongoing, externalities of convict labor have never been thoroughly studied.

In this paper, I show that coercive institutions that appeared in the United States after the Civil War affected the economic welfare of free laborers. I document that convict labor decreased wages in manufacturing, especially for women. At the same time, it hastened technology adoption and capital investments that allowed firms competing with prisons to thrive. In my follow-up papers (Poyker (2018a) and Poyker (2018b)) I show that convict labor demonstrably exacerbated inequality of opportunities. Poyker (2018b) finds that regions historically exposed to a more severe exploitation of convict labor are still worse in terms of absolute upward mobility and better of in terms of relative upward mobility. My results suggest that the divergence of welfare of wage earners and capital owners was the driving force of growing persistence in intergenerational mobility in the Northern states, while in the South, this persistence was driven mainly by the increase in incarceration rates.

Troubling the private use of convict labor was allowed again in 1979. Convict labor benefits specific interest groups and institutions in the federal and state prison systems, as well as private prison companies. Federal Prison Industries, a U.S. government corporation operating under the federal Bureau of Prisons (BOP) (with prison population of approximately 192,000) pays inmates roughly $0.90 an hour to produce a wide range of everyday products, from mattresses and spectacles, to road signs and body armor for other government agencies, earning $500 million in sales in fiscal 2016.[84] Meanwhile, state prison systems and private prisons often contract out prison labor to private manufacturing (e.g., inmates in North Carolina made lingerie for Victoria's Secret in the 1990s, and until 2016, prisoners in Colorado made goat cheese and raised tilapia for Whole Food's).[85] The current expansion of private prisons is heavily debated in the public space.[86]

[84] The Economist (2017); BOP (2017).

[85] The Washington Post (2015); NPR (2015).

[86] The trend also exists in other countries: for example, in Russia, the government started

The morning after Donald Trump was elected President, the share price of controversial private prison operator CoreCivic (Corrections Corporation of America) jumped 34%, while GEO Group stocks rose by 18% (Quartz (2016)). Even the BOP had been planning to stop renewing contracts with private prisons, the new Trump' administration has decided to extend contracts with several private prison companies.[87]

According to the U.S. Census of prisons and jails, more than 1 million prisoners were employed in 2500 U.S. prisons in 2000.[88] Some of those prisoners are assigned to halfway houses and are allowed to work outside the prison premises.

Two important observations are worth nothing. First, labor mobility has increased over time, shrinking the wage effect in the proximity of the prison. However, Autor et al. (2013) argue that U.S. labor mobility remains low. Second, as transportation costs have decreased over time, competition with prison-made goods may spread farther from the prison. Thus, the overall effect of convict labor on contemporary manufacturing wages could be smaller around the prison but larger overall. Moreover, the number of convicts has soared from approximately 80 thousand in 1920 to more than 2 million today.

While I observe neither the industry where prisoners are employed nor the value of goods produced, I attempt to elicit the magnitude of the effect of competition with prison-made goods on wages in manufacturing by using a back-of-the-envelope calculation.[89] Take Colorado State Penitentiary, located in the Fremont county. As of 2010, its capacity was 756 beds but it was expanded by adding ad-

convict leasing in 2017 and began allowing state-owned companies to house and employ prisoners for almost zero wages (The Moscow Times (2016)).

[87]See U.S. Department of Justice (2016) and CNN (2017).

[88]See Figure A1.14 for a map of contemporary prisons.

[89]To do it, I cannot use the estimate from Column IV of Table 1.6 with point-estimate $\gamma = -0.12$ ($se = 0.0023$). I choose the discrete specification, as it yields an estimate more conservative in magnitude, and because it is easier to interpret the results. Suppose that the relationship between convict labor and wages remains the same.

ditional 300 beds. This 39% increase in prisoners led to $0.39 \times 0.12 \times 100 = 4.68\%$ decrease in local wages in manufacturing. Even if my estimate is an upper bound of the effect and the actual effect is smaller, the contemporary policy of placing prisons in economically depressed regions may be fallacious. Thus the government should at least consider imposing welfare redistribution to local low-skilled workers to alleviate the effect of competition with convict labor.

My analysis highlights the fact that many aspects of economic life and many groups of people can be affected directly and indirectly by competition from prison-made goods. Thus when we evaluate the overall effect of the penitentiary system, we should carefully weight the long-run impact and the negative externalities created by convict labor.

Appendix A

Additional Background Information

Convict Labor and the Wages of Women in Manufacturing: The Case of Trenton and Jersey City

Here, I will provide a case study of New Jersey State Prison in the city of Trenton, Mercer county. One of the oldest prison in the United States it was opened in 1798 under the name of the Penitentiary House. Since then it adopted the Pennsylvanian prison system, where prisoners were serving solitude confinement and were doing some labor in their quarters while not in industrial scales (Stonaker (1913); Barnes (1918); Jackson (1927)). The first time the prison gain profit for the state was in 1873 when it reported $30000 of surplus earnings (NJ Treasury Dept (1873)). By 1886 it was operated under the piece-price system of labor and the value of goods produced by Trenton's prison was equal to $835859.60. The majority (63.4%) of it came from the production of men's "low grade" and "common" clothes, while the rest came from the production of boots and shoes production (30.7%) and brooms and brushes (5.9%) (Department of Labor (1887)).

At the same time, in the Secaucus, Hudson county, very close to the Jersey City, and since 1863 was operating a Jail and Workhouse at County Farm (State of New Jersey (1863)). By 1886, it was producing output comparable in the value $548740.5 with one in Trenton's prison. However, only 3.8% of its production was in men's clothing industry: they were producing men's clothes under state-use system and not for sale on the open market.[90] Another 2.6% of output came from road construction under the public works and ways system.[91] The rest 94.6%

[90] While we do not know what exactly they were doing, generally under the state-use labor system prisons were involved in creating clothes for state's inmates and employees. (Department of Labor (1887))

[91] This also means that it is hard to define the true market value of road construction.

came from the “stone quarrying, cutting, and crushing” under the public account system.

In the Table 1.9 provide the closest comparison of the clothing industries in the cities of Trenton and Jersey City in 1870 and 1890. As both cities are situated close to each other, both had prisons, however, one of them, plausibly endogenous chose to be involved in the production of men’s clothes, while other due to proximity to the stone quarry, become involved in quarrying instead. As can be seen, Trenton already had a small men’s clothes industry represented by 22 firms with 8.8 employees on average, while New Jersey had both men’s and women’s clothes industries with even smaller by size firms, and very different in terms of capital-labor ratio. In 1890, all clothing industries in Trenton were producing $991,011 worth of goods, while the local prison was producing clothes worth $530,047.2 in 1886. There was only one man’s clothing factory, too small to be included in the Census tables as a separate industry, and 37 small firms (6.7 employees on average), involved customary (not low grade) clothes and repairs. At the same time, women clothes industry was booming, with 159 very tiny firms and three factories.

These observations show no prima facie evidence against my hypothesis, since, there is almost no production in the same type of good where the competition with prison is the most severe, while men’s clothes industry entirely moved into the“custom work and repairing” where it can produce higher grade/non-common goods. In addition, we can observe, that the capital-labor ratio has tripled in “men’s custom work and repairing” industry, while is equal to $314.1 and $411.9 in women’s clothes industries.

Jersey City is quite different: in addition to factories producing women’s clothes, there also factories making men’s clothes. There also a comparable number of firms involved in custom work and repairing of men’s clothes. However, the capital-labor ratio of these firms (men’s and women’s factories) in Jersey City is lower than firms (and women's factories) in Trenton, that is also in line with the hypothesis.

The salary of male workers is approximately the same for men industries in Jersey City and men's custom work and repairing industry in Trenton.[92] At the same time, women's wage in those industries is lower everywhere in Trenton than in Jersey, as predicted by our hypothesis.

Jersey City also has three dressmaking firms employing five people, that may be non-representative to draw any conclusions. However, the fact, that there are only 3 firms, at the same time there are 159 small firms in Trenton may be explained by the low wage in Trenton. Women who didn't want to work with a new decreased wage preferred to stay at home and start their own individual business of making customary clothes. That's why we can observe that wages in women's dressmaking industry are approximately similar in two cities. At the same time, the female wage in Jersey City was not distorted and women chose to go to be employed.

Overall, the case study above shows that prison completely drives out industries competing in exactly the same type of goods, and forced firms operating in that industry to invest more in capital and make more special higher grade goods. In addition, a wage of low-skilled workers (females in clothing industry) was lower in a location with prison competition. This gives us two testable hypothesis: we expect counties with a higher value of an output of prison-made goods to increase capital-labor ratio and decrease wage of females in manufacturing.

[92] Male's average wage in women's clothes factory is based on the sample of four men only and may be not representative as they are most likely engineers who supervise the machinery.

Table 1.9: Case Study of Trenton and Jersey City

1870					1890						
Industry	# of firms	Persons employed	Average wage	K/L	Industry	# of firms	Males employed	Av. wage male	Females employed	Av. wage female	K/L
					Trenton						
Men's clothes	22	180	$178.5	$302.8	Factory prod. (men)	1*					
					Custom work (men)	37	149	$577.7	100	$212.8	$1,198
Women's clothes					Dressmaking (women)	159			153	$292.1	$314.1
					Factory prod. (women)	3	22	$224.1	20	$175	$411.9
					Jersey City						
Men's clothes	17	61	$397.5	$690.4	Factory prod. (men)	3	12	$520	26	$356.2	$262
					Custom work (men)	32	114	$615.5	9	$324.2	$958.7
Women's clothes	8	35	$98.6	$25.6	Dressmaking (women)	3			5	$317.6	$830
					Factory prod. (women)	4	4	$780	120	$249.6	$254.4

All values are in dollars of 1890. *The firm was too small to be entered separately, and was included in "other industries category". Source: 1870: Table XI pages 694-695 of the "Compendium of the Ninth Census". 1890: Table 3 pages 305, 635 of the "Report of Manufacturing Industries of the Eleventh Census."

Data Appendix

All individual and county-level data for controls are taken from U.S. censi (Haines (2004); Ruggles et al. (2015)). The first outcome variable, the capital-labor ratio is computed as the sum of the capital across all manufacturing firms in a county divided by the total wage paid in manufacturing in that county. The second variable – is computed by dividing total wage paid in manufacturing paid to female employees, divided by the number of female employees.

I work with county level data for the years 1850 to 1950. The data for the years 1850 to 1930 was obtained from Historical, Demographic, Economic, and Social Data: The United States, 1790-2000, ICPSR 2896 (Haines (2004)) with the exception of the variables manuf, agr, and other, which were built from individual level data from IPUMS from a 1% extract from the 1900, 1910, 1920 and 1930 Census respectively (Ruggles et al. (2015)).

For 1940 to 1950 each variable was built from two alternative data sources: the County and City Data Book [United States] Consolidated File: County Data, 1947-1977, from ICPSR 7736; and from Historical, Demographic, Economic, and Social Data: The United States, 1790-2000, ICPSR 2896 (Haines (2004)). In most cases the variable definitions are the same using these two alternative data sources. When the definitions are different, I use the one from ICPSR 2896.

Additionally, I use data on county topography and demographics from the paper "Data Set for Births, Deaths, and New Deal Relief During the Great Depression" by Fishback et al. (2007) generously made available on Price Fishback's website.

I drop counties in Hawaii or Alaska and underpopulated counties with population less than 1,000 in any decade in the 20th century.

The quality of some of the key variables is not ideal. Substantial measurement error is likely to be present at the beginning of our sample period. Moreover, in early years, direct data on workers wages are unavailable at the county level. As an expedient, I proxy for the average wage in manufacturing by dividing the total annual wage bill in manufacturing by the estimated number of workers in the industry. This is unlikely to provide a perfect measure of the marginal product of labor as it fails to account for differences in the number of hours worked and quality of workers. Moreover, in some counties, the wage bill is missing. For agriculture, the county wage bill is not available, so there is no good way to compute an average agricultural wage. More specifically, the key variables are the following:

- Total population of each county.
- Urban population in each county divided by the total population of each county. For 1850 to 1920 it was calculated as population residing in places of 2,500 or more persons. For 1930, 1940, and 1950, calculated directly as total urban population.
- Share of Population of African-American. Defined as the share of Black over total population.
- Share of employment in manufacturing. For 1850 to 1920 defined from individual level data as the number of individuals who reported working on manufacturing over the total number of individuals with reported industry. For 1930, defined as the average number of wage earners in manufacturing in 1929 over total employment. For 1940 defined as workers in manufacturing over total employment. For 1950, defined both directly as share of employment in manufacturing and also as workers in manufacturing over total employment for 1950.
- Manufacturing total employment. For 1850-1940 it was defined as the average number of manufacturing wage earners and for 1950 as manufacturing production workers.

- Share of employment in agriculture. For 1850 to 1930 defined from individual level data as the number of individuals who reported working on agriculture over the total number of individuals with reported industry. For 1940-1950, defined as workers in agriculture over total employment.

- Total county level manufacturing wages in thousands of dollars. For 1900, 1920, 1930, given in dollars, so divided by 1,000. For 1940 defined as 1939 wages. For 1950 defined as 1947 wages of manufacturing production workers.

- Total number of housing units. For 1850-1930 defined as total dwellings. For 1940-1950 defined as total housing units. Missing for 1860, 1870, 1880, and 1930.

- Total number of slaves for 1850 and 1860 from the slave census.

- Value of farm products in thousands of dollars. Total value of farm products for each county in thousands of dollars. For 1900, defined as the value of miscellaneous crops with acreage reported in 1899 plus the value of miscellaneous crops without acreage reported in 1899. For 1910-1930, defined as value of all crops divided by 1,000. For 1940, defined as value of all farm products sold, traded or used. For 1950, defined as value of all farm products sold, in thousands of dollars. Missing for 1850, 1860.

Estimating Technological Frontier

To estimate technological choice shifts I use methodology developed in Caselli and Coleman (2006). I start by assuming CES production function:

$$Y = \left((A_k K)^{\alpha\sigma} + (A_L L)^{(1-\alpha)\sigma}\right)^{\frac{1}{\sigma}}.$$

To compute productivity of capital (A_K) and productivity of labor (A_L) I use firm-level data from Atack and Bateman (1999).

First, I create an indicator variable "treated" equal to one if county c, industry i is in the top 33 percentile of the competition with prison-made goods in 1886. I also created variable "not treated" equal to one if county c, industry i is in the bottom 33 percentile of the competition with prison-made goods in 1886. I drop firms located in the middle third of the counties percentile.[93]

Second, I sum up output, capital, labor, and total wages over counties, treatment variable, and decade (1870 and 1880, before and after convict labor was introduced). I compute wages w as a ratio of total wages (wL) divided by total number of laborers (L). As I do not have price of capital (r), I compute it from the budget constraint ($r = \frac{Y-wL}{K}$).

Then I can compute capital and labor productivity:

$$A_K = \frac{Y}{K}\left(\frac{rK}{rK+wL}\right)^{\frac{1}{\sigma}}, \text{ and } A_L = \frac{Y}{L}\left(\frac{wL}{rK+wL}\right)^{\frac{1}{\sigma}}.$$

The main idea of Caselli and Coleman (2006) is that countries can specialize in adopting technologies favoring labor while making capital less productive, or adopting technologies favoring capital but making labor less productive.[94] Thus "technological frontier" can be interpreted as a budget constrain for two types of technologies. By investing in technology supporting in capital, you invest less in those helping labor. And being on the frontier means that firm maximizes its budget. In my case I measure technological frontier for industries in treated counties before and after convict labor was allowed. Industries in counties not affected by the competition are used as a placebo to show that they do not exhibit any similar changes.

Thus technological frontier can be written as follows:

$$Y = \left((A_K K)^{\alpha\sigma} + (A_L L)^{(1-\alpha)\sigma}\right)^{\frac{1}{\sigma}}$$

[93] I also tried to split counties by half and it results hold. However, if I use 25percentiles, I get to few firms in each industry and result become sensitive to parameters.

[94] Authors use high skilled labor vs. low skilled labor technologies in their model, but the idea is the same.

$$\text{s.t.} \qquad (A_K)^{\omega} + \gamma (A_L)^{\omega} \leq B,$$

where $\omega, \gamma, B > 0$; I also assume that $\omega > \sigma / (1 - \sigma)$.[95]

This system of equation can be solved and shown as a function of technological frontier (B):

$$A_K = \left(\frac{B}{1 + \gamma^{\sigma/(\sigma-\omega)} (K/L)^{\omega\sigma/(\sigma-\omega)}} \right)^{\frac{1}{\omega}}$$

$$A_L = \left(\frac{B/\gamma}{1 + \gamma^{\sigma/(\omega-\sigma)} (K/L)^{\omega\sigma/(\omega-\sigma)}} \right)^{\frac{1}{\omega}}$$

However, parameters ω and γ are unknown and needed to be estimated. To do so, I first derive both first order conditions and divide one on another to get the following equation:

$$\ln \left(\frac{A_K^i}{A_L^i} \right) = \frac{\sigma}{\omega - \sigma} \ln \left(\frac{K^i}{L^i} \right) + \frac{1}{\omega - \sigma} \ln (\gamma) .$$

Assuming, that γ differs for all observations we can estimate ω and γ from the regression:

$$\ln \left(\frac{A_K^i}{A_L^i} \right) = \underbrace{\frac{\sigma}{\omega - \sigma}}_{\tau} \ln \left(\frac{K^i}{L^i} \right) + \underbrace{\frac{1}{\omega - \sigma} \ln \left(\gamma^i \right)}_{\nu^i} .$$

As I have already estimated the productivity we know $\ln \left(\frac{A_K^i}{A_L^i} \right)$; $\ln \left(\frac{K^i}{L^i} \right)$ is also known. I estimate OLS to get coefficient $\widehat{\tau}$ and residual $\widehat{\nu^i}$. Then I get ω and γ^i by solving the following system of equations:

$$\frac{\sigma}{\omega - \sigma} = \widehat{\tau},$$

[95]Following Caselli and Coleman (2006) I use $\sigma = 0.09$. Results are robust to other values of σ that Caselli and Coleman (2006) use for robustness.

$$\frac{1}{\omega - \sigma} \ln \left(\gamma^i\right) = \widehat{\nu^i}.$$

Finally, I plot log of productivity of capital and labor for treated and untreated counties in 1870 and 1880 to demonstrate that firms in affected counties and industries investment in more capital-intensive technologies.

Additional Robustness and Sensitivity Checks

Convict Labor and Labor Market Outcomes: IV Specifications

In the result section I have estimated adverse effects that convict labor caused on labor market outcomes, and its positive effects on the number of registered patents and capital accumulation. Here, I will address other sources of endogeneity that can bias my results, provide sensitivity checks, to strengthening my identifying assumptions, and finally, propose alternative differences-in-differences identification strategy, that while using the same source of exogenous variation relies on a different set of assumptions.

First, I draw additional evidences for my wage results from the firm-level data spanning 1850-1880. I use state-level representative samples available at Jeremy Atack's site (Atack and Bateman (1999)).[96] I estimate the following empirical specification:

$$\ln\left(\text{Wage}\right)_{f,i,c,t} = \alpha_c + \beta_t + \xi_i + \gamma CL_{i,c,t} + \Pi \mathbb{X}_{f,i,c,t} + t\delta_s + \varepsilon_{f,i,c,t}. \tag{1.13}$$

Unit observation is the firm f in industry i in county c at decade t. Treatment

[96] As this data is a repeated cross-sections and is not representative by county, results based on these data should be interpreted only as suggestive correlations.

in this specification is the value of prison-made goods in industry i, in county c at time t. I use only more conservative discrete measure of convict labor, since the sample of firms is representative only on state-level, and spatial treatment may be biased.[97] I cluster standard errors on state level (in parentheses), and on state-industry level, as while convict labor is a state-level policy, the treatment is on the industry level (in square brackets).

Table 1.10: Convict Labor and Wages: Firm-Level Data

	I	II	III	IV
Dependent Variable:	ln of Wage in Manufacturing			
Convict Labor	-0.014**	-0.014**	-0.015**	-0.014**
	(0.006)	(0.007)	(0.006)	(0.007)
	[0.007]	[0.007]	[0.006]	[0.007]
County & Decade FE	X	X	X	X
Industry FE		X	X	X
Industry x State FE			X	X
State-specific trends				X
R squared	0.28	0.32	0.35	0.36
Observations	30,066	30,066	30,066	30,066

Both values of exposure to convict labor are normalized. All columns contain constant and log value of manufacturing output. Robust clustered by state standard errors in parentheses. Standard errors clustered by state and SIC two digit industry codes are in square brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

I present results in Table A1.10. In Column I present specification, with county and year fixed effects, and control for value of firm's manufacturing output. One percent increase in convict labor output decreases wages by 1.4%. This result is remarkable close to one in Table 1.6. I add industry dummies in Column II, but the estimates does not change. In Column III, I add industry-state interaction, in case some state-level laws affected different industries differential. The magnitude of the coefficient increased slightly, and it remained significant. Finally, in in Column IV, I also add state-specific linear trends, but they don't affect the magnitude of the estimate. While this table does not provide causal linkage between

[97] I suspect, that majority of the firms in the sample are located in big cities, and I will overestimate the effect of convict labor.

convict labor and wages it provides another evidence, that the relationship I have established in previous table is correct.

Second, I perform sensitivity checks for my panel-specification results. In case spatial correlation is still an issue, in addition to the discrete exposure to convict labor, I also use discrete instrument, that treats only those counties that had pre-convict labor prisons and discrete measure of convict labor output in Table A1.11. Such approach only assumes that only counties that used to have a prison will be more likely to employ prisoners, but not the location around county with prison. Such instrument should yield more powerful first stage, while identification in such case comes only from a small number of "treated" counties: about 6% of the whole sample. As expected, the result of the first stage is extremely strong with the F-statistics of the excluded instrument above 100, that is the consequence of the fact, that most counties that had prisons before convict labor was allowed did not reallocate them but started to employ inmates there.

Table 1.11: Convict Labor and Economic Outcomes: IV with Discrete Instrument

	I	II	III	IV
Outcome:	ln of Wage in Manufacturing	Capital-Labor Ratio (K/L)	Number of Patents in Competing Industries	Number of Patents in Noncompeting Industries
Convict Labor (Discrete)	-0.21***	42.87**	0.111**	0.027
	(0.055)	(21.34)	(0.054)	(0.016)
R-squared	0.76	0.26	0.62	0.61
Kleibergen-Paap F stat	11.93	8.784	24.67	24.67
Instrument's coefficient	0.407***	0.539**	0.379***	0.379***
	(0.126)	(0.207)	(0.117)	(0.117)
Observations	17,397	7,631	16,366	16,366

All columns contain second-stage results. OLS regressions are the same as in corresponding specifications with discrete measure of exposure to convict labor. All columns contain constant, county, and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, employment share in manufacturing, employment share in agriculture, ln of value of farm products, number of dwellings, number of slaves, and state-specific linear trends. Columns with second-stage include the first-stage coefficient of instrument on the explanatory variable. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Also, as most of the old prisons were concentrated in Northern states, I propose an alternative instrument. I use actual capacities of existing prisons instead of those built in pre-convict labor era. This instrument increases the power of the first stage and also allow us to use variation in prison capacity from Southern and Western states. However, it also assumes that the location and size of prisons built after the adoption of convict labor laws are not influenced by the consideration of the use of convict labor. I report the resulting estimates in Table A1.12. While the first stage becomes stronger than in baseline results, the resulting coefficients of the 2SLS are not statistically different. It suggests, that my results can be considered valid beyond the Northern States and that the effect of convict labor is correctly estimated.

Table 1.12: Convict Labor and Economic Outcomes: IV with Actual Prison Capacities

Panel A	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent Variable: ln Wages in Manufacturing									
Sample	Full Sample				w/o South		w/o West		w/o North	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	-0.06***	-0.09***			-0.07***		-0.09***		-0.06	
	(0.008)	(0.018)			(0.019)		(0.019)		(0.047)	
Convict Labor (Discrete)			-0.02***	-0.13***		-0.10***		-0.13***		-0.16
			(0.005)	(0.034)		(0.032)		(0.036)		(0.130)
R-squared	0.841	0.814	0.838	0.775	0.828	0.796	0.819	0.775	0.813	0.811
Kleibergen-Paap F-stat		181.6		25.66	273.2	35.78	178.9	25.56	5.957	4.711
Instrument's coefficient		0.86***		0.60***	0.92***	0.70***	0.91***	0.58***	0.50**	0.18*
		(0.069)		(0.127)	(0.060)	(0.126)	(0.091)	(0.119)	(0.222)	(0.090)
Observations	15,366	13,593	15,366	13,593	7,669	7,669	11,691	11,691	7,826	7,826

Panel B	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent Variable: Number of Patents in Competing Industries per 10,000 people									
Sample	Full Sample				w/o South		w/o West		w/o North	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	0.01***	0.01***			0.01***		0.01***		-0.00	
	(0.002)	(0.004)			(0.003)		(0.004)		(0.000)	
Convict Labor (Discrete)			0.00**	0.02***		0.02***		0.02***		-0.00
			(0.000)	(0.006)		(0.005)		(0.006)		(0.001)
R-squared	0.869	0.406	0.855	-0.339	0.969	0.950	0.964	0.943	0.982	0.982
Kleibergen-Paap F-stat		116.5		35.83	208.8	41.24	132.7	28.39	5.241	4.972
Instrument's coefficient		0.86***		0.60***	0.91***	0.72***	0.89***	0.61***	0.51**	0.19*
		(0.069)		(0.127)	(0.064)	(0.130)	(0.079)	(0.139)	(0.232)	(0.094)
Observations	16,371	14,615	16,371	14,615	9,039	9,039	12,484	12,484	7,707	7,707

All columns contain constant, county, and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, employment share in manufacturing, employment share in agriculture, ln of value of farm products, number of dwellings, number of slaves, and state-specific linear trends. Columns with second-stage include the first-stage coefficient of instrument on the explanatory variable. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Also, I explore the assumption of the continuous effect of convict labor: in particular that it experiences iceberg costs and fade over distance. Following standard gravity models, I weight prison output by the inverse distance to a penitentiary. If I use “slower” fading such as a log or square root of distance, my first stage F-statistics falls but remains around 10. However, if I use “faster iceberg” costs ((Distance^2 and Distance + Distance^2), my results approach those in discrete setting. Worse predictive power in “slower” specifications suggest, that the effect of prison competition while expanse beyond the county is still localized.

IV identification is based on the same plausible exogenous variation of the pre-convict labor era prisons. Even if the instrument demonstrates strong relationship with exposure to convict labor, it is impossible to directly test the assumption of exclusion restrictions. To alleviate this concern, I follow the approach proposed in Conley et al. (2012).

I relax the exogeneity assumptions of the instrument and examine the bounds we are able to place on the true effect of convict labor on the arrests of Black. The idea behind the method is simple: if in addition to exogenous and endogenous variables I add instrument (exposure to pre-convict labor era prisons) its coefficient (β) required to be equal to zero according to standard IV estimation. However, by relaxing the constraint we can find the bounds for the IV estimate of convict labor (γ_0). As an example, let us consider specification with wage in manufacturing. If one expects instruments to have direct or indirect positive effect on the wages ($\beta > 0$) I will underestimate the true effect of the convict labor. This gives the maximum prior for β. More challenging is to determine the minimum prior of β. Thus I assume, that the maximum direct effect of instrument will be not bigger than the size of the biggest effect of one of the control covariates. In the wage regression, such covariate is share of urban population. Similarly, I found the minimum and maximum priers for all outcome variables. The bounds on the strength of γ_0s do not contain zero (at 95% confidence level). Therefore, even allowing for imperfect exogeneity, the causal effect of convict labor on outcomes of interest is confirmed.

Convict Labor and Labor Market Outcomes: Difference-in-Differences Specification

Finally, an instead of relying on the IV identifying assumption, I use differences-in-differences (DD) framework that utilize the same source of cross-sectional and state-time variation:

$$y_{c,t} = \alpha_c + \beta_t + \gamma_0 \text{Old Prisons}_{c,t} + \Gamma \mathbb{X}_{c,t} + t\delta_s + \varepsilon_{c,t}, \tag{1.14}$$

where as a dependent variable $y_{c,}$ is an outcome variable in county c at year t, $t \in \{1860; 1950\}$. Variable Old Prisons$_{c,t}$ measures exposure of each county by the prisons around it: Old Prisons$_{c,t}$ $= \sum_{p \in P} \left(\frac{\ln\left(\text{Old prison capacity}_p\right)}{\text{Distance}_{c,p}} \right)$ if convict labor is allowed at year t, and zero otherwise; X_i is the matrix of county-level controls described below in a greater details; α_c, and β_t are county and decade fixed effects; and $t\delta_s$ is a state- specific linear trends. As the convict labor is state policy, I cluster errors on the state level as well.

Under the assumption that wage in manufacturing used to exhibit similar trends in all counties, we can think of the parameter γ_0 as measuring the averaged weighted effect of convict labor. It is important to note, that coefficient $\hat{\gamma}_0$ should be interpreted as reduced form result of my IV specification. Thus specification 1.14 estimate the effect of located close to prisons existed before convict labor was allowed, and not necessary the actual effect of prisons that employed convicts.

I present results of the specification 1.14 in the Panel A of the Table A1.13. I start with state-specific trends that aim to absorb state-level changes in legislature and build-up of state correctional systems. Column I reports results of regression with the annual wage in manufacturing. One standard deviation in exposure to pre-convict labor era prison capacities decreases wage by 2.9% in manufacturing. Consistently with the story, I found an adverse effect of convict labor on labor share and positive impact on the capital-labor ratio Columns II and III show that a standard deviation in exposure to pre-convict labor era prison capacities decreases labor share by 0.011 percentage-points and increases the capital-labor ratio by 0.335 percentage-points. Column IV also supports technology adoption story, as I found a positive effect of convict labor on the number of patents: one standard deviation in an increase to exposure of convict labor increases the number of patents registered in that county in the next ten years by 154.7. At the same time, I find no effect on the share of employed in manufacturing (Column V).

The fundamental assumption of the difference-in-difference approach is that the potential outcomes for the control group (counties without prison before imposition of convict labor) and the treatment group (with prisons) are the same. This assumption cannot be tested directly. However one of the ways to explore its plausibility is to look at trends in the dependent variable before the treatment within the treatment group and the control group. If those trends are parallel, and the changes in trends coincide with the time of the treatment, it should increase our confidence, which the usual challenges to causal identification might be alleviated in this case.

To alleviate pre-trend concerns, I report differential pre-trends under each Column.[98] Wage in manufacturing exhibited a positive pre-trend in affected regions, thus only making it harder to find the adverse effect of convict labor. Labor share did not have a differential pre-trend. Capital-labor ratio and the number of patents both have positive differential pre-trends, meaning that affected regions

[98] These trends remain if I control for urban, population or manufacturing outcome trends, suggesting that they exist in addition to all other processes happening during that time.

Table 1.13: Effects of Convict Labor: Difference-in-Difference

Panel A (Continuous)	I	II	III	IV	V	VI	VII	VIII	IX	X
Outcome:	ln of Wage in Manufacturing	Labor Share (wL/pY)	Capital-Labor Ratio (K/wL)	Number of Patents	Share of Employed in Manufacturing	ln of Wage in Manufacturing	Labor Share (wL/pY)	Capital-Labor Ratio (K/wL)	Number of Patents	Share of Employed in Manufacturing
ln of Pre Convict Labor Era	-0.028**	-0.011***	0.334***	166.2**	-0.001	-0.065***	-0.010**	0.636***	91.5**	-0.004
Prison Capacities x $(\text{Distance})^{-1}$	(0.012)	(0.003)	(0.105)	(66.749)	(0.002)	(0.021)	(0.004)	(0.145)	(35.330)	(0.005)
R-squared	0.484	0.545	0.437	0.779	0.738	0.581	0.703	0.609	0.948	0.849
Controls			State specific linear trends					FIPS specific linear trends		
Observations	18,504	14,249	9,214	24,272	16,941	18,504	14,249	9,214	24,272	16,941
Differencial pre-trend	0.218***	-0.003	76.861**	471.5***	0.004***	0.218***	-0.003	76.861**	471.5***	0.004***
	(0.037)	(0.005)	(32.721)	(88.596)	(0.001)	(0.037)	(0.005)	(32.721)	(88.596)	(0.001)
Panel B (Trimmed Sample)	I	II	III	IV	V	VI	VII	VIII	IX	X
Outcome:	ln of Wage in Manufacturing	Labor Share (wL/pY)	Capital-Labor Ratio (K/wL)	Number of Patents	Share of Employed in Manufacturing	ln of Wage in Manufacturing	Labor Share (wL/pY)	Capital-Labor Ratio (K/wL)	Number of Patents	Share of Employed in Manufacturing
ln of Pre Convict Labor Era	-0.012	-0.003***	0.107***	128.4**	0.000	-0.069***	-0.008**	0.573***	95.9**	-0.004
Prison Capacities	(0.008)	(0.001)	(0.037)	(47.894)	(0.001)	(0.018)	(0.004)	(0.130)	(36.877)	(0.005)
R-squared	0.490	0.550	0.438	0.780	0.749	0.587	0.706	0.601	0.948	0.854
Controls			State specific linear trends					FIPS specific linear trends		
Observations	16,108	12,340	7,942	21,068	14,679	16,108	12,340	7,942	21,068	14,679
Differencial pre-trend	0.157***	-0.002	50.878***	545.3***	-0.000	0.157***	-0.002	50.878***	545.3***	-0.000
	(0.028)	(0.004)	(17.025)	(98.784)	(0.001)	(0.028)	(0.004)	(17.025)	(98.784)	(0.001)

All columns contain constant, county and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

already had higher growth rates than less affected by convict labor. The share of manufacturing also exhibits positive differential pre-trend, however, we would expect adverse effect on labor share by convict labor, thus it also only makes it harder to find adverse effect.

To treat pre-trends for capital-labor ratio and patents, and to get more precise estimates of other outcome variables that have significant pre-trends, I introduce county-specific trends in Columns VI-X. The point-estimate for the wage increase in magnitude, suggesting, that positive pre-trend indeed affected results. One standard deviation in exposure to pre-convict labor era prison facilities decreases wages in manufacturing by 5.3%. As expected, the coefficient for labor share did not change (while R^2 jumped up by 15.8%). In Column VIII we can see that the coefficient for capital-labor ratio also increased despite positive differential pre-trend that can be attributed to post-treatment change in differential trend in more affected locations.[99] At the same time, the point-estimate for the number of patents decreases 91.9 patents over next decade upon the inclusion of unit-specific linear trends (Column IX). Finally, even if the coefficient of the share of manufacturing employment increases in magnitude in Column X it remains insignificant.

In in the Panel B I use a discrete treatment: exposure of each county by the prisons around it: $\text{Old Prisons}_c = \ln(\text{old prison capacity}_c + 1)$ if convict labor is allowed at year t, and zero otherwise. As there are too few treated counties (only 102 pre-convict labor era counties or 5% of all counties had at least one prison) and those counties are very different from non-treated in terms of observable covariates, similar to Kline and Moretti (2014) I trim the sample in order to get rid of "worse" possible controls. Similarly, I look at state-specific and county-specific trends specifications. All results remain, and coefficients demonstrate very

[99] As those places were more developed before the industrialization, these trend differences can be consistent with simple models of regional convergence (e.g. Barro et al. (1991)).

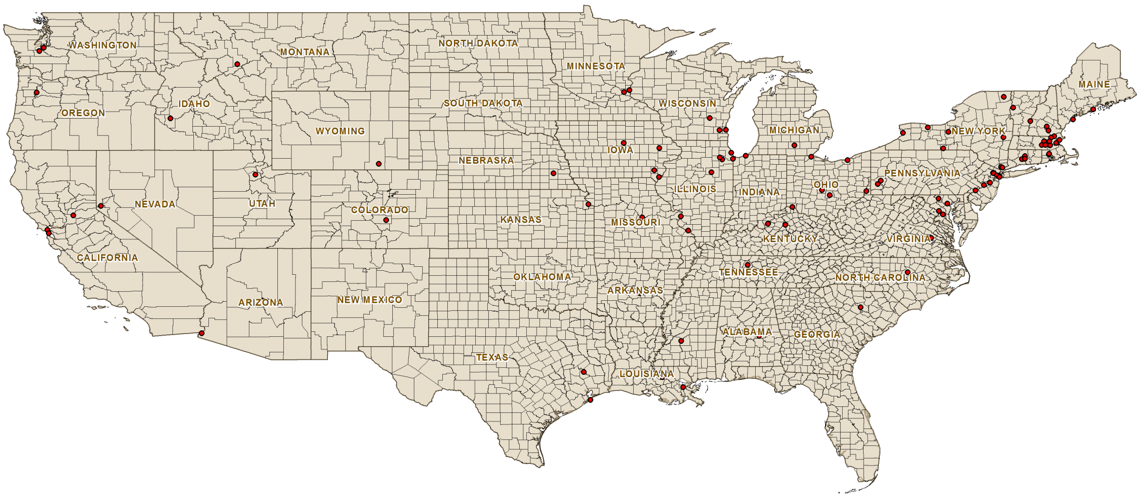
high robustness in their magnitudes.

I address DD identifying assumptions by showing the absence of pre-trend, and directly control for the possible local wage trends by inclusion county-specific trends.

In the Appendix, instead of static specification, I use event-study approach to address negative weighting in canonical DD of γ_0-s and directly test for pre-trend, using methods, proposed in Borusyak and Jaravel (2016). Besides, in Appendix following Abadie et al. (2010) methodology I use synthetic control DD (as in Jardim et al. (2017)) to estimate reduced form effect of pre-convict labor era prison facilities on economic outcomes. I also report results of specifications, using alternative measures of explanatory variables, and following Oster (2017) show that selection of unobservables is not an issue.

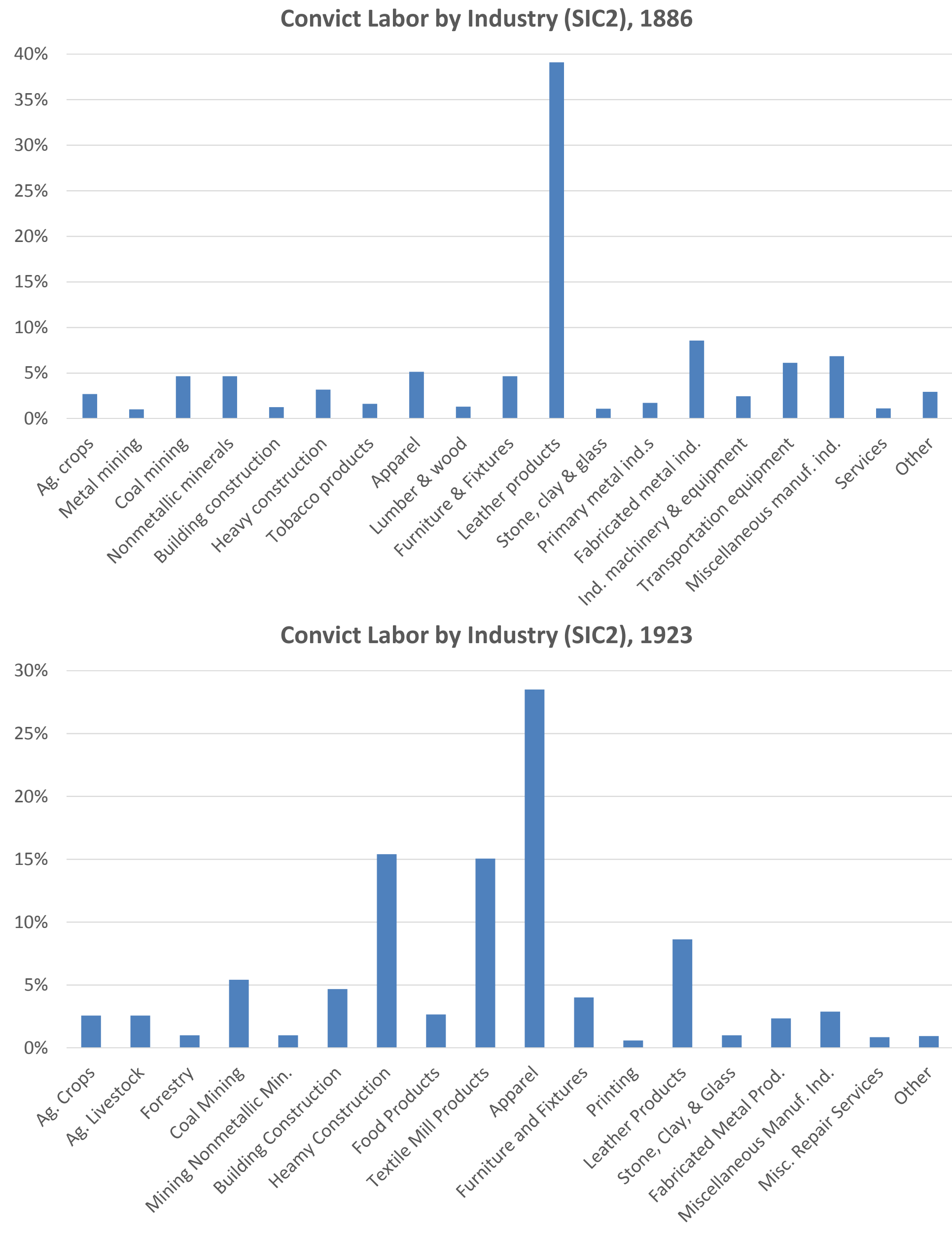
Figures

Figure 1.9: Prisons in 1870



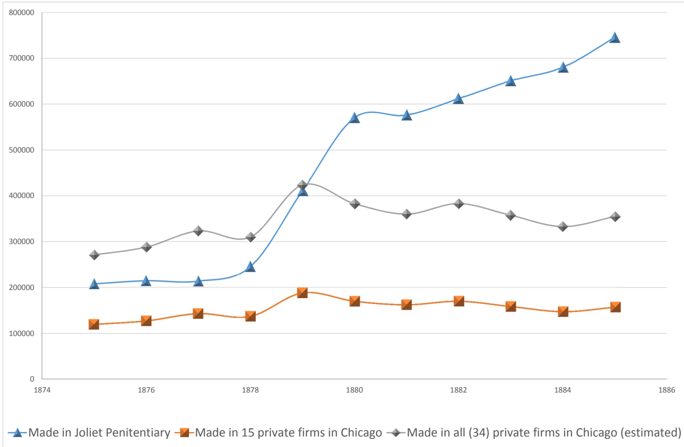
Red dots represent location of state prisons and penitentiaries that existed before their states adopted convict leasing, contract or piece-price systems of convict labor. Source: Locations are from North American Review (1866) and Department of Labor (1887), and coordinates are found using R.

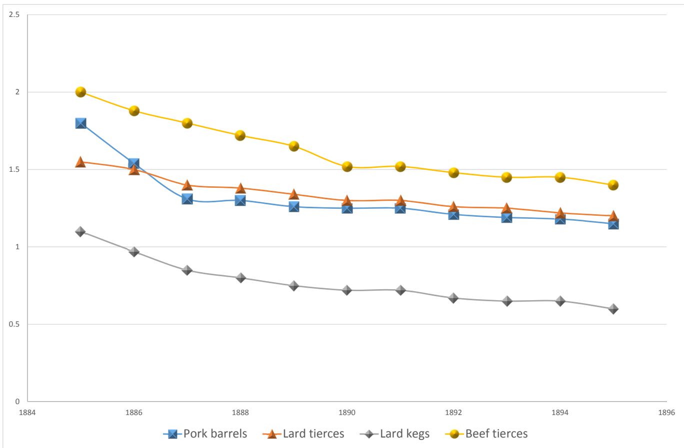
Figure 1.10: Convict Labor by SIC 2-digit code (1886, and 1923)



Source: Computed using the data from Department of Labor (1887).

Figure 1.11: The Case of Chicago's Coopers

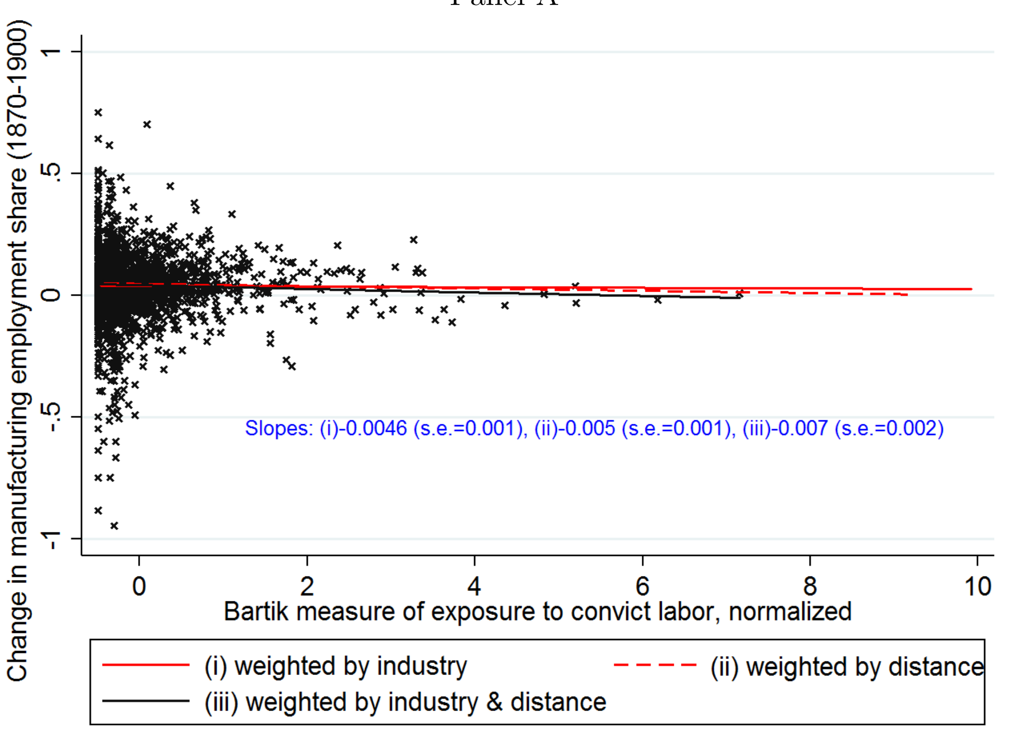




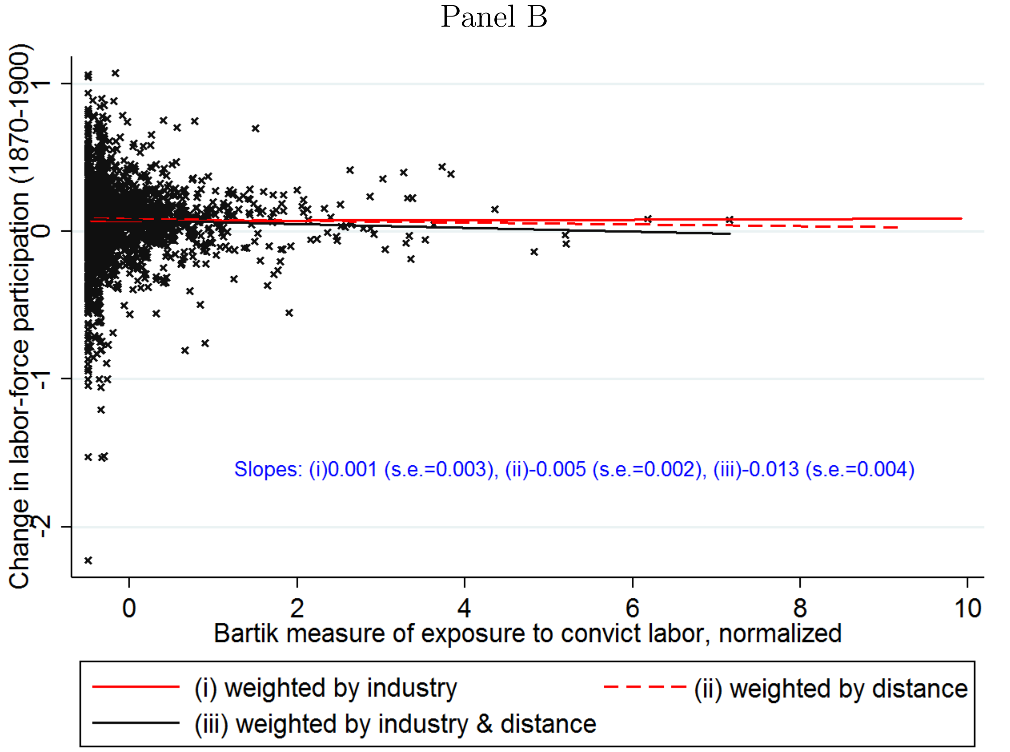
Annual wages in dollars of 1895. Source: Department of Labor (1900), pp. 386-40.

Figure 1.12: Convict Labor and Changes in Manufacturing Employment Share and Labor-Force Participation

Panel A



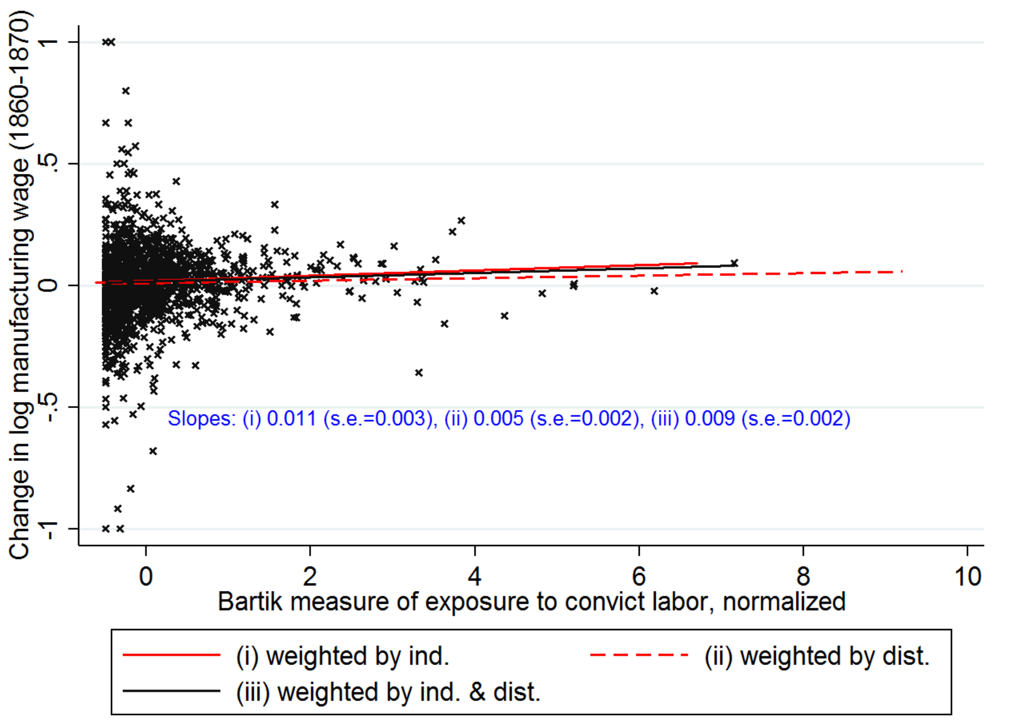
Panel B



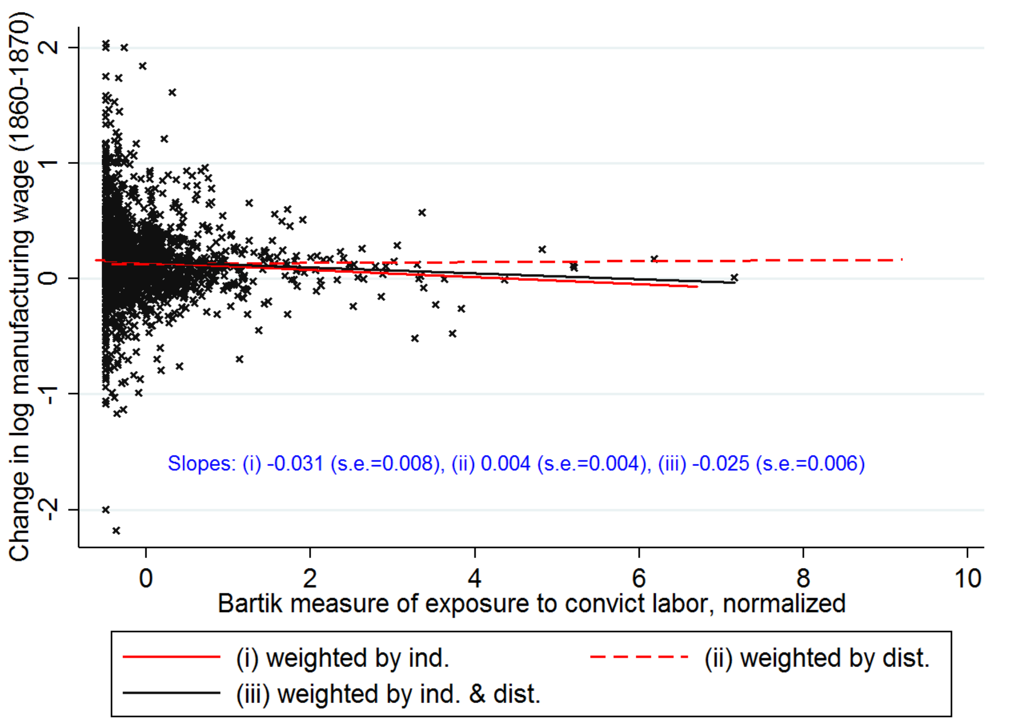
Each dot is a county. Source: U.S. Department of Labor, and Haines (2004).

Figure 1.13: Convict Labor and Changes in Manufacturing Employment Share and Labor-Force Participation: Placebo

Panel A

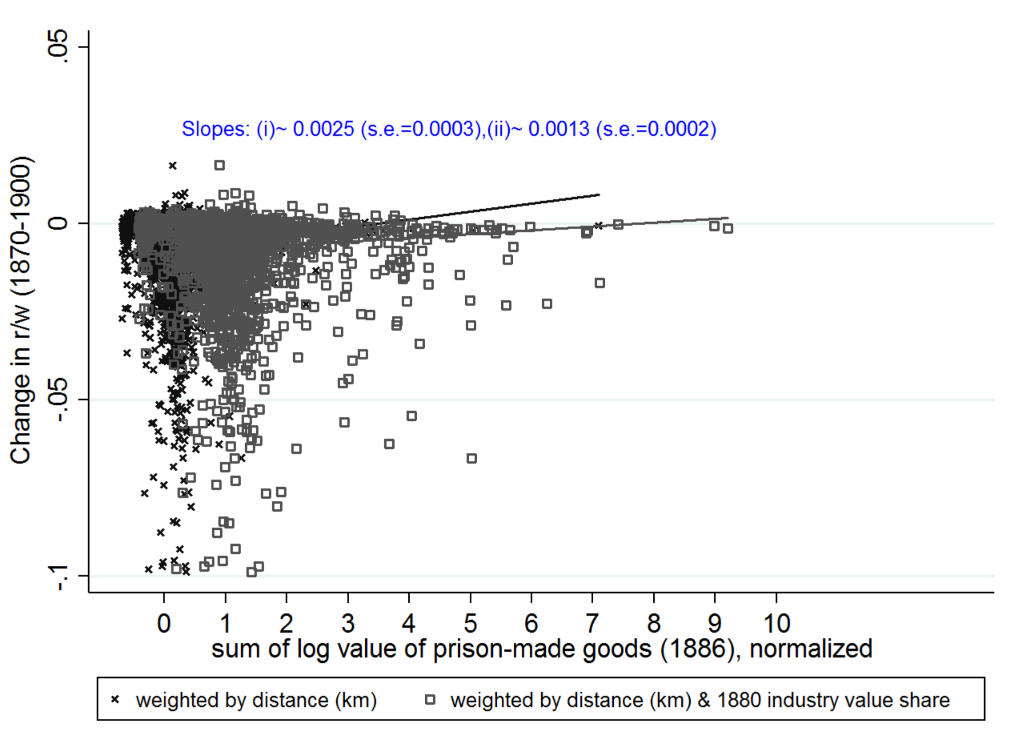


Panel B



Each dot is a county. The relationship between convict labor and changes in lagged changes in employment share in manufacturing is positive. At the same time the slope for labor-force participation is positive only for specification with weighting by distance, and is negative for specifications with industry weightings. In Section 1.5 I show that conditional on the set of my baseline controls the relationship is non-negative. Source: U.S. Department of Labor, and Haines (2004).

Figure 1.14: Convict Labor and Technology Adoption



Each dot is a county. Source U.S. Department of Labor, and Haines (2004).

Figure 1.15: The First Stage: Residual Plot

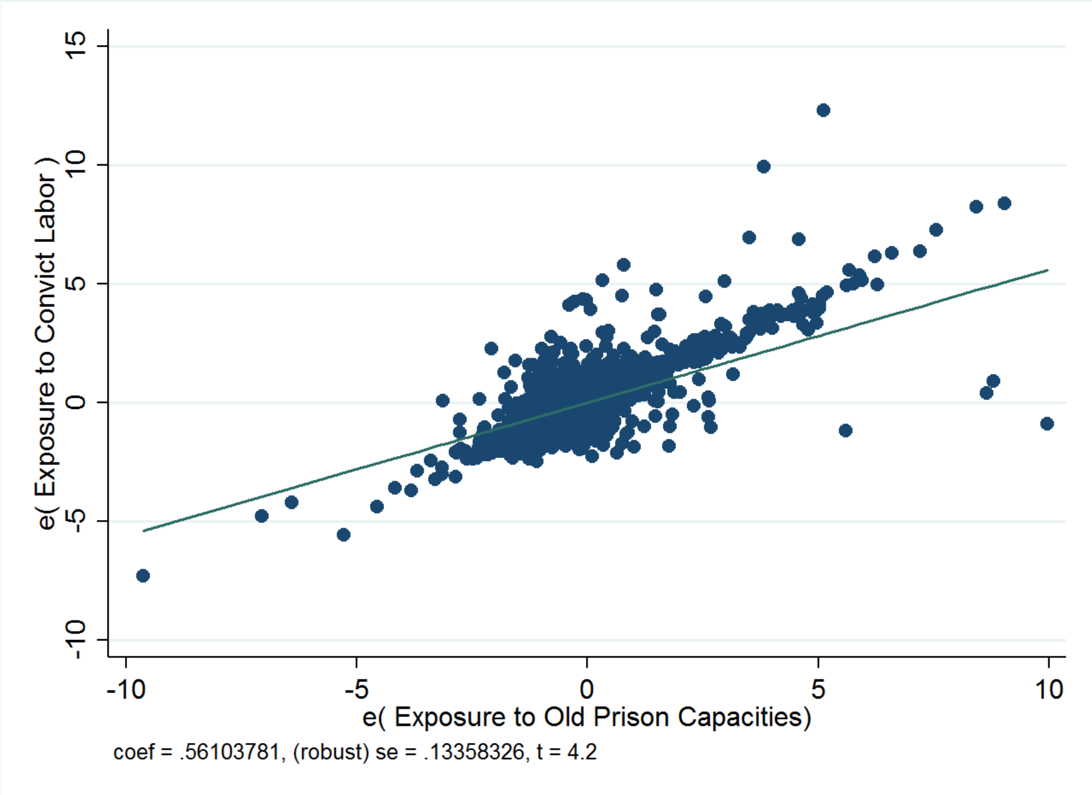
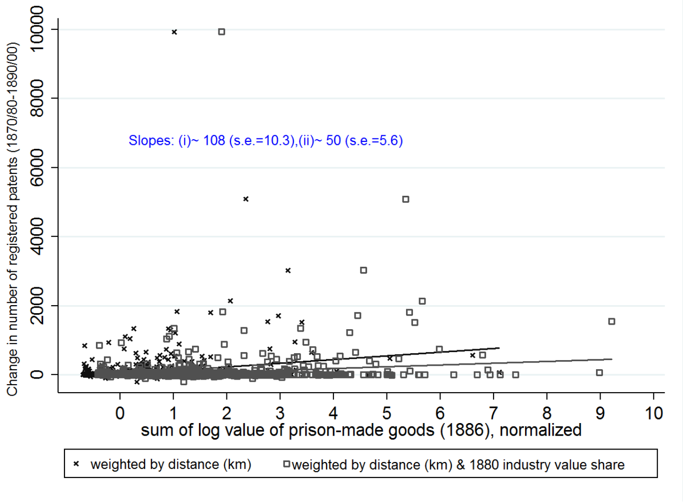


Figure 1.16: Convict Labor and Patenting



Each cross is a county. Source U.S. Department of Labor, and Haines (2004).

Figure 1.17: Technological Frontier in Other Industries

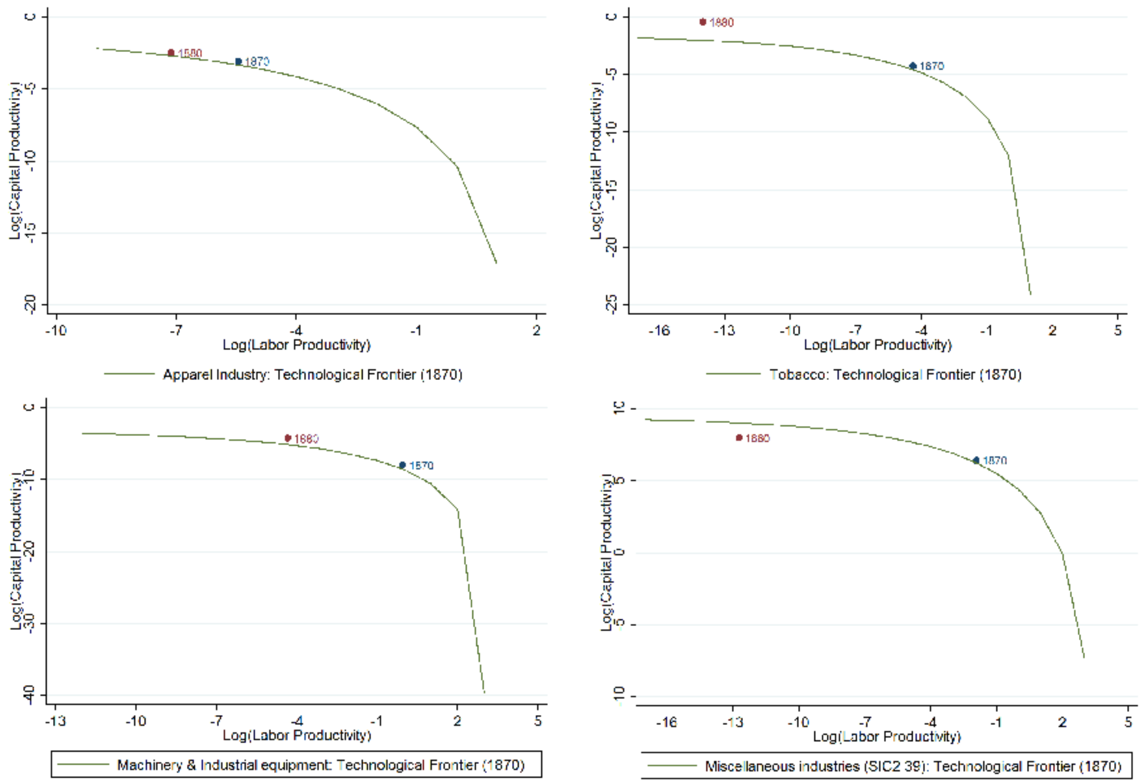


Figure 1.18: Technological Frontier in Quarrying and Timber Production

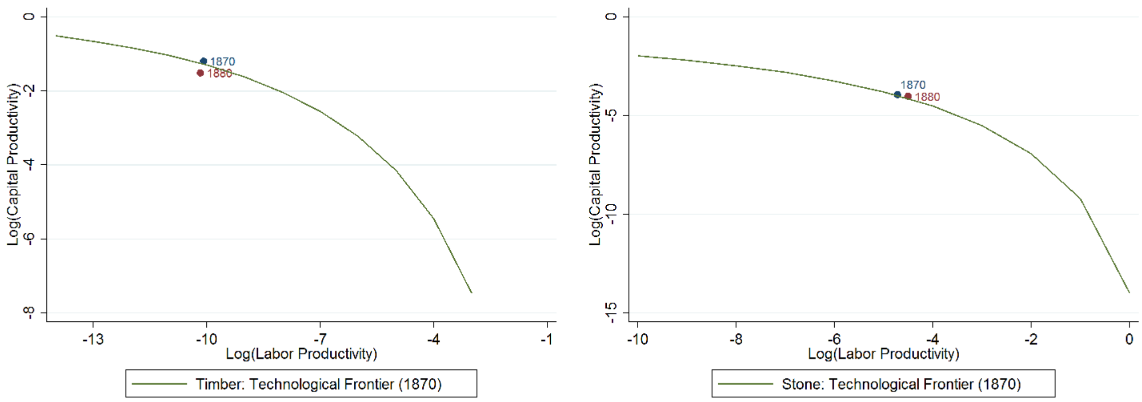
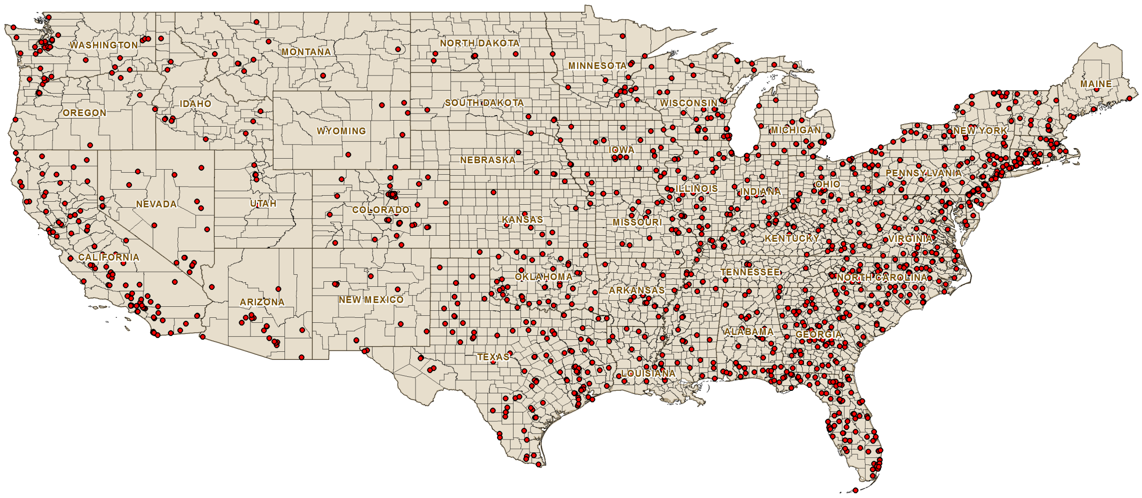


Table 1.14: Prisons in 2005



Red dots represent location of state prisons that employ prisoners at 2005. Source: Addresses of state correctional facilities are from the Census of State and Federal Adult Correctional Facilities (ICPSR 24642), and coordinates are found using R.

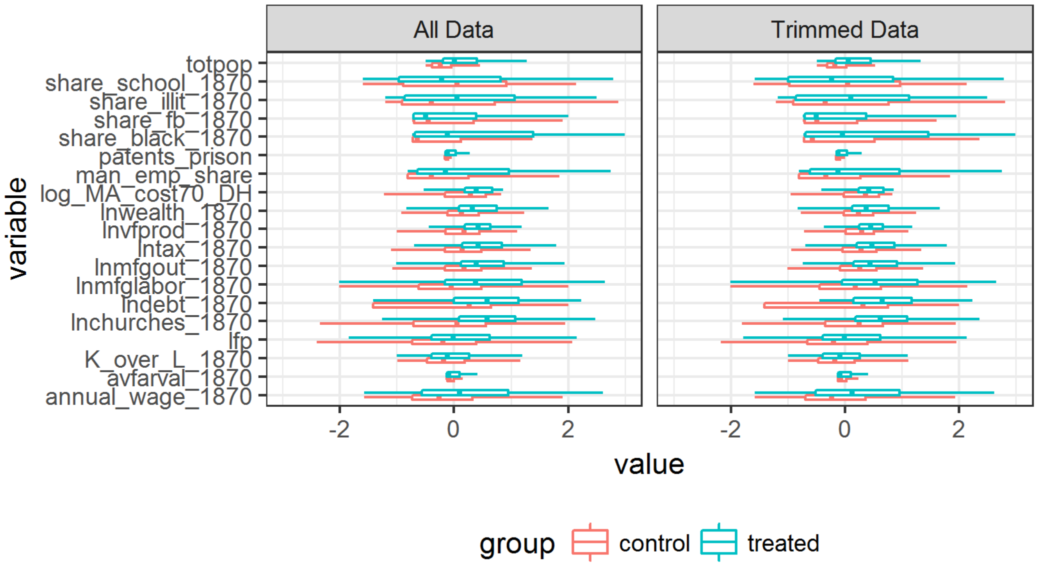
Tables

Table 1.15: Evolution of Convict Labor: Share of Total Value of Goods Produced

System	1886	1895	1905	1914	1923	1932	1940
Convict leasing	15	11.4	9	1.8	0	0	0
Contract	70.4	43	48.6	26.2	24	8	0
Piece-price	6.1	19.9	9.4	6.5	16.2	14	0.5
State-account			13.9	36.9	21.6	16.4	15.6
State-use	8.5	25.7	10.7	22.2	18.1	28.2	60.2
Public works and ways			8.4	6.4	20.1	33.4	23.7
	100	100	100	100	100	100	100

State-account, state-use and public works and ways systems were reported together as a public-account system before 1905. Source: Computed using the data from U.S. Department of Labor.

Table 1.16: Summary Statistics



The unit of observation is a county. The trimmed sample is obtained by dropping control counties which, based on their pre-convict labor era characteristics, have a predicted propensity score in the bottom 25 percent. All monetary values are in constant 1940 dollars. Data are from the 1870 and 1880 Census of Population and Housing, with the exception of farm value data, which are from the 1870 and 1880 Agricultural Census, market access data computed by Donaldson and Hornbeck (2016),and elevation data, which were collected by Fishback et al. (2007). Manufacturing wage is obtained by dividing the total annual wage bill in manufacturing by the estimated number of workers in the industry. Details on data construction and limitations are provided in the Appendix 1.8.

Table 1.17: First Stage: Convict Labor and Old Prisons

	Introduction of Convict Labor (1870-1886)	
	I	II
Outcome:	Convict Labor	
	Baseline Measure	Autor, Dorn, Hanson (2013)
Old Prison Capacities	0.561***	0.577***
	(0.134)	(0.138)
R-squared	0.495	0.493
Observations	2,356	2,356

Table 1.18: Reduced Form: Local Labor Market Outcomes

	Introduction of Convict Labor (1870-1886)		
	I	II	III
Outcome:	Δ log Wage in Manufacturing	Δ Labor-Force Participation	Δ Employment Share in Manufacturing
Old Prison Capacities	-0.012***	-0.002**	-0.005**
	(0.003)	(0.001)	(0.002)
R-squared	0.200	0.031	0.109
Observations	1,954	2,122	2,226

	Introduction of Convict Labor (1870-1886)		
	I	II	III
Outcome:	Δ log Patents in Competing Industries	Δ log Patents in Noncompeting Industries	Δ Capital-Labor Ratio
Old Prison Capacities	0.027*	-0.036***	81.851***
	(0.015)	(0.011)	(14.046)
R-squared	0.242	0.167	0.276
Observations	2,356	2,356	2,230

Table 1.19: Subsample Analysis: Local Labor Marker Outcomes

	Dependent Variable: Δ log Wage in Manufacturing				
	I	II	III	IV	V
Sample	w/o South	w/o Northeast	w/o Midwest	w/o Far West	w/o Great Plains
Convict Labor (Continuous)	-0.052***	-0.047*	-0.037***	-0.030***	-0.043***
	(0.015)	(0.024)	(0.011)	(0.010)	(0.011)
R-squared	0.272	0.194	0.209	0.249	0.232
Kleibergen-Paap F-stat	8.72	3.53	12.70	11.03	12.37
Observations	1,175	1,715	1,305	1,830	1,791

	Dependent Variable: Δ Labor-Force Participation				
	I	II	III	IV	V
Sample	w/o South	w/o Northeast	w/o Midwest	w/o Far West	w/o Great Plains
Convict Labor (Continuous)	-0.006	-0.004	-0.007	-0.006	-0.007*
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
R-squared	0.074	0.038	0.041	0.025	0.033
Kleibergen-Paap F-stat	10.51	3.84	16.04	12.99	14.99
Observations	1,243	1,885	1,458	1,985	1,917

	Dependent Variable: Δ Employment Share in Manufacturing				
	I	II	III	IV	V
Sample	w/o South	w/o Northeast	w/o Midwest	w/o Far West	w/o Great Plains
Convict Labor (Continuous)	-0.016**	0.001	-0.016*	-0.018**	-0.016**
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)
R-squared	0.090	0.104	0.126	0.110	0.111
Kleibergen-Paap F-stat	10.19	3.45	13.53	11.92	13.11
Observations	1,346	1,987	1,529	2,080	1,962

Table 1.20: Robustness Checks: Introduction of Convict Labor, 1870-1886

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII
	Dependent Variable: Δ log Wage in Manufacturing												
	Robustness Checks									Placebo			
	Only Industry Weights	w/o Industry Weights				w Industry Weights				w/o Industry Weights			
	OLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict Labor (Cont.)	-0.009*	-0.022***	-0.030***										
	(0.005)	(0.006)	(0.005)										
Convict Labor (Disc.)				-0.007***	-0.010***	-0.003**	-0.011***	-0.003	-0.016**				
				(0.001)	(0.002)	(0.001)	(0.004)	(0.002)	(0.007)				
Convict Labor in Farming (Continuous)										-0.001	-0.126		
										(0.001)	(0.370)		
Convict Labor in Farming (Discrete)												-0.002	-0.022
												(0.001)	(0.042)
Kleibergen-Paap F-stat			54.28		25.17		20.71		13.93		0.11		0.23
Sample	Full (trimmed for Discrete)					Counties with Prisons				Full (trimmed for Discrete)			
Observations	1,954	1,954	1,954	1,474	1,474	311	311	311	311	1,954	1,954	1,474	1,474

Table 1.21: Discrete Measure of Convict Labor: Introduction of Convict Labor, 1870-1886

Panel A	Introduction of Convict Labor (1870-1886)					
	I	II	III	IV	V	VI
Outcome (1880-1900):	Δ log Wage in Manufacturing		Δ Labor-Force Participation		Δ Employment Share in Manufacturing	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict Labor (Discrete)	-0.007***	-0.013***	-0.002**	-0.002*	-0.005***	-0.006***
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)
R-squared	0.256	0.250	0.033	0.033	0.118	0.116
Kleibergen-Paap F-stat		9.80		9.69		8.33
Observations	1,767	1,767	1,897	1,897	1,904	1,904

Panel B	Introduction of Convict Labor (1870-1886): Placebo					
	I	II	III	IV	V	VI
Outcome (1860-1870):	Δ log Wage in Manufacturing		Δ Labor-Force Participation		Δ Employment Share in Manufacturing	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict Labor (Discrete)	0.055***	0.138***	0.005**	0.002	0.007*	0.022***
	(0.014)	(0.035)	(0.002)	(0.006)	(0.003)	(0.008)
R-squared	0.391	0.355	0.557	0.556	0.335	0.309
Kleibergen-Paap F-stat		9.80		9.69		8.33
Observations	1,596	1,596	1,784	1,784	1,800	1,800

Table 1.22: Convict Labor and Wages: Specifications weighted by Industry Labor Share of 1880

	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent Variable: ln of Wage in Manufacturing									
Sample	Full Sample				w/o South		w/o West		w/o North	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	-0.06***	-0.20***			-0.18***		-0.20***		-0.05**	
	(0.008)	(0.040)			(0.039)		(0.042)		(0.022)	
Convict Labor (Discrete)			-0.02***	-0.24***		-0.19**		-0.24***		-0.14**
			(0.005)	(0.088)		(0.076)		(0.088)		(0.055)
R-squared	0.841	0.774	0.838	0.601	0.794	0.632	0.776	0.589	0.796	0.794
Kleibergen-Paap F-stat		17.84		7.203	15.06	6.075	16.49	7.007	86.85	17.63
Instrument's coefficient		0.44***		0.43***	0.41***	0.48***	0.45***	0.44***	0.59***	0.17***
		(0.106)		(0.130)	(0.114)	(0.158)	(0.110)	(0.134)	(0.086)	(0.026)
# States	41				29		30		22	
Observations	15,366	15,364	15,366	15,364	8,685	8,685	13,180	13,180	8,863	8,863

Both values of exposure to convict labor are normalized. All columns contain constant, county, and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, employment share in manufacturing, employment share in agriculture, ln of value of farm products, number of dwellings, number of slaves, and state-specific linear trends. Columns with second-stage include the first-stage coefficient of instrument on the explanatory variable. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.23: Matching

	Dependent Variable: Δ log Wage in Manufacturing			
	I	II	III	IV
Matching	Nearest Neighbor	Radius	Kernel-based	Stratification
ATT	-0.043	-0.244	-0.061	-0.047
Bootstrapped standard errors	(0.023)	(0.026)	(0.019)	(0.019)
t-statistics	-1.89	-9.4	-3.18	-2.4
# treated	349	35	349	349
# controls	222	1623	1688	1688

	Dependent Variable: Δ Labor-Force Participation			
	I	II	III	IV
Matching	Nearest Neighbor	Radius	Kernel-based	Stratification
ATT	-0.010	-0.011	-0.012	-0.011
Bootstrapped standard errors	(0.010)	(0.008)	(0.009)	(0.008)
t-statistics	-1.01	-1.39	-1.32	-1.43
# treated	349	138	349	349
# controls	228	1674	1688	1688

	Dependent Variable: Δ Emp. Share in Manufacturing			
	I	II	III	IV
Matching	Nearest Neighbor	Radius	Kernel-based	Stratification
ATT	-0.000	0.012	-0.011	-0.009
Bootstrapped standard errors	(0.009)	(0.005)	(0.006)	(0.006)
t-statistics	-0.009	-2.30	-1.64	-1.28
# treated	349	280	349	349
# controls	230	1685	1688	1688

Table 1.24: Convict Labor and Labor Market Outcomes: Introduction of Convict Labor (Autor et al. (2013) Measure)

Panel A	Introduction of Convict Labor (1870-1886)					
	I	II	III	IV	V	VI
Outcome (1880-1900):	Δ log Wage in		Δ Labor-Force		Δ Employment Share	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict Labor (Continuous)	-0.011**	-0.023***	-0.003***	-0.004*	-0.006***	-0.011**
	(0.004)	(0.009)	(0.001)	(0.002)	(0.002)	(0.004)
R-squared	0.198	0.189	0.033	0.032	0.113	0.105
Kleibergen-Paap F-stat		13.21		16.18		13.54
Observations	1,954	1,954	2,122	2,122	2,226	2,226
Panel B	**Introduction of Convict Labor (1870-1886): Placebo**					
	I	II	III	IV	V	VI
Outcome (1860-1870):	Δ log Wage in		Δ Labor-Force		Δ Employment Share	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict Labor (Continuous)	0.078***	0.148***	0.001	0.000	0.014***	0.033***
	(0.014)	(0.025)	(0.003)	(0.004)	(0.004)	(0.007)
R-squared	0.309	0.257	0.521	0.521	0.218	0.146
Kleibergen-Paap F-stat		9.58		10.46		9.7
Observations	1,709	1,709	1,929	1,929	2,034	2,034

Exposure to convict labor is normalized. All columns contain a constant. The following variables are used as controls (in changes): ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, ln of value of farm products, log of number of slaves in 1860 (level), and log of market access (the change and the base level of 1870). All columns have corresponding lagged outcome variable (level) as a control. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.25: Convict Labor and Capital-Labor Ratio

	Introduction of Convict Labor (1870-1886)			
	I	II	III	IV
Outcome:	Δ Capital-Labor Ratio			
	OLS	2SLS	OLS	2SLS
	(1880-1900)		Placebo (1860-1870)	
Convict Labor	50.4***	164.1***	10.7	38.048
	(16.2)	(43.6)	(7.7)	(26.1)
R-squared	0.255	0.224	0.318	0.316
Kleibergen-Paap F-stat		16.07		10.61
Observations	2,230	2,230	1,709	1,709

Table 1.26: Convict Labor and Labor Market Outcomes: Introduction of Convict Labor (Autor et al. (2013) Measure)

	Introduction of Convict Labor (1870-1886)							
	I	II	III	IV	V	VI	VII	VIII
Outcome (1880-1900):	Δ log Patents in Competing Industries		Δ log Patents in Noncompeting		Δ Capital-Labor Ratio		Δ log Patents in Competing Industries	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
							Placebo (1860-1870)	
Convict Labor	0.028	0.069*	-0.005	-0.092*	49.299***	143.325***	-0.097***	-0.221***
	(0.020)	(0.042)	(0.014)	(0.052)	(15.460)	(38.655)	(0.022)	(0.067)
R-squared	0.242	0.234	0.162	0.130	0.255	0.217	0.106	0.039
Kleibergen-Paap F-stat		6.18		6.18		17.06		5.65
Observations	2,356	2,356	2,356	2,356	2,230	2,230	2,034	2,034

Exposure to convict labor is normalized. All columns contain a constant. The following variables are used as controls (in changes): ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, ln of value of farm products, log of number of slaves in 1860 (level), and log of market access (the change and the base level of 1870). All columns have corresponding lagged outcome variable (level) as a control. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.27: Convict Labor and American Women by Geographical Regions

Panel A	Introduction of Convict Labor (1870-1886)											
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
Sample	w/o South		w/o West		w/o North		w/o South		w/o West		w/o North	
Outcome:	Δ Wage in Manufacturing						Δ Labor-Force Participation					
	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male
Convict Labor (Continuous)	-0.069**	-0.063***	-0.119***	-0.055***	-0.085	-0.039**	-0.012***	-0.016***	-0.009**	-0.008	-0.006	0.004
	(0.030)	(0.012)	(0.031)	(0.009)	(0.078)	(0.015)	(0.004)	(0.005)	(0.004)	(0.006)	(0.009)	(0.016)
Convict Labor (Discrete)	-0.025***	-0.018***	-0.041***	-0.014***	-0.064	-0.018	-0.004***	-0.004**	-0.003***	-0.004**	-0.008	-0.011
	(0.008)	(0.003)	(0.010)	(0.003)	(0.044)	(0.011)	(0.001)	(0.002)	(0.001)	(0.002)	(0.005)	(0.008)
Observations	1,426	1,355	1,879	1,818	1,407	1,281	1,382	1,398	1,869	1,872	1,373	1,386
Panel B	**AS and WH Public Contracts Acts (1936)**											
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
Sample	w/o South		w/o West		w/o North		w/o South		w/o West		w/o North	
Outcome:	Δ Wage in Manufacturing						Δ Labor-Force Participation					
	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male
Convict Labor (Continuous)	0.184***	0.144***	0.140**	0.141***	0.101**	0.011	0.029***	0.044***	0.019**	0.028***	0.015	0.027
	(0.040)	(0.040)	(0.057)	(0.043)	(0.045)	(0.068)	(0.006)	(0.012)	(0.007)	(0.010)	(0.013)	(0.016)
Convict Labor (Discrete)	0.021*	0.023***	0.030**	0.024**	-0.004	-0.013	0.003	0.006***	0.003*	0.005**	0.003	-0.005
	(0.011)	(0.007)	(0.012)	(0.009)	(0.044)	(0.050)	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)	(0.011)
Observations	1,503	1,502	1,910	1,910	1,487	1,486	1,503	1,502	1,910	1,910	1,487	1,486

Both values of exposure to convict labor are normalized. Coefficients in Panel B are multiplied by -1 to show the reduction in convict labor output. All columns contain OLS in first differences. All columns contain a constant. The following variables are used as controls (in changes): ln of total population, urban share, share of Black, share of women, share of foreign-born. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.28: Convict Labor and Wages: County-Specific Trends

	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent Variable: ln of Wage in Manufacturing									
Sample	Full Sample				w/o South		w/o West		w/o North	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	-0.07***	-0.20***			-0.18***		-0.18***		-0.04***	
	(0.012)	(0.039)			(0.043)		(0.043)		(0.012)	
Convict Labor (Discrete)			-0.02***	-0.24***		-0.18***		-0.12***		-0.07***
			(0.005)	(0.080)		(0.069)		(0.021)		(0.017)
R-squared	0.878	0.826	0.873	0.670	0.844	0.709	0.79	0.75	0.80	0.80
Kleibergen-Paap F-stat		14.90		7.162	11.42	6.055	11.42	20.6	105.8	41.9
Instrument's coefficient		0.38***		0.55***	0.38***	0.62***	0.24***	1.03***	0.36***	0.26***
		(0.086)		(0.155)	(0.101)	(0.171)	(0.054)	(0.148)	(0.080)	(0.085)
# States	41				29		30		22	
Observations	15,366	15,364	15,366	15,364	8,685	8,685	8,685	13,314	8,963	8,963

Both values of exposure to convict labor are normalized. All columns contain constant, county, and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, employment share in manufacturing, employment share in agriculture, ln of value of farm products, number of dwellings, number of slaves, and county-specific linear trends. Columns with second-stage include the first-stage coefficient of instrument on the explanatory variable. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.29: Convict Labor and Employment Outcomes

	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent Variable: Labor-Force Participation									
Sample	Full Sample				w/o South		w/o West		w/o North	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	0.01	-0.03			-0.00		-0.04		-0.01	
	(0.007)	(0.021)			(0.016)		(0.024)		(0.028)	
Convict Labor (Discrete)			-0.00*	-0.02*		-0.00		-0.02*		-0.04
			(0.001)	(0.011)		(0.007)		(0.011)		(0.044)
R-squared	0.59	0.37	0.61	0.38	0.36	0.38	0.37	0.38	0.42	0.44
Kleibergen-Paap F-stat		13.67		5.72	15.30	41.20	12.22	5.28	4.36	1.822
Instrument's coefficient		0.25***		0.43**	0.32***	0.72***	0.25***	0.44**	0.23**	0.12
		(0.074)		(0.197)	(0.088)	(0.122)	(0.078)	(0.203)	(0.098)	(0.158)
# States	41				29		30		22	
Observations	15,612	15,612	13,470	13,470	8,488	7,286	14,794	12,946	9,220	7,701
	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent Variable: Employment Share in Manufacturing									
Sample	Full Sample				w/o South		w/o West		w/o North	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	0.00	-0.00			-0.01		-0.00		0.00	
	(0.003)	(0.007)			(0.008)		(0.000)		(0.004)	
Convict Labor (Discrete)			-0.00	-0.00		-0.01		-0.01		0.01
			(0.002)	(0.009)		(0.011)		(0.009)		(0.013)
R-squared	0.838	0.747	0.838	0.747	0.806	0.799	1.000	0.758	0.595	0.595
Kleibergen-Paap F-stat		27.12		7.121	22.93	5.974	25.51	6.904	40.75	13.58
Instrument's coefficient		0.49***		0.35**	0.46***	0.37**	0.49***	0.36**	0.66***	0.21***
		(0.099)		(0.137)	(0.100)	(0.160)	(0.102)	(0.142)	(0.108)	(0.060)
# States	41				29		30		22	
Observations	19,293	19,293	19,293	19,293	10,788	10,788	16,444	16,444	11,354	11,354

Both values of exposure to convict labor are normalized. All columns contain constant, county, and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, ln of value of farm products, number of dwellings, number of slaves, and state-specific linear trends. Columns with second-stage results include the first-stage coefficient of instrument on the explanatory variable. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.30: Convict Labor and Wages in Manufacturing: Only with Industry Weighting

	I	II	III	IV
	Dependent Variable: ln of Wage in Manufacturing			
Sample	Full Sample	w/o South	w/o West	w/o North
Convict Labor (Continuous)	-0.03***	-0.02*	-0.03***	-0.02***
only Industry Weights	(0.009)	(0.011)	(0.010)	(0.007)
R-squared	0.838	0.843	0.842	0.835
# States	41	29	30	22
Observations	15,366	8,686	13,181	8,865

Exposure to convict labor is normalized. All columns contain constant, county, and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, employment share in manufacturing, employment share in agriculture, ln of value of farm products, number of dwellings, number of slaves, and state-specific linear trends. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.31: Convict Labor and Patenting: Placebo

	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent Variable: Number of Patents in Noncompeting Industries per 10,000 people									
Sample	Full Sample				w/o South		w/o West		w/o North	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	0.166	0.132			0.149		0.116		0.004	
	(0.133)	(0.109)			(0.092)		(0.074)		(0.005)	
Convict Labor (Discrete)			0.004	0.184		0.182		0.158		0.012
			(0.012)	(0.118)		(0.132)		(0.115)		(0.015)
R-squared	0.984	0.945	0.983	0.874	0.955	0.893	0.947	0.880	0.972	0.972
Kleibergen-Paap F-stat		16.52		6.753	13.91	5.759	15.53	6.598	40.11	14.71
Instrument's coefficient		0.46***		0.36**	0.43***	0.39**	0.45***	0.37**	0.60***	0.19***
		(0.114)		(0.144)	(0.121)	(0.167)	(0.117)	(0.148)	(0.096)	(0.059)
Observations	16,371	16,366	16,371	16,366	10,073	10,073	13,930	13,930	8,729	8,729

Both values of exposure to convict labor are normalized. All columns contain constant, county, and decade fixed effects. The following variables are used as controls: ln of total population, urban share, share of Black, share of women, share of foreign-born, ln of manufacturing output, employment share in manufacturing, employment share in agriculture, ln of value of farm products, number of dwellings, number of slaves, and state-specific linear trends. Columns with second-stage results include first-stage coefficient of the instrument on the explanatory variable. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

CHAPTER 2

U.S. Convict Labor System and Racial Discrimination

2.1 Introduction

One of the greatest example of the racial discrimination in the contemporary U.S. is the racial composition of the inmates in the U.S. penitentiary, as the ratio of Black-to-White and Hispanic-to-White prisoners is 5.6 and 1.8 to 1 correspondingly. These racial disproportions cannot be explain by the socioeconomic controls, and are often regarded as continuances of various forms of racial discrimination. However, this puzzle in the race-based discrimination in arrests may be a legacy of the old U.S. convict labor system.

Convict labor in the XIX-XX century United States affected incarceration rates directly and indirectly. The former took place as some systems of convict labor legally provided monetary incentives to police and judicial system (Wilson (1933), Blackmon (2009) and Litwack (2010)), or because some prisons bribed judges and policy to increase supply of prisoners (Gildemeister (1978)). The later affected incarceration through lower opportunity costs of crime due to distortion of wages of low-skilled workers who competed with prison-made goods (Poyker (2018c)). In this paper I show that countries more exposed to convict labor had higher incarceration rates. In addition, convict labor had differential effects on the incarceration of minorities. Jim Crow laws and overall racial discrimination made it easier to arrest African-Americans in the South, while hostility toward migrants made an easy target foreign-born Whites in the Northern States. Moreover, even after abolishing of the convict labor racial discrimination in policing may persist, thus creating contemporary disparities in arrest rates.

In this paper I explore effect of convict labor on incarceration rates, and the long-term persistence of the interracial hatred by using the new data-set of the U.S. convict labor camps in the end of XIX - begging of the century. I start my analysis by showing that the introduction of the institution of convict labor in the United States increased incarceration rates, especially among the Black and foreign-born White males. First, I measure the exposure of each county to convict labor as the value of convict-made goods in all U.S. prisons weighted by the distance from those prisons to the county centroid. Second, I construct cross-section of the county-level incarceration rates. As U.S. censi contain data on county of confinement of prisoners, but not the county of arrest I use first available data from Eriksson (2015) to construct the incarceration rates in 1920 and 1930, based on prisoners' county of origin.

Using ordinary-least-squares specification with state fixed effects I also test if convict labor affected incarceration rates: groups of people that benefit from convict labor might be incentivized to increase incarceration rates.[1] To estimate the causal effect of convict labor on incarceration, I use data from Eriksson (2015) to construct the incarceration rates in 1920 and 1930, based on prisoners' county of origin, and I show that higher exposure to convict labor led to increased incarceration rates. I also show that counties that were more exposed to convict labor and more likely to over-incarcerate Black and foreign-born White males. While I control for a wide set of socioeconomic and demographic variables, it is challenging to identify causal effects of convict labor on incarceration rates due to the embedded endogeneity problem. First, 19th century prisons were located

[1]Alternatively, the opportunity cost of crime decreased in those locations due to competition with convict labor.

in highly populated areas where crime rates were higher. Second, both racial discrimination, and endogenous choice of industry and the amount of goods produced by prisons can be correlated with local institutions.

To address these concerns, I employ an instrumental variable estimation. In particularly, I use first massive rapid expansion of the U.S. convict labor brought about when the National Prison Association (hereafter NPA) held its first congress, in Cincinnati, in 1870.[2] Thus, I use within-state distance to Cincinnati as an instrument for the value of goods produced by convict labor. This distance correlates with the likelihood that wardens would attend the conference and with the cost of getting information about convict labor profitability. Distance to Cincinnati does not, however, correlate with any important variable in 1870.

Thus I impose two main identifying assumptions. First, that distance to Cincinnati is an information treatment, and that the closer wardens and other prison executives lived to Cincinnati the less it cost them to arrive and get new ideas about employing prisoners for industrial purposes. During the first NPA congress its "Declaration of Principles" was accepted. This declaration promoted the establishment of agricultural or industrial departments within prisons to relax states' financial burden and give prisoners useful skills as a part of their rehabilitation (Wines (1871)). Thus we expect to see a higher value of goods produced by convict labor and a greater number of employed prisoners in 1886 the closer the prison was to Cincinnati. Second, I assume, that distance to Cincinnati does not affect incarceration rates not through the convict labor.

To address plausibility of the identifying assumptions I perform a number of sensitivity tests. First, I use placebo distances to all U.S. counties as an instrument, to show, that distance to Cincinnati yields the strongest first-stage. Second,

[2]The conference was the creation of one visionary man, Reverent Enoch Wines, who was the secretary of the New York Prison Association at that time. Being a deeply religious person, he believed prisoners could be rehabilitated through education, Bible study, and hard labor. He convinced the governor of Ohio to help organize the conference in Cincinnati, and then he became the association's first president. (See more details in McKelvey (1936).)

I show that results are entirely driven by "compliers" — 24 states that were represented at the NPA meeting. Third, I show, that distance to Cincinnati affects convict labor only through the NPA's participants. Finally, I provide extensive evidence that distance to Cincinnati does not correlate with any important variable in the 1870s U.S. Manufacturing census[3].

Despite the fact that convict labor was abolished by the 1940s and policemen lost their direct (or indirect) incentives to arrest Black and foreign-born population, racial discrimination persisted through the police traditions. A generation after, with the start of the war on drugs, the tradition of stopping Black/Hispanic people on streets end up in charging them with possession of drugs. Even if consumption of drugs is similar cross racial groups in the U.S. higher checks of the minorities lead to higher convictions rates, and Black/Hispanic prison population grew even without monetary incentives to the police.

To test this hypothesis, I use the same IV strategy, and estimate the reduced form effect of convict labor on contemporary arrest rates of adults and juveniles for marijuana offenses in 2000 by race. Usage of marijuana related arrests provides the cleanest measure of racial discrimination, since consumption of marijuana is similar for both black and white population groups, while incarceration data contain only information about current location of penitentiary where inmates are located but not the county of their arrests.

I show that the results are robust to various model specifications and ways I construct the explanatory variable. I demonstrate that results are not entirely driven by differences between counties with and without prisons: I find that results hold within the sample of counties with prisons. Finally, I show that my results are not driven by a sub-sample of states.

My contribution to the literature lies in the following aspects. First, it con-

[3]While it correlates with the value of agricultural output and share of Black population, I control for these variables in all my specifications.

tributes to the economic paper that studies the coercive institutions (Fogel and Engerman (1974); Wright (1978); Acemoğlu and Wolitzky (2011); Naidu (2010); Naidu and Yuchtman (2013); Dippel et al. (2015)) and coerced labor in the United States in particular (Poyker (2018c,b)). Second, this paper contributes to the field of U.S. economic history, and especially in racial discrimination (Alsan and Wanamaker (2016); Edwards et al. (2013); Eriksson (2015); Tian (2017)). Third, the paper is related to the literature related to coerced labor and long-run economic consequences. Buggle and Nafziger (2015); Markevich and Zhuravskaya (2017) studies economic consequences of the institutions on serfdom in Russian Empire, while Kapelko et al. (2015) investigate the consequences of Soviet GULAG. Dell (2010) examined the long-run adverse impacts of the forced mining labor system in 16-19 century Peru and Bolivia on contemporary health outcomes. Although, a growing literature continues to study the importance of institutions the case of US convict labor system is unique as it allows to show the persistent effect of labor coercion on economic outcomes and distinguish the channel of its effect.

The paper is organized as follows. The historical background of convict labor in the United States is introduced in Section 2.2. Section 2.3 describes the data. Section 2.4 presents my identification strategy and estimation results. Section 2.5 address alternative explanations, and presents robustness and sensitivity checks. Section 2.6 lays out my estimates of long-run effects of convict labor on contemporary racial discrimination in policing. Section 2.7 concludes.

2.2 Convict Labor: Historical Background and Implications

U.S. convict labor system emerged after the Civil War. Sharp increase in crime and incarceration rates demanded expansion of the states' correctional facilities.[4] States with their depleted budgets were reluctant to increase their expanses on penitentiaries and started to impose convict labor legislation. These laws allowed prison wardens to employ prisoners and use resulting profits to maintain themselves and contribute to their states' revenues.

2.2.1 Convict Labor in the Northern States

By 1870, only eight prisons across the U.S. (all in New York) operated with a modest net profit (Department of Labor (1900)). However, by 1886, all (but two) U.S. states have accepted some sort of convict labor legislation. It created a differential change in the amount of prison-made goods: from near zero dollar amount, mostly stone production, to at least 0.5% of GDP in 1886.

The catalyst for this rapid expansion was the creation of the National Prison Association (hereafter NPA) in 1870. The secretary of the New York Prison Association, Reverent Enoch Wines, with a help of a few other believers in rehabilitation of prisoners through education and labor, convinced then the governor of Ohio (and future U.S. president) Rutherford Hayes to organize a conference in Cincinnati: the first congress of the NPA.[5] Wines was able to gather and convince 140 delegates from 24 states to sign a "Declaration of Principles," which emphasized the importance of labor for rehabilitation, whereby prisoners would be taught

[4] The literature on U.S. convict labor system summarized by Poyker (2018c). More detailed descriptions of the system can be found in McKelvey (1936); Gildemeister (1978), and Lichtenstein (1996).

[5] The organization exists nowadays as the National Correctional Association: it is the primary U.S. supranational prison-overseeing association devoted to rehabilitation of prisoners through labor and education.

skills that would help them to find a job upon release. Probably more important, they presented papers describing the successful, self-sufficient, and profitable existence of the New York prisons and prisons using convict labor in various industries around the globe (e.g., the Irish system) (Wines (1871)).[6] NPA's establishment will be integral part of the identification stratery used in the empirical part of the paper and will be discussed in grater details later on.

The main feature of the convict labor in the Northern States was establishment of the factory on the prison premises and employment of prisoners within prison walls. In most cases inmates were employed by a private contractor who organized the production and trained the prisoners, while the state was housing and guarding the prisoners.[7] In some cases prion warden was in charge of employment of prisoners, and state furnished prison's workhouse or factory. These prisons were employed in predominantly low-skill intensive industries and were producing cheap goods. A the unit-labor cost of prison labor was much smaller than that of free laborers, parties involved (private contractors and prison wardens) benefited substantially from convict labor Department of Labor (1901).[8]

Convict labor systems accepted in the North did not directly incentivized policy to arrest more people. Nevertheless, as wardens were personally benefiting from convict labor they often used bribes to increase supply of the convict workforce.[9] They could bribe police to arrest and "invent" a crime for some particular

[6] It is unclear if the well-being of prisoners guided delegates of that congress the same way as Reverent Wines or that they cared about the profitability of their prisons, but after 1870 industrial and agricultural complexes started to appear in almost all correctional facilities and jails. Prison-made goods became so widespread and caused so many complaints from firms that were competing with prisons that Senate and House of Representative directed the Commissioner of Labor to collect data concerning convict labor in 1886 and analyze if convict labor affects firms using free labor in the same industries (Department of Labor (1887)).

[7] The were no free entry into employing prisoners, and only politically-connected firms were able to establish production there (Gildemeister (1978)).

[8] Prisoners were essentially worked for free and were punished for not working. More about working conditions of prisoners can be found here Department of Labor (1887).

[9] Wardens' salary did not directly depend on the profitability of the prison. Thus they often misreported the true output of their prisons and acquiring those profits themselves instead of

people that interested wardens: "... negro in the [omitted] penitentiary ... was a wizard at cutting. Soon after he was released they planted something on him and got him sent back because they couldn't spare him."[10] Wardens also bribed judges to hand down longer sentences.

The situation is similar to the contemporary "Kids for Cash" scandal (New York Times (2009)) and in line with the mechanism of how nowadays private prisons affect sentencing decisions (Dippel and Poyker (2018)). For example, John T. McDonough, former New York Secretary of State and chief of New York's BLS to the Industrial Commission (Department of Labor (1901) p. 296.) said in his testimony:

> "In the penitentiaries in the old times, when we did not have enough men to do the work, our police were instructed to put men in there. ... Judge Nott, lately, the [Albany] county judge, testified ... that he was offered $100 for every long-term prisoner that he would send to penitentiary [Clinton prison]".

McDonough also said that police were monitoring newly discharged prisoners. As they were entitled to some money upon release, they usually did not go far until they began spending it. If they went on a bender or to a brothel and "pushed the door" they might be arrested for burglary and returned to the prison.[11]

paying it to the state.

[10]Department of Labor (1925), p. 124.

[11]This happened not only in states with massive private interest in the convict labor industries. In Alleghany County of Pennsylvania, magistrates at the requests of the workhouse superintendent gave stiffer sentences to good coopers. For example, one black worker from Pittsburgh was arrested on a drunk charge who got six-month sentence instead of usual 2 to 30 days because "he was the fastest barrel maker in the state" (National Labor Tribute (January 14, 1882)).

2.2.2 Convict Labor in the Southern States

While convict labor in the North was driven largely by industrialization and rapid population\crime growth, reasons for its emergence in the South were different: there the main factors were the lack of prisons and the abolishment of slavery.

Systems of convict labor varies across states but two systems of convict labor worth mentioning separately, due to their geographic prevalence, unique properties, and interrelation with southern political institutions and slavery. These systems are convict leasing, and public works and ways (better known as chain gangs). One of them – convict leasing – assumed monetary incentives to the police and judicial system.[12] Convicts were leased on the first-bid actions, and while the lion share of the money went to state, some portion of that price was paid to sheriff and judge who were directly involved in that case. These created incentives for police to arrested less socially protected Black people and charged them with vagrancy or minor crimes, in order to auction out them later. Moreover in the South, various Jim Crow laws made it even easier to find an excuse for arrest. Sometimes, police would "round up idle blacks in times of labor scarcity" (Cohen (1976)). However sometimes, sheriffs were directly asked to arrest more people before the cotton harvest season (Blackmon (2009); Oshinsky (1997)).

Convict leasing is system of penal labor frequently regarded as a continuation of the slavery in the southern sates[13]. It was introduced during the Reconstruction period (1865–1877) when the government of the U.S. were trying to revive economy of the former Confederate states and was intended to replace the labor force once their slaves had been freed. Prisons had right to "lease" convicts to the firms or farms/plantation to work for free (in comparison with non-southern states that payed (miserable) wage to convicts).

[12] More about systems of convict labor, and its emergence in the United States in Sharkey and Patterson (1933).

[13] Convict leasing existed in some northern states as well but was less widespread and posses less similarity with slavery (Lichtenstein (1996)).

Most of the prisoners involved in the convict leasing were black males (Litwack (2010)), thus creating a racial incarceration gap that persists until today. The practice peaked around 1880 and was used to supply labor to farming, railroads, mining, and timber industry. The state of Virginia never imposed convict leasing system, Tennessee was the first state to officially abandon it in 1893 while Alabama was the last one (in 1928). However, whenever state prohibited convict leasing they were substituting the existing system with the Public Works and Ways system. Prisoners continues to work on infrastructural projects under direct states' supervision. Thus convict leasing persisted in various forms until it was abolished for good by Franklin Roosevelt in 1941.[14]

The reason why convict leasing lasted for so long was mainly economic: according to Mancini (1996) on average profit from each convict was four times higher then cost of prison administration. In addition to black people, white immigrants also were frequently leased to work for some factories, however in the south, due to discrimination white people mostly worked on less difficult works and were employed inside prisons by contract system.

2.2.3 Demise of the Old Convict Labor System

By the late 1930s, the modern American prison system had existed for more than one hundred years. During that time, many penal institutions themselves had remained unchanged. Convicts lived in a barren environment that was reduced to the absolute bare essentials, with less adornment, private property, and services than might be found in the worst city slum. One aspect that had changed rather significantly, however, was the prison labor system. In 1929 Congress passed the Hawes-Cooper Act, which enabled any state to prohibit within its borders the sale of any goods made in the prisons of another state. By the time the act became effective in 1934, most states had enacted laws restricting the sale and

[14] See Section 2.7 for additional information about convict leasing.

movement of prison products. In 1935 the Ashurst-Sumners Act strengthened the law to prohibit the transportation of prison products to any state in violation of the laws of that state, and prohibited to have any contracts with private contractors.[15] In 1940 Congress enacted legislation to bar, with a few exceptions, the interstate transportation of prison-made goods. These developments contributed to decreased reliance on prison labor to pay for prison costs. More and more inmates became idle and were not assigned to jobs.

As a result, by 1940, all convict labor was concentrated in the public systems, either producing goods for consumption by its state or employing prisoners in chain gangs. The latter was abolished in 1941 by President Roosevelt's Circular 3591. State-use of convict labor remained the only form of convict labor afterward, and the problem of competition with convict labor was quieted until 1979, when Congress revived the private system of convict labor by establishing the Prison Industry Enhancement Certification Program.

2.3 Data

In this Section I describe data sources and the way I constructed main variables.

2.3.1 Data on Convict Labor

To measure exposure of each county to I digitalized an archival dataset of prisons and convict-labor camps in 1886, 1895, 1905, 1915, 1923, 1932 and 1940 collected by the U.S. Department of Labor.

As my outcome variables are cross-sectional, my main main explanatory variable is also cross-sectional. I construct three measure of counties exposure to convict labor:

[15] Although it allowed to sign contracts not exceeding $10,000 annually.

The first is similar to one used in Poyker (2018c), where I weigh the effects of each prison by the distance between it and a given county and by counties industrial composition:

$$CL^1_{c,t} = \sum_{i \in I} \left(\lambda_{i,c} \times \sum_{p \in P_t} \frac{\ln \left(\text{Value of goods produced}_{i,p,t} \right)}{\text{Distance}_{c,p}} \right), \tag{2.1}$$

where P_t is the set of all prisons at year t, $\text{Distance}_{c,p}$ is a distance between prison p and county's c centroid (in km), and $\lambda_{i,c}$ is a value share of industry i in county c in 1870.

Second measure only weighs output of each U.S. prison by the distance from it to the county's centroid:

$$CL^2_{c,t} = \left(\sum_{p \in P_t} \frac{\ln \left(\text{Value of goods produced}_{p,t} \right)}{\text{Distance}_{c,p}} \right). \tag{2.2}$$

The second measure may be more applicable as we do not want to measure effect of convict labor on the local laboe market. Even if county is not affected by the competition with prison-made goods because its industrial composition is different incarceration may be still affected as local police will may be incentivized to increase number of employed convicts in a nearby prison. Hereafter I refer to the first two measures of convict labor as "continuous" as they treat all counties.

The third measure is constructed as the value of goods produced in a county, and thus assume only those counties as treated if they had a prison (hereafter I refer to this measure of convict labor as "discrete"):

$$CL^3_{c,t} = \ln\left(\sum_{p\in P_{c,t}} \left(\text{Value of goods produces}_{i,p,t}\right)\right), \tag{2.3}$$

The underling assumption here is that wardens are more capable to incentivize police and judges nearby, and at the same time, the demand for convict labor is higher around the prison. I prefer this measure and use it as a baseline hereafter because it may be easier for prison wardens to incentivize local police than police in a counties farther away.

2.3.2 Data on Historical Incarceration Rates

The quality of incarceration data makes it difficult to identify the effect of convict labor on incarceration immediately after convict labor was allowed in 1870s-1880s. In particular censi contain information about the location of group quarters, thus we can observe where inmates are currently confined but not where they were arrested.

First available data that contains the county of pre-arest residence of inmates from Eriksson (2015) is for 1920. I use the following formula to construct incarceration rates for 1920:

$$\text{Incarceration Rate}_{c,1920} = \frac{\#\text{Inmates}_{c,1920}}{\text{Population}_{c,1920}} \times 100,000. \tag{2.4}$$

Similarly, I construct incarceration rates by race, gender, and foreign-born status. E.g., Black Incarceration $\text{Rate}_{c,1920} = \frac{\#\text{Black Inmates}_{c,1920}}{\text{Black Population}_{c,1920}} \times 100,000.$

2.3.3 Data on Contemporaneous Arrests Rates

Data for contemporaneous arrests come from Uniform Crime Reporting Data: Arrests by Age, Sex, and Race (2000) available at ICPSR.[16] I calculate the arrest rate for drug usage and vagrancy as the number of arrests for drug and vagrancy is again divided by the total population in the given county in the year of 2000 multiplied by 100,000 to obtain the arrest rate per 100,000. Similarly, I compute arrests by races and gender. In addition, I compute placebo outcome – arrests for violent crimes that should be less dependent on racial practicing of the police.

2.3.4 Other Data

All county-level data for controls are taken from U.S. censi (Haines (2004); Ruggles et al. (2015)). I work with county level data for the years 1870 and 1880. The data was obtained from Historical, Demographic, Economic, and Social Data: The United States, 1790-2000, ICPSR 2896 (Haines (2004)). Additionally, I use data on county topography and demographics from the paper "Data Set for Births, Deaths, and New Deal Relief During the Great Depression" by Fishback et al. (2007) generously made available on Price Fishback's website.

I drop counties in Hawaii or Alaska and underpopulated counties with population less than 1,000 in any decade in the 20th century. The quality of some of the key variables is not ideal. Substantial measurement error is likely to be present at the beginning of our sample period.

I use the following variables as controls: total population of each county; urban population in each county divided by the total population of each county (calculated as population residing in places of 2,500 or more persons); shares of population of black and foreign-born (defined as the share of Black (foreign-born)

[16]While data is available starting from 1990s, I choose 2000 as a baseline specification for my summary statistics to be more comparable with those of Bunting et al. (2013). However all results hold if I use other years.

over total population); share of employment in agriculture (defined from individual level data as the number of individuals who reported working on agriculture over the total number of individuals with reported industry); total county level manufacturing wages in thousands of dollars of 1880; value of farm products in thousands of dollars of 1880 (total value of farm products for each county in thousands of dollars). Lynching data is taken from the Historical American Lynching Data Collection Project.

2.4 The Effect of Convict Labor on Incarceration Rates

In the previous section, I described how convict labor incentiviced police arrest more people, and provided historical records that convict labor by itself caused an increase in crime rates and incarceration across the United States between 1870 and 1940. Thus convict labor could affect incarceration rates by providing incentives to police, judges and wardens. Moreover, manufacturing wages and employment suffered on the local labor markets around prisons (Poyker (2018c)) thus making opportunity cost of crime smaller. This indirect channel could also contribute to the raise in incarceration rates.

Convict labor could have affected incarceration through both direct and indirect effects. First, the decrease in wages could have decreased the opportunity cost of crime (à la Becker (1968)) for the poor, increasing crime rates. Second, prisons could have directly affected incarceration, through two main channels. First, predominantly in Southern states that adopted convict leasing, the police and the judicial system were directly incentivized to arrest more people. In particular, inmates were leased out via an auction, the highest bidder paid the bid to the state, while everyone involved (sheriff, judge, public officials, and even witnesses) were getting a share (Cohen (1976); Blackmon (2009); Oshinsky (1997)). Second, while other forms of convict labor provided no direct monetary incentives for police or judges, prison wardens themselves were colluding with judges, police, and contractors. Wardens also bribed judges to hand down longer sentences (Department of Labor (1900, 1901)).

Estimation of the effect of convict labor on incarceration is problematic due to lack of data. Even if the full count censuses of 1880 and 1910 were available for construction of county-level measures of convicts, there would be a measurement error, as they reflect the county where inmates served their term but not where they lived. The first reliable source of data comes from Eriksson (2015), who collected data on prisoners and their county of residence for 1920 and 1930. Thus I choose to use the cross-sectional specification:

$$\text{Incarceration Rate}_{c,1920} = \alpha + \beta CL_{c,1886} + \mathbb{X}_c^{'}\Gamma + \mu_s + \varepsilon_c, \tag{2.5}$$

where Incarceration $\text{Rate}_{c,1920}$ is the number of inmates in any state prison who live in county c.[17] $CL_{c,1886}$ is weighted by distance log of value of goods produced by all prisons measured for county c at year $t = 1886$; $\mathbb{X}_{c,1880}$ is a matrix of county-level controls at year $t = 1880$; and μ_s are state fixed effects. I use a set of socioeconomic control for counties' economic conditions, and I use population, urban share, and share of Black and foreign-born population as proxies for crime rates. I cluster standard errors on the state level.

2.4.1 Identification

Because I cannot control for baseline crime rates well enough, endogeneity concerns remain. For example, if prison were located in locations with higher crime rates, I will overestimate the effect of convict labor.

[17]Almost all prisoners at that time were confined under state penitentiaries and thus were incarcerated in the states of their residence.

To address this concern, I use instrumental variable analysis. I exploit the massive expansion of convict labor brought about when the National Prison Association (hereafter NPA) held its first congress, in Cincinnati, in 1870.[18] Thus, I use distance to Cincinnati as an instrument for the value of goods produced by convict labor.

The congress held a series of lectures about the experience of penitentiaries around the world, and how education and labor rehabilitate prisoners, by teaching them skills that will prevent them from ending up in prison in the future. In particular, the reports featured stories from New York prisons and prisons in Ireland that already had an extensive history of employing prisoners. After the congress, the NPA enshrined its "Declaration of Principles" (Wines (1871)). It declared that "We [shall] have imparted to him [prisoner] the capacity for industrial labor and the desire to advance himself by worthy means." In particular, it suggested that prisons should establish industrial and/or agricultural departments, as appropriate, and that "these would be run as efficient business organizations, returning profits to the institution and providing training and craft skills to the inmates."

The idea behind the instrument is that the closer wardens and other prison executives lived to Cincinnati the less it cost them to arrive and get new ideas about employing prisoners for industrial purposes. Thus the distance correlates with the likelihood that wardens would attend the conference and with the cost of getting information about convict labor profitability. We expect to see a higher value of goods produced by convict labor and a greater number of employed prisoners in 1886 the closer the prison was to Cincinnati.

I introduce an example from New Jersey to demonstrate that visiting the NPA congress in Cincinnati indeed affected the decision to open industrial or agricul-

[18]The conference was the creation of one visionary man, Reverent Enoch Wines, who was the secretary of the New York Prison Association at that time. Being a deeply religious person, he believed prisoners could be rehabilitated through education, Bible study, and hard labor. He convinced the governor of Ohio to help organize the conference in Cincinnati, and then he became the association's first president. (See more details in McKelvey (1936).)

tural departments within prisons' premises. Five representatives from New Jersey attended the congress. One of them, was Samuel Allinson, deputy of New Jersey's governor and a member of the board trustees of the state reform school in Yardville, in Mercer County. In 1869, Allinson was appointed to a commission at the Trenton State Prison, but he did not mention convict labor in his recommendations (New Jersey Historical Society (1884)). After attending the congress, he wrote two papers, about discharged prisoners (Allinson (1872)) and about scholastic and industrial education in reform schools (Allinson (1876)). In 1879, he was appointed to a similar board at the same prison, where he wrote a report suggesting expanding convict labor. We can't know for sure whether Allinson modified his beliefs about convict labor because of what he learned at the congress, but his obituary (New Jersey Historical Society (1884)) indicates that he became a pro-convict labor activist after 1870. This example shows that the instrument is indeed plausible, and that the ideas disseminated at the NPA's first meeting could have affected attitudes toward convict labor across the United States.

Overall, the first stage can be written as follows:

$$CL_{c,1886} = \tilde{\alpha} + \widetilde{\beta}\text{Distance to Cincinnati}_c + \mathbb{X}'_{c,1880}\widetilde{\Gamma} + \widetilde{\Pi}\Psi_{c,1870} + \tilde{\mu}_s + \epsilon_c, \quad (2.6)$$

where $CL_{c,1886}$ is weighted by distance log of value of goods produced by all prisons measured for county c at year $t = 1886$; $\mathbb{X}_{c,1880}$ is a matrix of county-level controls at year $t = 1880$; $\Psi_{c,1870}$ – matrix of pretreatment ($t = 1870$) controls; and μ_s are state fixed effects.

The second stage can be written as follows:

$$\text{Incarceration Rate}_{c,1920} = \alpha + \beta\widehat{CL_{c,1886}} + \mathbb{X}'_{c,1880}\Gamma + \Pi\Psi_{c,1870} + \mu_s + \varepsilon_c. \quad (2.7)$$

For distance to Cincinnati to be a good instrument we need it to be not correlated to other variables in 1870 that can potentially correlate with dependent and explanatory variables. For example, if it correlates to the urbanization share or local crime rates it can potentially violate exclusion restriction.[19] To test it, in Table 2.9 I run OLS regression of distance to Cincinnati on, incarceration rates, log of population, urban share, share of women, share of African-American, log of manufacturing and agricultural outcomes, literacy, etc.[20] Only log of agricultural output and share of Black seems to correlate with the instrument, and I control for these two variables in all specifications.[21]

To demonstrate that distance to Cincinnati is indeed an information treatment for those wardens that came to the conference in Table 2.1. In Columns I-V I regress the instrument on the number of delegates from each county. I show results with no controls in Column I, and then add state fixed effects (Column II), socioeconomic and demographic controls (Column III), and geographic controls (Column IV). The coefficient is very significant, suggesting that counties further away are less likely to send delegates. In Column V I restrict the sample to the 25 states that actually sent delegates, in case state that did not do it are different; nevertheless, the results hold. In Column VI I test other functional form (log distance) of the instrument, and I use log of counties market access (from Donaldson and Hornbeck (2016)) in Column VII. Both yield robust results. These results hold if I use a indicator variable if the county sent a delegate.

Even if I have names and origin cities of each of the 140 participants, I do not use the dummy if a county sent a representative (or a number of delegates) to the conference since it can be endogenous to the personal views of the warden and

[19]It is hardly true that distance to Cincinnati has a direct effect on incarceration rates in 1920, but indirect effect may be plausible.

[20]As incarceration rates I used actual number of people in prisons, thus it rather shows that distance to Cincinnati does not correlate to prisons in 1870 and the capacities of these prisons.

[21]Also results do not change if I drop them. I tries other covariates from the 1870 manufacturing census, but non of them are significant, and I don't report them.

Table 2.1: National Prison Association Congress Delegates, Distance to Cincinnati, Ohio and Convict Labor

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI
Outcome:				Delegate at NPA					Value of Prison Output (1886)		(1895)
Distance to Cincinnati, OH	-0.0197***	-0.0549***	-0.0792***	-0.0959***	-0.159***					-0.466	0.533
(1000 km)	(0.00611)	(0.0197)	(0.0222)	(0.0226)	(0.0291)					(0.400)	(0.712)
ln of Distance to Cincinnati, OH						-0.0366***					
						(0.0121)					
ln of Market Access (1870) to Cincinnati, OH							-0.0643**				
							(0.0242)				
Delegate at NPA								3.987***	4.017***	4.000***	2.918***
								(0.726)	(0.747)	(0.749)	(0.833)
State FE		X	X	X	X	X	X	X	X	X	X
Socio-Economic Controls			X	X	X	X	X	X	X	X	X
Geographical Controls				X	X	X	X	X	X	X	X
Sample		Full				25 NPA-States		Full		25 NPA-States	
R squared	0.01	0.09	0.17	0.18	0.18	0.18	0.18	0.43	0.38	0.38	0.20
Observations	3,109	3,108	2,265	2,265	1,373	1,373	1,372	2,266	1,374	1,374	1,374

All columns contain constant. Robust clustered by state standard errors in parentheses. *** p $p < 0.01$, ** p $p < 0.05$, * p $p < 0.1$

his propensity to promote convict labor. However, I can use it to demonstrate the mechanism, how distance to Cincinnati affected convict labor. In Columns VIII and IX I regress number of delegates on the log value of prison-made goods produced in that county in 1886 on the full sample of states, and on the subsample of 25 states with delegates.[22] The point-estimate is virtually the same, and is highly significant. In Column X I add distance to Cincinnati in addition to the number of delegates. The coefficient for the number of delegates does not change, and the distance to Cincinnati becomes insignificant. This suggest, that whole effect comes through the delegates, but not the distance to Cincinnati per se. Finally, in Column XI I do the same specification bu use value of convict labor in 1895 instead of 1886. While results hold (delegates has positive effect on value of convict labor, and distance to Cincinnati is insignificant), the magnitude of the point-estimate for the deligates decreases. Its explanatory power decreases over time, as by 1895 convict labor becomes more widespread, and delegates and distance to Cincinnati are worse predictors of convict labor further away from 1886.

Finally, I present reduced form results with the number of NPA Congress participants in Table A2.14.

2.4.2 Results

In this section I first, estimate the effect of convict labor on incarceration rates in 1920 and 1930, and second, study if the effect was disproportional for the minorities.

[22]In fact instead of log I use inverse hyperbolic sine transformation ($\log(y_i + (y_i^2 + 1)^{1/2})$). It is approximately equal to $\log(2y_i)$ or $\log(2) + \log(y_i)$, and so it can be interpreted in exactly the same way as a standard logarithmic variable but without doing $\log(1 + y_i)$ (Burbidge et al. (1988)).

2.4.2.1 Effect of Convict Labor on Incarceration

I start my analysis by demonstrating in Table 2.2 strong correlation of the incarceration rates in 1920 and instance of convict labor measured as log of value of goods produced in a county. In Column I I show strong negative correlation of convict labor in 1886 and incarceration rates in 1920. The standard deviation of the log of value of prison-made goods in a county with prison is 2.2, thus one standard deviation increase in the value of prison-made goods in 1886 is associated with 73 additional people convicted in that county in 1920 or 50 percent of its standard deviation. Similar results can be observed in Columns II-V where I run ordinary-least-squares regression of convict labor in 1895, 1905, 1915, and 1923 on the incarceration rates. While the coefficient varies — one standard deviation in convict labor is associated with increase in incarceration rates from 36.4% to 47.8% of its standard deviation.

Table 2.2: Convict Labor and Incarceration in 1920

	I	II	III	IV	V
	Dependent variable: Incarceration (1920)				
Convict labor (1886)	33.21***				
	(7.253)				
Convict labor (1895)		48.05***			
		(11.16)			
Convict labor (1905)			25.05***		
			(6.039)		
Convict labor (1915)				39.72***	
				(12.06)	
Convict labor (1923)					41.89***
					(9.082)
R-squared	0.27	0.30	0.23	0.20	0.26
Observations	2,185	2,185	1,946	1,503	1,800

All columns contain constant and state fixed effects. The following variables are used as controls: ln of total population (1880), urban share (1880), share of Black (1870, and 1880), share of women (1880), share of foreign-born (1880). Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Similar correlation is observed if I use continuous measures of convict labor (3.1 and 3.2). In Table 2.10 of Appendix I present similar results for incarceration rates in 1930.

Table 2.3: Effect of Convict Labor om Incarceration Rates in 1920

	I	II	III	IV	V	VI
	Dependent variable: Incarceration (1920)					
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict labor (continuous)	84.32***	69.27**				
(weighted by industry & distance)	(23.09)	(29.07)				
Convict labor (continuous)			47.00*	61.58**		
(weighted by distance)			(25.16)	(24.76)		
Convict labor (discrete)					33.21***	53.96**
(nonweighted)					(7.253)	(23.63)
R-squared	0.18	0.18	0.11	0.11	0.27	0.20
Kleibergen-Paap F stat		12.31		17.20		6.93
Partial R-squared		0.031		0.112		0.004
Instrument's coefficient		-0.00102***		-0.00108***		-0.00142***
		(0.000289)		(0.000271)		(0.000517)
Observations	2,228	2,228	2,228	2,228	2,228	2,228

All columns contain constant and state fixed effects. The following variables are used as controls: ln of total population (1880), urban share (1880), share of Black (1870, and 1880), share of women (1880), share of foreign-born (1880). Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Correlations are remarkably stable over time and to be sure that the effect is indeed causal and not driven by unobserved characteristics of counties with convict labor (in particular crime rates) I employ instrument variable analysis. I present results of the IV estimation in Table 2.3. First, I use continuous measure of counties exposure to convict labor weighted by county's industrial composition and distance to prison. For the interpretation purposed I standardize all variables by subtracting their mean and dividing by their standard deviation. One standard deviation in exposure to convict labor in 1886 increases incarceration by 84 people in 1920 (Column I). The IV coefficient in Column II decreases by 18 percent but remains significant: comparing two counties, one on 25th percentile exposure to convict labor and one on 75th percentile, the more exposed on experienced 53 percent of its standard deviation increase in incarceration rates. The OLS coefficient for the specification with convict labor exposure weighted only by distance in Column III is almost twice as small as the one in Column I. But the

IV coefficient (Column IV) has similar magnitude to one in Column II. Finally, in Columns V and VI I show that results also hold for the discrete measure of exposure to convict labor.

I also report results for the similar specification but with incarceration in 1930 as a dependent variable in Table 2.11 of Appendix. All results hold.

2.4.2.2 Over-Incarceration of Minorities

To show that convict labor disproportionately affected minorities I slightly augment the specification:

$$\text{Black inc. rate}_{c,1920} = \alpha + \beta CL_{c,1886} + \Gamma \mathbb{X}_{c,1880} + \mu_s + \varepsilon_c, \tag{2.8}$$

where Black inc. $\text{rate}_{c,1920}$ is incarceration rate of Black in county c in state s at year $t = 1920$; $CL_{c,1886}$ is a weighted by distance log of value of goods produced by all prisons measured for county c in state s at year $t = 1886$; $\mathbb{X}_{c1880}$ is a matrix of county-level controls dated by pre-convict labor date of 1880, and μ_s are state fixed effects. The standard errors are clustered at the state level.

Usage of continuous variable of interest $CL_{c,1886}$ is imperative, since police in counties with prisons were more likely to be incentivized to arrest more people than those where there were no labor camp. However, I assume that policemen located in the county that is closer to prison will be more incentivised to arrest more people than in a county that is further away, since the costs of transportation of prisoners are increasing.

The revenue and expenses of prison were directly linked to state's budget, thus state fixed effects can eliminate the concern that the poorer counties could have higher crime rates among the poorest population, and at the same time more extensive usage of convict labor that would decrease costs of up-keeping existing prisons and improve its financial situation. However, as a state could install a plant in those prisons strategically in order to stimulate future tax revenues in depressed counties will magnify the coefficient of interest, thus, I control for county tax revenues, as a proxy for the fiscal health of the county.

In addition, as prisons appeared in places with higher population and urban share, I use the corresponding controls.

Fixed effects are especially important, as convict labor laws were state-specific, and because states were prohibiting usage of private forms of convict labor and switching to the state use and public works and ways system at different years. Thus, there is a heterogeneity in the number of how many years convict labor affected racial discrimination.

Nevertheless, unobserved heterogeneity concern remains, as some important issues cannot be addressed and cause a bias that magnifies the coefficient of interest. First, counties with more developed coercive institutions can be more involved in the convict labor and at the same time to have higher level of racial discrimination, thus affecting incarceration rates on non-whites. Second, if location of prison decreases social capital of the surrounding counties (e.g., because prisoners remain nearby prisons after release) it may affect local crime rates and cause higher incarceration rates in the future. These two sources of unobserved heterogeneity will cause upward bias that in the OLS estimates.

While issues described above tend to magnify the coefficient of interest, there is an important source of the bias that can bias it toward zero: the measurement error. As labor camps were afraid that their activity would be subject to state or federal legal restrictions due to unfair competition with firms using free labor, prisons' administration could under-report the value of goods produced. However, it will work against me showing the effect on incarceration rates. Finally, reverse causality is unlikely to cause bias, as I use Black incarceration rate 34 years after

Table 2.4: Convict Labor and Black Incarceration Rates (1920)

	I	II	III	IV
Dependent variable:	Black incarceration rates (1920)			
	OLS	Reduced form	First stage	Second stage
St.dev. Convict labor	19	713	713	19
Log of value of goods produced (1886)	17.29			51.43*
	(15.4)			(29.3)
Distance to Cincinnati , km		-1.17*	-0.023***	
		(0.69)	(0.005)	
Partial R^2			0.1	
F- stat of ex. instrument			22.96	
Prob > F			0.00	
Anderson-Rubin p-value			0.08	
Observations	2,112	2,112	2,112	2,112
adjR^2	0.03	0.03	0.87	0.03

All columns contain constant, state fixed effects, geographic controls (latitude and longitude, dummies for coastal counties), 1880 and 1870 socio-economic controls (ln of total population, urban share, share of Black, share of women, share of foreign-born). Robust clustered by state standard errors in parentheses. 41 cluster. *** p<0.01, ** p<0.05, * p<0.1

the year of the main variable of interest. These facts make the overall direction of bias unclear.

Table 2.4 introduced OLS ans 2SLS regression for specification described above. Column I contain OLS specification: while the relation between convict labor and Black incarceration rate is positive, it is insignificant. The reduced form is presented in Column II, suggesting that Black incarceration rate in 1920 was larger in counties situated closer to Cincinnati. Column III contains the first stage of the 2SLS specification. The relation between the distance and convict labor output is very strong. The F statistics of excluded instrument is equal to 23 and partial $R^2 = 0.1$, suggesting that the instrument is unlikely weak. Finally, the second stage is reported in Column IV. The coefficient of interest is positive and significant, thus corroborating the hypothesis that convict labor favored incarceration of Black through racial discrimination.

It should be noted that the IV coefficient is three times bigger than the OLS one. This can be attributed to the fact, that the attenuation bias due to measurement error was bigger than possible upward bias caused by unobserved heterogeneity. Alternatively it could be the result of the weak instrument. However as we can see, the first stage result suggest that it is not the case. Finally, inflated IV coefficient can be result of the violation of the exclusion restrictions. While exclusion restrictions cannot be tested directly, a sensitivity check intended to alleviate this concern will be provided later in the robustness check section.

Table 2.5 show OLS and 2SLS results for incarceration of other population groups. As can be seen, incarceration was affected among all groups, even among the white, suggesting that it was not only because of the racial discrimination or due to distortion of human capital around prisons or economic incentives to increase labor force in prisons. The effect of convict labor is the biggest for non-white and non-black population group, that includes Hispanic and Asian (mostly in the west). All first stages are strong, with F-statistics above 10 in all Columns V-VIII.

2.5 Robustness and Sensitivity Checks

In this section, I briefly address the most important robustness and sensitivity checks for the long-run analysis. I start with showing additional pieces of evidence in favor that the distance to Cincinnati is a legitimate instrument for the expanse of convict labor in its first years and not violating exclusion restrictions.

Placebo

While the validity of distance to Cincinnati as an instrument for convict labor in 1886 is covered in great details in Section 2.4.1, it is important to show that the instrument is not spurious, by employing a series of placebo tests. Exclusion

Table 2.5: Convict Labor and Incarceration Rates by Races (1920)

	I	II	III	IV	V	VI	VII	VIII
Dependent variable:	Incarceration rates (1920)							
	Non-white	Non-white & non-black	White foreign-born	White	Non-white	Non-white & non-black	White foreign-born	White
	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
St.dev. explanatory variable	19	19	19	19	19	19	19	19
Log of value of goods	18.563	449.120	12.905	3.307	64.00**	1061**	34.15***	10.09***
produced (1886)	(15.516)	(329.356)	(8.125)	(2.090)	(29.57)	(456.7)	(13.00)	(3.477)
Partial R^2					0.12	0.06	0.12	0.12
F stat of excluded instrument					23.12	23.32	23.28	23.32
Prob > F					0.00	0.00	0.00	0.00
Anderson-Rubin p-value					0.02	0.03	0.01	0.00
Observations	2,134	1,233	2,159	2,175	2,134	2,175	2,159	2,175
adjR^2	0.017	0.035	0.019	0.044	0.01	0.02	0.02	0.04

All columns contain constant, state fixed effects, geographic controls (latitude and longitude, dummies for coastal counties), 1880 and 1870 socio-economic controls (ln of total population, urban share, share of Black, share of women, share of foreign-born). Robust clustered by state standard errors in parentheses. 41 cluster. *** p<0.01, ** p<0.05, * p<0.1

restriction can be violated if the distance to Cincinnati is correlated to trade or migration patterns that took place after 1870 and before the realization of intergenerational outcomes. In this case, even if the instrument is not associated to important socioeconomic variables in 1870, it still accumulates other effects that had happened during the century. One way to address this point is to show, that distance to Cincinnati, not just by accident a good correlate of convict labor, I present first stage F-statistics of the first-stage regressions with all possible distances to county centroid. This simple placebo test shows that geographic proximity to Cincinnati, yield the largest F-statistics among of all placebo tests substituting proximity to all other counties (See Figure 2.1). Thus the effects I measure is specific to geographic proximity to Cincinnati and not to a post-1886 condition affecting the United States overall.

Figure 2.1: First Stages for Placebo Tests with Proximity to all Other Counties

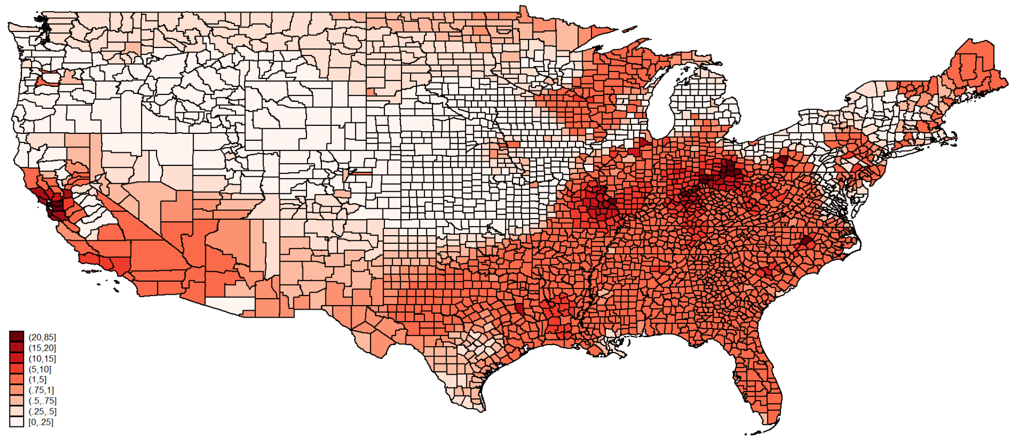


Figure 2.1 plots F statistics resulted from the first stage regressions (see Specification 2.6) with distance to each U.S. county instead of distance to Cincinnati, Ohio. Darker tones reflect higher first stage F-statistics. Source: Distances calculated using NEARSTATA module in STATA (Jeanty (2012)).

SUTVA

In the case of NPA congress, IV assumptions can be regarded as SUTVA, as wardens living close to Cincinnati who came at the conference were "compliers" and we would expect the instrument to affect only through them (Angrist et al. (1996)). Thus I used he fact, which only 25 states sent their delegates: in Table 2.16 I show that the instrument affects outcomes only in a sub-sample of the twenty-five states that sent delegates to the NPA Congress. Indeed, the size of IV coefficients is statistically insignificant from those on the full sample. At the same time, the same specification on a sub-sample of the states without delegates in Panel B yield week first stage F-statistics below one suggesting a relationship between the instrument and convict labor.

Sub-sample Analysis

In Table 2.15 I examine if results are driven by some sub-sample of states. First, in Columns, I report the baseline specification from Table 2.4.2. In Column II I omit North-Eastern states. All coefficients but relative absolute mobility remain significant (p-value= 0.16). The size of the coefficient does not change much, and I attribute this marginal insignificance to low sample size and marginally week first stage. For Columns II-VII the first stage F-statistics ranges between 5 and 8, that still passes week instrument test on 95% level. Then in Column III, I exclude southern non-Confederate states. All coefficients are significant and remain stable. However, if in Column IV I exclude Confederate states, the effect of convict labor on incarceration vanishes. Then, in Column V I drop Midwestern states. The resulting coefficients differ from those with full sample only in size of standard errors. Finally, in Column VI I drop both Great Plains and Far West states. A few sent their delegates to the NPA Congress anyway, and I do not expect much changes upon exclusion of these states: and indeed, results are as expected are very similar, although incarceration effect is smaller in magnitude than in the full sample.

Relaxing Exogeneity Assumptions on the Instrument

Finally, in vein of Conley et al. (2012) I relax the exogeneity assumptions of the instrument and examine the bounds we are able to place on the true effect of convict labor on the arrests of Black. The idea behind the method is simple: if in addition to exogenous and endogenous variables I add instrument (distance to Cincinnati) its coefficient (γ) required to be equal to zero according to standard IV estimation. However, by relaxing the constraint we can find the bounds for the IV estimate of convict labor (β). If one expects instruments to have direct or indirect negative effect on the arrests of black ($\gamma < 0$) I will underestimate the true effect of the convict labor on racial discrimination. This gives the minimum prior for γ. More challenging is to determine the maximum prior of γ. Thus I assume, that the maximum direct effect of instrument will be not bigger than the size of the biggest effect of one of the control covariates. The covariate with the biggest significant covariate (standardized) is the urban population in 1880. Applying Conley et al. (2012), I find that the bounds on the strength of β are still below zero (at 95% confidence level): $[7; 98]$ for black incarceration in 1920 and $[22; 180]$ for arrests of Black in 2000. Therefore, even allowing for imperfect exogeneity, the positive effect of convict labor on the racial discrimination is confirmed.

2.6 Long-Run Effects of Convict Labor: Contemporary Discrimination in Arrests

In this Section I show the persistent effect of convict labor on the over-incarceration of Black and other minorities. Unfortunately, contemporary incarceration data does not allow us to create good county level dataset, due to the fact that we can only observe the county of incarceration, but not the county of arrest. In addition, as around 13% of the prison population is contained under the federal prison system, those inmates serve their term even outside their states. To alleviate this concern I use the arrest data instead of incarceration.

In Table 2.6 I provide results of the arrests of Black for drug-related offenses and vagrancy laws. Column I contains OLS regression. While positive it is not significant. I report the reduced form in Column II. Column III contains the first stage of the 2SLS regression of value of goods produce by convicts on the Black arrest rate for drugs and vagrancy. The F statistics of excluded instrument is equal to 20.6, and despite the fact, that I add fixed effects, and the identification comes from the within-state variation of distance to Cincinnati, partial $R^2 = 0.09$. Column IV shows the result of the second stage, showing results consistent with those received in Table2.4.

Table 2.6: Convict Labor and Black Arrest Rates (2000)

	I	II	III	IV
Dependent variable:	Black arrest rate for drugs and vagrancy (2000)			
	OLS	Reduced form	First stage	Second stage
St.dev. convict labor	19	713	713	19
Log of value of goods	30.595			121.4**
produced (1886)	(48.592)			(61.2)
Distance to Cincinnati, km		-1.54*	-0.021***	
		(0.81)	(0.004)	
Partial R^2			0.09	
F stat of ex.instrument			20.60	
Prob > F			0.00	
Anderson-Rubin p-value			0.05	
Observations	1,813	1,763	2,172	1,813
adjR^2	0.082	0.089	0.874	0.08

All columns contain constant, state fixed effects, geographic controls (latitude and longitude, dummies for coastal counties), 1880 and 1870 socio-economic controls (ln of total population, urban share, share of Black, share of women, share of foreign-born). Robust clustered by state standard errors in parentheses. 41 cluster. *** p<0.01, ** p<0.05, * p<0.1

Similarly, I report results for other population groups in Table 2.7. Arrest rates among Hispanic are even bigger than for the Black population, however, other groups have the opposite or no effect on arrest rates.

Table 2.7: Convict Labor and Arrest Rates by Race (2000)

	I	II	III	IV	V	VI	VII	VIII
Dependent variable:	Arrest rates for drugs and vagrancy (2000)							
	Hispanic	Indian	Asian	White	Hispanic	Indian	Asian	White
	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
St.dev. explanatory variable	19	19	19	19	19	19	19	19
Log of value of goods	6.764	-12.123	0.621	-2.332	155.6*	-140.9**	-10.07	4.425
produced (1886)	(12.386)	(19.635)	(15.048)	(3.842)	(88.54)	(70.49)	(50.88)	(21.86)
Partial R^2					0.09	0.09	0.09	0.09
F stat of excluded instrument					20.39	20.39	20.34	20.39
Prob > F					0.00	0.00	0.00	0.00
Anderson-Rubin p-value					0.01	0.02	0.00	0.01
Observations	1,758	1,758	1,753	1,758	1,758	1,758	1,753	1,758
adjR^2	0.136	0.137	0.027	0.298	0.06	0.09	0.03	0.30

All columns contain constant, state fixed effects, geographic controls (latitude and longitude, dummies for coastal counties), 1880 and 1870 socio-economic controls (ln of total population, urban share, share of Black, share of women, share of foreign-born). Robust clustered by state standard errors in parentheses. 41 cluster. *** p<0.01, ** p<0.05, * p<0.1

Overall these results suggest that convict labor in 1886 has a persistent effect on racial discrimination that can be approximated through over-incarceration of Black population and other minorities.

It is important to note, that results provided in Tables 2.6 and 2.7 are based on the assumption that minorities are more likely to be stopped by police and thus checked for drugs, while at the same time have the same consumption of drugs as white population (Edwards et al. (2013)). This seems corroborate my story, since racial discrimination will cause police to pay more attention on minorities. At the same time arrest for the serious crimes should be solely explained by socioeconomic and demographic characteristics. Thus employing arrests for 20 other offense category provide no consistent significant effect of convict labor on arrests of any type of population group.

2.7 Conclusion

Institutional history has a profound influence on economic development and world inequality as many countries today still live in the shadow of colonial institutions established more than at least a century ago. One of the most damaging of such institutions is forced labor. When elites are able to coerce part of the population to perform unpaid labor, it can cement growth-killing power arrangements, reduce innovation, cripple "creative destruction" and eventually lead to lackluster economic growth. This study is part of the agenda of studying medium and long-term consequences of the forced labor institutions (Poyker (2018c,b)). While many other papers look at the institutions of developing nations, I explore institutions of forced labor in one of the most developed nations of the world: United States of America. Certainly, it might seem paradoxical to look at the adverse effects of forced labor in a country that rightfully belongs to the club of the richest nations, but one should be aware of an unequal distribution of the benefits of economic development in the U.S. (Oliver and Shapiro (2006); Frank (2009); Chetty et al. (2014a,b)).

In this paper, I showed that coercive institutions that had appeared in the United States after the civil war had an effect on the racial discrimination that resulted in over-incarceration of the minorities. Prison labor created incentives to arrest unprotected minorities in order to increase coerced labor supply, and even after its abolishment continued to persist through racial discrimination. I found that regions that were exposed to a more severe exploitation of convict labor experienced higher incarceration rates among minorities at 1920 are still worse off in terms of arrest rates for minor crimes.

The intuition behind the mechanism of racial discrimination is simple, as sheriffs and local police were incentivized monetary in arresting more people, they were trying to arrest as much as possible, especially among the least protected and easily visible distinguishable groups of states' population (e.g., Afro-Americans). As convict leasing system existed for long period of time (e.g., 82 years in Alabama), police get used to arrest members of those unfortunate groups and may continue to arrest them more often even after abolishing of the convict leasing system and thus monetary incentives. Since, police was slowly renewing itself overtime, more experienced policemen may share their traditions with those who was enrolled into service after the abolishing, thus transmitting the tradition for generations ahead[23]

[23]Similar tradition concerns related to corruption triggered the Georgian government to hire all policemen after the Revolution of Roses in 2007 and recruit entirely new police. Ukrainian government did the same in 2014, while their police reform was incomplete due to small supply of possible police candidates.

Appendix B

Additional Background Information Regarding Convict Leasing

Convict leasing is system of penal labor frequently regarded as a continuation of the slavery in the southern sates[24]. It was introduced during the Reconstruction period (1865–1877) when the government of the US were trying to revive economy of the former Confederate states and was intended to replace the labor force once their slaves had been freed. Prisons had right to "lease" convicts to the firms or farms\plantation to work for free (in comparison with non-southern states that payed (miserable) wage to convicts).

Most of the prisoners involved in the convict leasing were black males (Litwack (2010)), thus creating a racial incarceration gap that persists until today. The practice peaked around 1880 and was used to supply labor to farming, railroads, mining, and timber industry. The state of Virginia never imposed convict leasing system, Tennessee was the first state to officially abandon it in 1893 while Alabama was the last one (in 1928). However, convict leasing persisted in various forms until it was abolished for good by Franklin Roosevelt in 1941 (Circular 3591).

The reason why convict leasing lasted for so long was mainly economic: according to Mancini (1996) on average profit from each convict was four times higher then cost of prison administration. In addition to black people, white immigrants also were frequently leased to work for some factories, however in the south, due to discrimination white people mostly worked on less difficult works and were employed inside prisons by contract system.

All convict leasing laws were determined on the state level and had federal

[24] Convict leasing existed in some northern states as well but was less widespread and posses less similarity with slavery (Lichtenstein (1996)).

government had little to do with it. Most of the states allowed several types of the convict labor system, with the contract system to be the most popular. Despite being dispraised by the federal government and public opinion almost everywhere the convict leasing system was imposed in 26 US states at different point of the history[25]. The practice peaked around 1880 and was used to supply labor to farming, railroads, mining, and timber industry. Alabama was the fist state to impose convict leasing system in 1846 and North Carolina was the last state to abolish it in 1933. However, convict leasing persisted in various forms until it was abolished for good by Franklin Roosevelt in 1941.

Such popularity popularity was partly imposed by the fact that it was not only the cheapest way to keep prisoners but even highly profitable way that does not require any effort from the State. Thus, according to Mancini (1996) on average profit from each convict was four times higher then cost of prison administration. Second, convict lease provide opportunity to fill the most dangerous and hard jobs with the work force: e.g., mining, or turpentine production, as prisoners had no choice and had to work in any case. According to the studies of convict leasing (McKay (1942); Taylor (1942); Green (1949); Shelden (1979); McCarthy (1985); Walker (1988); Ledbetter (1993); Lichtenstein (1993); Mancini (1996)) it was always used only for providing workers for mines, factories and plantations that needed unskilled labor. Third, in some, predominantly southern states, there was a shortage of labor that had happened due to the abolishment of slavery after the Civil War, that resulted most of the southern states to adopt convict leasing system. Finally, for the slave states, fear of freed African-Americans that got access to arms after the war caused white population to ask to incarcerate more black people, and as prison system in most of states were not developed and had small capacities, it was easier to lease prisoners out than to build new prisons.

[25]Even in southern states, as can be seen in the "Gone with the Wind" by Margaret Mitchell (1936), where Scarlett's plan to lease convicts to work in the mills was heavily criticized.

Table 2.8: Evolution of Convict Labor: Share of Employed Convicts

System	1886	1895	1905	1914	1923	1932	1940
Convict leasing	20	14	6	3	0	0	0
Contract	30	24	23	16	7	3	0
Piece-price	6	10	5	4	4	6	0
State-account			14	20	16	10	5
State-use	20	24	12	14	22	22	26
Public works and ways			5	7	12	12	13
Not Employed	24	28	35	36	39	47	56

State-account, state-use and public works and ways systems were reported together as public-account system before 1905. Source: U.S. Department of Labor.

While convict leasing is a broad name of the convict labor system, it differ from state to state. Thus, for example, in Colorado and Virginia leased convicts were only allowed to work in mines\quarries and railroad construction correspondingly. In New Mexico, all wardens should have been employed in the states penitentiary system even if they work for a lessee in her private barracks, while in Maryland all prisoner related costs were on prison. Some states allowed to lease only certain type of prisoners: those with misdemeanors in Missouri and only females for work in homes in Massachusetts (I am not sure why somebody wants to have a criminal to cook your food). Finally, Idaho's convict leasing system resembled contemporary private prisons in a way that state leased the whole prison and lessee could employ prisoners wherever she likes.

The question, why some states adopted convict leasing and some did not goes beyond the scope of this paper. Nevertheless, it was a combination of budgetary health, demand for cheap unskilled labor force, bargaining power of small firms that cannot afford a "small prison" and have to rely on free labor and public opinion toward the convict leasing.

Additional Figures and Tables

Table 2.9: Correlates of the Distance to Cincinnati

	I		II	
VARIABLES	Independent variable: Log distance to Cincinnati			
Incarceration rates, black males	-0.054	(-1.564)	-0.042	(-1.313)
Incarceration rates, males	-0.006	(-0.273)	0.023	(-0.658)
Incarceration rates, all	0.005	(-0.176)	0.034	(-0.857)
Number of slaves (1860)	0.013	(-0.383)	0.03	(-0.901)
Share black population	0.279*	(-1.697)	0.272*	(-1.962)
Share foreign-born population	0.18	(-1.558)	0.123	(-1.538)
Share children in school	0.044	(-0.756)	0.051	(-1.236)
Total population	-0.126	(-1.388)	-0.089	(-1.045)
Urban share	0.003	(-0.064)	-0.008	(-0.186)
Mean-to-median farm size	-0.092	(-0.979)	-0.106	(-1.396)
Gini (land)	0.067	(-1.013)	0.051	(-0.942)
Manufacturing output	-0.083	(-0.473)	-0.101	(-0.547)
Agricultural output	-0.116	(-1.579)	-0.134*	(-1.845)
Labor in manufacturing	-0.075	(-1.415)	-0.019	(-0.489)
Value of gold and silver mines output	-0.084	(-1.239)	-0.087	(-1.264)
Value of coal mines output	-0.07	(-0.571)	-0.062	(-0.501)
Value of iron mines output	0.103	(-0.954)	0.112	(-1.045)
Capital-labor ratio	-0.047	(-0.854)	-0.041	(-0.839)
Socioeconomic controls	✓		✓	
Geographic controls	×		✓	

Columns I, and II contain beta coefficient and t-statistics for the regression of log distance to Cincinnati on variables related to incarceration, slavery, demographic, inequality and industrial and agricultural outcomes. For example, row 2 of Column I says, that beta coefficient of the regression of the log of distance to Cincinnati on the incarceration rates of males in 1870 without any controls is -0.015, and t-statistics is equal to -0.728. Similarly, in Column II I add longitude and latitude controls in Column III. As we can see, distance to Cincinnati is correlated with the share of black population and agricultural output, thus I will control for these variables in the IV section. All columns contain constant and state fixed effects. Robust clustered by state standard errors in parentheses. *** p $p < 0.01$, ** p $p < 0.05$, * p $p < 0.1$

Table 2.10: Convict Labor and Incarceration in 1930

	I	II	III	IV	V	VI
	Dependent variable: Incarceration (1930)					
Convict labor (1886)	54.13***					
	(15.38)					
Convict labor (1895)		84.25***				
		(25.28)				
Convict labor (1905)			43.26***			
			(10.37)			
Convict labor (1915)				83.07***		
				(21.13)		
Convict labor (1923)					94.78***	
					(19.49)	
Convict labor (1932)						35.71***
						(7.474)
R-squared	0.27	0.32	0.25	0.28	0.36	0.26
Observations	2,228	2,228	2,228	2,228	2,228	2,228

All columns contain constant and state fixed effects. The following variables are used as controls: ln of total population (1880), urban share (1880), share of Black (1870, and 1880), share of women (1880), share of foreign-born (1880). Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.11: Effect of Convict Labor om Incarceration Rates in 1930

	I	II	III	IV	V	VI
	Dependant Variable: Incarceration Rate (1930)					
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict labor (continuous)	129.8***	86.37*				
(weighted by industry & distance)	(31.39)	(48.22)				
Convict labor (continuous)			20.17	75.40*		
(weighted by distance)			(29.94)	(44.62)		
Convict labor (discrete)					55.71***	67.28
(nonweighted)					(15.93)	(42.42)
R-squared	0.18	0.18	0.13	0.13	0.26	0.25
Kleibergen-Paap F stat		12.31		17.20		6.93
Partial R-squared		0.031		0.112		0.004
Instrument's coefficient		-0.00102***		-0.00108***		-0.00142***
		(0.000289)		(0.000271)		(0.000517)
Observations	2,228	2,228	2,228	2,228	2,228	2,228

All columns contain constant and state fixed effects. The following variables are used as controls: ln of total population (1880), urban share (1880), share of Black (1870, and 1880), share of women (1880), share of foreign-born (1880). Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.12: Convict Labor and Black Incarceration Rates (1930)

	I	II	III	IV
Dependent variable:	Black incarceration rates (1930)			
	OLS	Reduced form	First stage	Second stage
St.dev. convict labor	19	713	713	19
Log of value of goods	23.4			88.43**
produced (1886)	(22.4)			(38.3)
Distance to Cincinnati, km		-2.005*	-0.023***	
		(1.11)	(0.005)	
Partial R^2			0.12	
F stat of ex. instrument			23.32	
Prob > F			0.00	
Anderson-Rubin p-value			0.05	
Observations	2,175	2,175	2,175	2,175
adjR^2	0.032	0.033	0.882	0.03

All columns contain constant, state fixed effects, geographic controls (latitude and longitude, dummies for coastal counties), 1880 and 1870 socio-economic controls (ln of total population, urban share, share of Black, share of women, share of foreign-born). Robust clustered by state standard errors in parentheses. 41 cluster. *** p<0.01, ** p<0.05, * p<0.1

Table 2.13: Convict Labor and Incarceration Rates by Races (1930)

	I	II	III	IV	V	VI	VII	VIII
Dependent variable:	Incarceration rates (1930)							
	Non-white	Non-white & non-black	White foreign-born	White	Non-white	Non-white & non-black	White foreign-born	White
	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
St.dev. explanatory variable	19	19	19	19	19	19	19	19
Log of value of goods	23.110	77.077	26.986	5.359	112.7*	397.4	27.78	15.12**
produced (1886)	(21.226)	(67.661)	(18.684)	(3.679)	(61.52)	(306.7)	(17.64)	(6.249)
Partial R^2					0.12	0.06	0.12	0.12
F stat of excluded instrument					23.32	7.75	23.32	23.32
Prob > F					0.00	0.01	0.00	0.00
Anderson-Rubin p-value					0.02	0.03	0.01	0.00
Observations	2,175	2,175	2,175	2,175	2,175	2,175	2,175	2,175
adjR^2	0.020	0.032	0.034	0.045	0.01	0.01	0.03	0.04

All columns contain constant, state fixed effects, geographic controls (latitude and longitude, dummies for coastal counties), 1880 and 1870 socio-economic controls (ln of total population, urban share, share of Black, share of women, share of foreign-born). Robust clustered by state standard errors in parentheses. 41 cluster. *** p<0.01, ** p<0.05, * p<0.1

Table 2.14: National Prison Association Congress Delegates: Reduced Form

	I	II	III	IV
Outcome:	Incarceration Rate (1920)	Incarceration Rate (1930)	Incarceration Rate (1920)	Incarceration Rate (1930)
Distance to Cincinnati, OH	-80.34***	-103.7*		
(1000 km)	(25.85)	(53.19)		
Delegate at NPA			232.7***	288.9*
			(78.18)	(161.6)
R squared	0.01	0.09	0.18	0.18
Observations	3,109	3,108	1,373	1,373

All columns contain constant. Robust clustered by state standard errors in parentheses. *** p $p < 0.01$, ** p $p < 0.05$, * p $p < 0.1$

Table 2.15: Convict labor and incarceration: Sub-sample analysis

	I	II	III	IV	V	VI	VII	VIII
	Dependent Variable: Incarceration Rate (1920)							
	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Convict Labor (Continuous)	47.91*	74.19***	63.60**	89.44*	36.06***	8.749	54.40**	39.04
(nonweighted)	(25.95)	(27.83)	(30.82)	(49.58)	(12.07)	(30.01)	(23.16)	(34.88)
Kleibergen-Paap F-stat	-	16.0	5.4	4.2	8.6	5.5	7.428	4.9
Sample	Full Sample	Full Sample	w/o North-East	w/o Mid-West	w/o Great Plains & Far West	w/o South	w/o South non-CSA	w/o ex-CSA
Observations	2,185	2,185	1,946	1,503	1,800	1,306	2,017	1,339

All columns contain constant. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.16: Testing for SUTVA

	I	II	III	IV	V	VI	VII	VIII
Outcome:	Incarceration Rate (1920)		Incarceration Rate (1930)		Incarceration Rate (1920)		Incarceration Rate (1930)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Convict labor	44.65***	74.47*	73.91***	123.1*	15.35**	109.1	27.17*	176.6
	(10.21)	(40.99)	(24.39)	(76.80)	(7.002)	(116.6)	(14.10)	(204.5)
R-squared	0.35	0.24	0.31	0.24	0.17	-1.95	0.23	-1.24
Partial R-squared		0.003		0.003		0.001		0.001
Kleibergen-Paap F stat		6.082		6.082		0.780		0.780
Prob > F		0.022		0.022		0.389		0.389
Anderson-Rubin p-value		0.049		0.0504		0.0169		0.0991
Sample	25 states with deligates at NPA				States without deligates at NPA			
Instrument's coefficient		-0.00132**		-0.00132**		-0.00139**		-0.00139**
		(0.000536)		(0.000536)		(0.000571)		(0.000571)
Observations	1,362	1,362	1,362	1,362	833	833	833	833

CHAPTER 3

Convict Labor as a Determinant of Intergenerational Mobility in the United States

3.1 Introduction

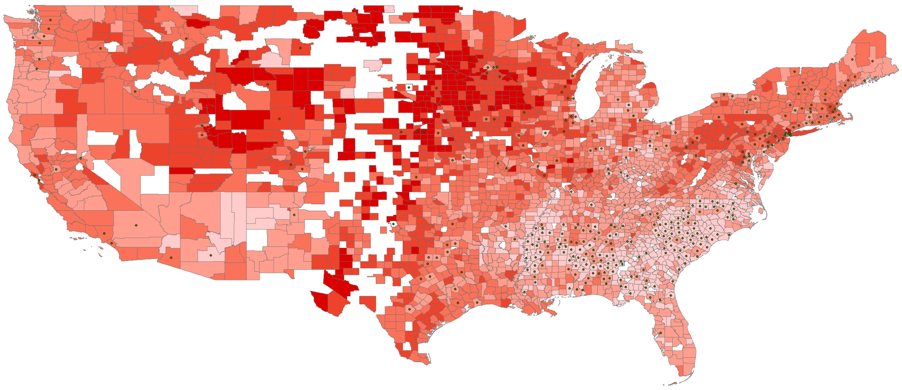
Institutional history has a profound influence on economic development and world inequality as many countries today still live in the shadow of colonial institutions established more than at least a century ago. One of the most damaging of such institutions is forced labor. When elites are able to coerce part of the population to perform unpaid labor, it can cement growth-killing power arrangements, reduce innovation, cripple "creative destruction" and eventually lead to the failures of economic growth. This study is part of the agenda of exploring long-term consequences of the forced labor institutions. While many other papers look at the institutions of developing nations, I explore institutions of forced labor in one of the most developed nations of the world: United States of America. Certainly, it might seem paradoxical to look at the adverse effects of forced labor in a country that rightfully belongs to the club of the richest nations, but one should be aware of very unequal distribution of the benefits of economic development in the US (Oliver and Shapiro (2006); Frank (2009); Chetty et al. (2014a,b)).

Previous studies highlighted the importance of the institutions and differences in the initial factor endowments in explaining the degree of inequality in wealth, human capital, and economic growth (Engerman and Sokoloff (2002, 2005)). Dippel et al. (2015) show that predisposition in sugar suitability determined how coercive institutions evolved due to sugar price changes. Dell (2010) examined the long-run adverse impacts of the forced mining labor system in XVI-XIX century Peru and Bolivia on contemporary health outcomes. Although, the growing literature continue to study the importance of institutions the case of US convict labor system is unique as it allows to show the persistent effect of labor coercion on economic outcomes and distinguish the channel of its effect.

After the Civil War, US states started to impose convict labor laws allowing employing prisoners into productive labor. Those laws varied a lot in terms of profitability for the state and other parties involved, and working conditions of prisoners. E.g., convict leasing, was the most profitable form that allowed states to auction prisoners and bear no costs for keeping them in correctional facilities. Such prisoners were usually employed in hard unpaid labor in mines or plantations and kept in conditions close those of slaves. At the same time, milder forms of convict labor allowed to use prisoners only for creating goods inside the prisons for in-prison consumption. Although, those convicts that were producing goods for sale on the open market had lower wages than free laborers. Those regions that experienced high usage of coerced labor had less fair employment opportunity for the free laborer in terms of wage and labor demand, thus negatively affecting their welfare (Poyker (2018c)).

By using the new dataset of the US convict labor camps in end of XIX - beginning of XX century, I show that convict leasing system has a persistent effect on contemporary economic outcomes. Those counties that experienced more extensive usage of convicts currently have worse intergenerational mobility nowadays. To illustrate this point in Figure 3.1 I show with green dots location of historical prisons, and the county–level intergenerational mobility (from Chetty et al. (2014a)).

Figure 3.1: Intergenerational mobility and historical prisons



Green dot indicates centroid of the county if there was at least one convict labor camp between 1886 and 1940. *Source:* Labor camps: US Department of Labor; Absolute upward mobility: Chetty et al. (2014a).

Given the direct effect of competition with free labor and the effect on incarceration rates, convict labor may have long-run effects on other socioeconomic outcomes. Prison labor makes local low-skilled workers poorer while benefiting owners of capital, and it can worsen intergenerational mobility, making it more likely that the most impoverished will remain poor and that the rich will remain rich. I find that after the convict-labor system was abolished (by the 1940s), wages and labor-force participation converged between more and less affected counties.[1] However, 80 years of wage depression may have cumulative effects on the welfare of low-skilled workers. Using data from Chetty et al. (2014a), I study the long-run effects of the U.S. convict-labor legacy on contemporary social mobility. By using patterns of expansion of convict labor in the 1870s, I show that counties that experienced larger shocks of convict labor had both lower absolute upward mobility (the probability that a child from the bottom of income distribution will end up in the top) and higher relative upward mobility (the slope of the regression of a child's percentile rank on his parents' percentile rank in their income distributions) for the 1980-1982 birth cohort.

[1]Convict labor was reinstated in 1979.

My contribution to the literature lies in the following aspects. First, this is the first economic paper that studies the coercive institutions in the United States. Second, this is the first paper that connects the legacy of coerced labor with contemporary economic outcomes. I find that 1 percent increase in value of goods produced by convicts results in decrease of the probability of person born in lower income quantile to move into the top quantile by 0.14 percentage points. Moreover, in contrast to these recent shocks, I estimate the long-run effects of competition coming from the convict labor system. I provide further evidence of how adapting and evolving firms shaped local economies and equality of opportunity (Chetty et al. (2014a, 2017)).

The paper is organized as follows. The historical background of convict labor in the United States is introduced in Section 3.2. Section 3.3 describes the data. Estimation results and robustness checks are presented in Section 3.4. Section 3.5 concludes.

3.2 Convict Labor and Intergenerational Mobility

Convict labor existed in the United States from early 1820s until now. It become widespread in 1870s and existed in that way until 1941 when it was severely regulated by the federal legislation, before it reappeared after 1979. While convict labor intended to rehabilitate prisoners up to my knowledge, there is no economic paper that causally estimated that effect. While positive effects of convict labor were not proven, Poyker (2018c) shows that counties more exposed to competition with prison-made goods were having lower wages in manufacturing and worse employment opportunities. As convict labor distorted wages of low-skilled workers for almost 70 years it could affect wealth of their families in the long-run. While Poyker (2018c) shows that wages rebounced as soon as convict labor was abolished, it does not mean that their wealth rebounced as well.

At the same time Poyker (2018c) finds that convict labor had positive effect on firm's productivity: patenting in affected industries had increased, and affected firms experienced higher rates of technology adoption in their attempts to evade competition with cheap prison-made goods. Thus while wage-earners were experiencing adverse wealth shock, capital-owners benefited from the convict labor, making the divide between poor and rich wider.

Moreover, Poyker (2018a) find that in addition on the effects on local-labor markets convict labor incentivized police to arrest more people around prisons. Thus in addition to divergence of wealth between wage-earners and capital owners more predominantly poor people end up in prison and where their human and social capital deteriorated.

This two channels may affected economic outcomes of the people living in a proximity of those prisons in a long-run (even if most of those prisons do not exist there anymore). I chose to concentrate on studying intergenerational mobility instead of other measures of inequality as it less depend on the other contemporaneous factors (like gini coefficient would be highly correlated with urban population). At the same time intergenerational mobility provides enough variation within and between rural and urban areas, and is more stable overtime than other inequality measures.

3.3 Data

In this Section, I describe construction of the main dependent and explanatory variables used in the paper.

3.3.1 Convict Labor Data

The primary source of the data for this paper is a set of U.S. Department of Labor reports devoted to convict labor. I collected and digitized seven reports for the following years: 1886, 1895, 1905, 1914, 1923, 1932, and 1940. Then, I matched all prisons and convict labor camps across years by name and assigned a FIPS code and GPS coordinates for each one of them. Overall, the dataset contains 464 different locations with correctional facilities. More detailed information about convict labor data can be found in Poyker (2018c).

As my outcome variables are cross-sectional, my main main explanatory variable is also cross-sectional. I construct three measure of counties exposure to convict labor:

The first is similar to one used in Poyker (2018c), where I weigh the effects of each prison by the distance between it and a given county and by counties industrial composition:

$$CL^1_{c,t} = \sum_{i \in I} \left(\lambda_{i,c} \times \sum_{p \in P_t} \frac{\ln \left(\text{Value of goods produced}_{i,p,t} \right)}{\text{Distance}_{c,p}} \right), \tag{3.1}$$

where P_t is the set of all prisons at year t, $\text{Distance}_{c,p}$ is a distance between prison p and county's c centroid (in km), and $\lambda_{i,c}$ is a value share of industry i in county c in 1870.

Second measure only weighs output of each U.S. prison by the distance from it to the county's centroid:

$$CL^2_{c,t} = \left(\sum_{p \in P_t} \frac{\ln \left(\text{Value of goods produced}_{p,t} \right)}{\text{Distance}_{c,p}} \right). \tag{3.2}$$

The second measure may be more applicable as we do not want to measure effect of convict labor on the local labor market. Even if county is not affected by the competition with prison-made goods because its industrial composition is different incarceration may be still affected as local police will may be incentivized to increase number of employed convicts in a nearby prison. Hereafter I refer to the first two measures of convict labor as “continuous” as they treat all counties.

The third measure is constructed as the value of goods produced in a county, and thus assume only those counties as treated if they had a prison (hereafter I refer to this measure of convict labor as “discrete”):

$$CL^{3}_{c,t} = \ln\left(\sum_{p\in P_{c,t}} \big(\text{Value of goods produces}_{i,p,t}\big)\right), \tag{3.3}$$

The underling assumption here is that wardens are more capable to incentivize police and judges nearby, and at the same time, the demand for convict labor is higher around the prison. I prefer this measure and use it as a baseline hereafter because it may be easier for prison wardens to incentivize local police than police in a counties farther away.

3.3.2 Intergenerational Mobility Data

Data on the U.S. intergenerational mobility comes from the seminal work by Chetty et al. (2014a). In particular, I use two measures of intergenerational mobility: absolute upward mobility and relative upward mobility.

Absolute upward mobility is the expected rank of children from families at any given percentile $p = 0.25$ of the national parent income distribution. It can be interpreted as an “American dream” as it represents probability to achieve financial success while being initially poor.

The second measure is the relative upward mobility. It is the difference in outcomes between children from top- vs. bottom-income families within the county. In other words it is a slope of the regression of farther’s rank in income distribution on son's rank.

3.4 Results

3.4.1 Convict Labor as a Correlate of Intergenerational Mobility

In this section, I study long-run effects of convict labor. It had adverse effects on wages in manufacturing and employment, yet it boosted the economy through growth in capital and number of patents. Convict labor discouraged wage-earners and benefited capital owners for decades. Even when wages and employment leveled-up after private systems were abolished, the accumulated changes in welfare persisted.

Because the welfare of low-skilled workers was adversely affected, I expect to see worse intergenerational mobility for the poorest population.[2] In particular, according to Chetty et al. (2014a), counties exposed to convict labor should have lower absolute upward mobility: "The mean rank (in the national child income distribution) of children whose parents are at the 25th percentile of the national parent income distribution." Conversely, high-skilled workers and capital owners benefited from convict labor, while the poor remained poor and their welfare has persisted across generations. Thus, I expect relative upward mobility, or the "rank slope," to be higher. Relative upward mobility represents the slope of the regression of the child's percentile rank on his parents' percentile rank in their income distributions; it explains how person A will likely remain rich if her father is rich while person B will likely remain poor if her father is poor.

I test the hypothesis that exposure to convict-made goods in the past had long-run effects on intergenerational mobility. Unfortunately, the first county-level

[2] I prefer to use intergenerational mobility over other forms of income inequality because I want to describe the equality of opportunities of people in different income quintiles across generations.

cross-sectional data that is available is for the 1980-1982 birth cohort (Chetty et al. (2014a)). Thus I consider cross-sectional regression of convict labor on contemporaneous intergenerational mobility:

$$y_{c,1980/82} = \alpha + \beta CL_c + \Gamma \mathbb{X}_c + \mu_s + \varepsilon_c, \tag{3.4}$$

where now I can use only state fixed effects (μ_s) to control for time-invariant unobservables. Contemporary controls may be affected by the explanatory variable. Thus I use historical controls and estimate a reduced form of the relationship between convict labor and intergenerational mobility, but I cannot directly pinpoint the mechanism. I use total population and urban share in 1880 which should be proxies for intergenerational mobility at that time, and because prisons appeared in places with a higher population and urban share. Because counties with a high share of Black and foreign-born population in the past may affect contemporaneous intergenerational mobility and convict labor outcomes, I control for the shares of Black and foreign-born population. Also, I control for slave population in 1860 to alleviate the concern that racial attitude toward African-Americans affected both contemporary intergenerational mobility and convict labor (especially under the convict leasing system) in 1886 (Sellin (1976); Stewart (1998); Soares et al. (2012)). I control for county tax revenues as a proxy for the county's wealth which can affect both the outcome and prison production. I add market access from Donaldson and Hornbeck (2016) and geographical controls from Fishback et al. (2007), such as dummies for coastal regions, lakes, latitude, longitude, average temperature, and land area. Similarly, standard errors are clustered at the state level.

Because contemporary prison locations are different from pre-1940 locations, and because the convict labor system was reestablished at the federal level only in

1979, I assume that the new convict labor wave did not confound my outcomes.[3]

In Column I of Table 3.1 I start my analysis by replicating Column VIII of Table VI in Chetty et al. (2014a). This specification serves the purpose of providing the reference point, of how much of the variation in intergenerational mobility is explained by the most important contemporary covariates. In Column II I regress absolute upward mobility of 1980/82 birth cohort on the value of prison-made goods produced in that commuting zone in 1940. One standard deviation in value of convict labor output decreases absolute upward mobility 6.5% of its standard deviation. In Column III, I add controls, used in Chetty et al. (2014a), as well as control historical controls. The resulting estimate remains significant, while now its standard deviation only explains 2.3% of standard deviation in absolute upward mobility. While it explains much less than share of Black, share of single mothers, or high school dropout rate, it is better predictor of intergenerational mobility than Gini bottom 99% or social capital index. In Columns IV and V I estimate similar specifications but with relative upward mobility as a dependent variable. Column V shows, that one standard deviation in value of convict labor output decreases relative upward mobility 3.8% of its standard deviation.

In Columns VI, VII, and VIII I experiment by using various measures of exposure to convict labor, as those commuting zones were subject to treatment for almost a 70 years of convict labor. In Column VI I use the sum of all value goods produced over all available years, and in Column VII I use weighting, by assigning higher weight to the recent years (1940), and smaller weights for the oldest (1886). Finally, In Column VIII I use first principle component off value of prison-made goods produced during each of the time period. All three column yield coefficients of similar size. Finally, in Column IX I interact convict labor with other covariates, chosen in Chetty et al. (2014a). While, exposure to convict labor has heterogeneous effects on intergenerational mobility, the estimate remain signifi-

[3] States could choose a discretion whether to adopt it. Only 31 did.

cant, and even slightly increases in magnitude. This findings suggest, that the legacy of convict labor by itself is an important correlate of the intergenerational mobility.

Table 3.1: Convict Labor as a Correlate of Intergenerational Mobility

	I	II	III	IV	V	VI	VII	VIII	IX
Dependent Variable:	Absolute Upward Mobility			Relative Upward Mobility		Absolute Upward Mobility			
Value of Convict Labor Output (1940)		-0.0646**	-0.0234*	0.0576***	0.0380**				
		(0.0242)	(0.0128)	(0.0155)	(0.0152)				
Total Value of Convict Labor Output (1886-1940)						-0.0289**			
						(0.0124)			
Total Value of Convict Labor Output (1886-1940), weigthed							-0.0286**		
							(0.0122)		
Value of Convict Labor Output (1886-1940), First Principle Component								-0.0229**	-0.0797**
								(0.0106)	(0.0318)
Fraction Short Commute	0.250***		0.256***		-0.0628	0.256***	0.255***	0.258***	0.237***
	(0.0763)		(0.0832)		(0.0621)	(0.0827)	(0.0829)	(0.0823)	(0.0852)
Gini Bottom 99%	-0.0143		-0.0231		-0.0267	-0.0219	-0.0222	-0.0222	-0.0187
	(0.0437)		(0.0439)		(0.0801)	(0.0433)	(0.0434)	(0.0433)	(0.0426)
High School Dropout Rate	-0.0794**		-0.0812**		0.00705	-0.0798**	-0.0799**	-0.0801**	-0.0871**
	(0.0372)		(0.0370)		(0.0456)	(0.0368)	(0.0368)	(0.0369)	(0.0363)
Social Capital Index	0.0475		0.0492		0.146**	0.0521	0.0520	0.0508	0.0433
	(0.0461)		(0.0464)		(0.0707)	(0.0467)	(0.0468)	(0.0465)	(0.0440)
Fraction Single Mothers	-0.636***		-0.632***		0.488***	-0.631***	-0.631***	-0.631***	-0.624***
	(0.0706)		(0.0718)		(0.0610)	(0.0715)	(0.0715)	(0.0715)	(0.0719)
Fraction Black	0.254***		0.257***		0.0858	0.261***	0.262***	0.258***	0.263***
	(0.0722)		(0.0787)		(0.0797)	(0.0798)	(0.0796)	(0.0794)	(0.0772)
Convict Labor x Fraction Black									-0.0711***
									(0.0258)
Convict Labor x Fraction Short Commute									-0.0868***
									(0.0290)
Convict Labor x Gini Bottom 99%									0.00588
									(0.0227)
Convict Labor x High School Dropout Rate									-0.0628***
									(0.0191)
Convict Labor x Social Capital Index									-0.0130
									(0.0243)
Convict Labor x Fraction Single Mothers									0.0822***
									(0.0257)
R-squared	0.869	0.665	0.870	0.571	0.690	0.871	0.871	0.870	0.873
Observations	709	709	709	709	709	709	709	709	709

Each column reports coefficients from an OLS regression with the specification from Column VIII of Table VI in with state fixed effects. The regressions are run using data for the 709 commuting zones with at least 250 children in the core sample. The dependent variable in Columns I-III and VI-IX is absolute upward mobility, the expected rank of children whose parents are at the 25th national percentile. The dependent variable in Columns IV and V is relative mobility, the rank-rank slope within each commuting zone. I use value of goods produced in prisons in 1940 in Columns I-V, total value of goods produced in prisons in (1886, 1895, 1905, 1914, 1923, 1932, and 1940) (in 1940 dollars) in Column VI, total value of goods produced in prisons weighted by year in Column VII, and first principle component of the value of goods produced in prisons in (1886, 1895, 1905, 1914, 1923, 1932, and 1940) (in 1940 dollars) in columns VIII and IX. All independent and dependent variables are normalized (in the relevant estimation sample) to have mean 0 and standard deviation 1. I use data from generously made available on Raj Chetty's website. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

3.4.2 Suggestive Causal Relationship

To estimate reduced-form effect of convict labor on intergenerational mobility I use an extensive set of controls, but endogeneity concerns remain. For example, if prison-made goods light up unionization, contemporaneous absolute upward mobility will increase and I will underestimate the convict labor effect. But if convict labor appeared in places with lower unionization, I will overestimate its effect. Thus I use the same cross-sectional IV specification as in Section with distance to Cincinnati as an instrument.

I present the main long-run effect of convict labor on absolute upward mobility in Panel A of Table 3.2. Columns I and II show results for the effect of baseline continuous measure of convict labor on absolute upward mobility. The OLS estimate yields a zero coefficient while the, IV estimate solved upward bias and resulted in a negative and significant coefficient. One standard deviation in exposure to convict labor output in 1886 decreases the probability of child from a bottom half of the national income distribution to end up in its top 2.7%. Results hardly change when I use exposure to convict labor without industry weighting. As in the long-run specification, I use a cross-sectional instrument, which explains the actual volume of prison-made goods; I expect the first stage to be stronger if I do not weight convict labor output by industry. Indeed, both the F-statistics of the excluded instrument and the partial R^2 increase, but the IV coefficient decreases somewhat. Finally, I use the discrete measure in Columns V and VI. The resulting coefficient shows robust negative sign and magnitude of the effect.[4]

Similarly, I report results for the relative upward mobility in Panel B. The OLS estimate shows zero effect, while the IV estimate is more meaningful: one standard deviation in exposure to convict labor output in 1886 increases the farther-son in-

[4]The first-stage F-statistics is equal to 7.8, but they remain within the thresholds determined by Stock and Yogo (2005), and the first stage is not weak at the 99% level; the Anderson-Rubin χ^2 test is satisfied at the 95% level.

Table 3.2: Convict Labor and Intergenerational Mobility

	I	II	III	IV	V	VI
Dependent Variable:	Absloute Upward Mobility		Relative Upward Mobility		Absloute Upward Mobility	
Sample	Full Sample				NPA	w/o NPA
	OLS	2SLS	OLS	2SLS	2SLS	2SLS
Convict Labor, 1886 (Discrete)	-0.05*	-0.58*	0.0013**	0.0099***	-0.67***	0.30
	(0.027)	(0.304)	(0.0002)	(0.0036)	(0.245)	(0.880)
R-squared	0.69	0.57	0.48	0.31	0.49	0.76
Kleibergen-Paap F stat		16.5		16.5	21.5	2.6
Instrument's coefficient		-0.00477***		-0.00477***	-0.00633***	0.00230*
		(0.00117)		(0.00117)	(0.00119)	(0.150)
# States	41				24	17
Observations	2,311	2,311	2,311	2,311	8,755	8,755

All values of exposure to convict labor are normalized. All columns contain constant and state fixed effects. The following variables are used as controls: ln of total population (1880), urban share (1880), share of Black (1870 and 1880), share of women (1880), share of foreign-born (1880), log of market access (1870), and log of number of slaves (1860). Columns with second stage include first stage coefficient of instrument on the explanatory variable. Robust clustered by state standard errors in parentheses. 41 clusters. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

come regression slope by 0.032. The coefficient remains stable if I use an other measure of convict labor exposure in Columns IV and VI. I conclude that convict labor improved the intergenerational mobility of the non-poorest populations but reduced the chances of the poorest income quantile attaining the "American dream."

In Table A3.5, I also show that results for both relative and absolute upward mobility are not driven by any subsample of states.

In this section I showed that the presence of convict labor in 1886 affected future intergenerational mobility. However, divergence of wealth between low-skilled wage earners and capital owners may not be the only channel through which this effect emerged. In particular, convict labor by itself caused an increase in crime rates and incarceration, hindering long-run human capital accumulation and intergenerational mobility. My findings suggest that while divergence of welfare of wage earners and capital owners was a mechanism in (mostly) Northern states, the incarceration channel was an important mechanism affecting intergenerational mobility in the South.

3.4.3 Convict Labor and Long-Run Outcomes

In this section, I briefly address the most important robustness and sensitivity checks for the long-run analysis.

I use the same instrument as in Poyker (2018a) where I show in grate details that the distance to Cincinnati is a legitimate instrument for the expanse of convict labor in its first years and not violating exclusion restrictions.

Distance to Cincinnati is uncorrelated to relevant socioeconomic factors back in 1870 (See Poyker (2018a) for details)). The only two variables that are correlated with the distance to Cincinnati are the share of black population and market access thus I control for these variables in all specifications. These should ensure that distance to Cincinnati does not affect incarceration or intergenerational mobility through some other variable.

Exclusion restriction can be violated if the distance to Cincinnati is correlated to trade or migration patterns that took place after 1870 and before the realization of intergenerational outcomes. In this case, even if the instrument is not associated to important socioeconomic variables in 1870, it still accumulates other effects that had happened during the century. One way to address this point is to show, that distance to Cincinnati, not just by accident a good correlate of convict labor, Poyker (2018a) presents first stage F-statistics of the first stage regressions with all possible distances to county centroid. This simple placebo test shows that geographic proximity to Cincinnati, yield one of the largest F-statistics among of all placebo tests substituting proximity to all other counties.[5] Thus the effects I measure is specific to geographic proximity to Cincinnati and not to a post-1886 condition affecting the United States overall.

[5]In fact, the second largest, with the largest in Columbus, Ohio.

Table 3.3: Convict labor and intergenerational mobility: Reduced form

	I	II	III	IV
Outcome:	Absolute Upward Mobility (1982-1984)	Relative Upward Mobility (1982-1984)	Absolute Upward Mobility (1982-1984)	Relative Upward Mobility (1982-1984)
Distance to Cincinnati, OH	2.779***	-0.0330**		
(1000 km)	(0.917)	(0.0153)		
Delegate at NPA			-0.971***	0.0105**
			(0.309)	(0.00452)
R squared	0.17	0.18	0.18	0.43
Observations	2,265	2,265	1,372	2,266

All columns contain constant. Robust clustered by state standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1

In Table 3.3I also demonstrate, that distance to Cincinnati is indeed a good predictor of having a participant from a given county.

To confirm, that having a delegate indeed increases convict labor output in Columns VIII and IX I show the correlation between dummy for having a representative in a county and county's convict labor output in 1886 for full sample and sample of 25 participating states. The coefficient it virtually the same for two columns, and is positive and significant. As an additional check, I add distance to Cincinnati as a correlate: it barely affects the coefficient of the delegate dummy and is insignificant by itself. This is another evidence, that distance to Cincinnati by itself does not affect convict labor, but through the Congress delegates. Similarly, NPA delegates predict the prevalence of convict labor in 1895 and beyond, while the correlation is becoming weaker over time.At the same time, even within-state distance to Cincinnati is a robust predictor even controlling for latitude and longitude of the convict labor in 1886; the relationship is fading away if I use 1895 levels as convict labor become more widespread and less dependent on which warden visited NPA meeting. The correlation completely disappears when I use 1905 convict labor output.These results suggest, that distance to Cincinnati is not correlated with some county specific characteristics that also relate with convict labor but only explain the expanse of the convict labor in its first decades suggesting, that it is because of the NPA's information treatment. Finally, I present reduced form results with the number of NPA Congress participants in Table A3.3.

In the case of NPA congress, IV assumptions can be regarded as SUTVA, as wardens living close to Cincinnati who came at the conference were "compliers" and we would expect the instrument to affect only through them (Angrist et al. (1996)). Thus I used he fact, which only 25 states sent their delegates, and in Panel A of Table 3.4, I show that the instrument affects outcomes only in a sub-sample of the twenty-five states that sent delegates to the NPA Congress. Indeed, the size of IV coefficients is statistically insignificant from those on the full sample. At the same time, the same specification on a sub-sample of the states without delegates in Panel B yield week first stage F-statistics below one suggesting a relationship between the instrument and convict labor.

Table 3.4: Testing for SUTVA

	I	II	III	IV	V	VI	VII	VIII
Outcome:	Absolute Upward Mobility (1982-1984)		Relative Upward Mobility (1982-1984)		Absolute Upward Mobility (1982-1984)		Relative Upward Mobility (1982-1984)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Convict labor	-0.0185	-2.527**	0.000126	0.0278**	-0.0620*	-0.000422	-0.00106***	0.00842
	(0.0323)	(1.148)	(0.000394)	(0.0134)	(0.0316)	(2.827)	(0.000345)	(0.0612)
R-squared	0.67	-0.53	0.36	-0.64	0.75	0.75	0.52	0.44
Partial R-squared		0.004		0.004		0.001		0.001
Kleibergen-Paap F stat		5.933		5.933		0.676		0.676
Prob > F		0.023		0.023		0.422		0.422
Anderson-Rubin p-value		0.0001		0.0008		0.9999		0.8816
Sample	25 states with deligates at NPA				States without deligates at NPA			
Instrument's coefficient		-0.00139**		-0.00139**		-0.000670		-0.000670
		(0.000571)		(0.000571)		(0.000815)		(0.000815)
Observations	1,304	1,304	1,304	1,304	789	789	789	789

All columns contain constant. Robust clustered by state standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In Table 3.5 I examine if results are driven by some sub-sample of states. First, in Columns, I report the baseline specification from Table 3.2. In Column II I omit North-Eastern states. All coefficients but relative absolute mobility remain significant (p-value= 0.16). The size of the coefficient does not change much, and I attribute this marginal insignificance to low sample size and marginally week first stage. For Columns II-VII the first stage F-statistics ranges between 5 and 8, that still passes week instrument test on 95% level. Then in Column III, I exclude southern non-Confederate states. All coefficients are significant and remain stable. However, if in Column IV I exclude Confederate states, the effect of convict labor on incarceration vanishes. Then, in Column V I drop Midwestern states. The resulting coefficients differ from those with full sample only in size of standard errors. Finally, in Column VI I drop both Great Plains and Far West states. A few sent their delegates to the NPA Congress anyway, and I do not expect much changes upon exclusion of these states: and indeed, results are as expected are very similar, although incarceration effect is smaller in magnitude than in the full sample.

Finally, in a vein of Conley et al. (2012) I relax the exogeneity assumptions of the instruments and examine the bounds we can place on the actual effect of convict labor on incarceration and intergenerational mobility. However, even allowing for imperfect exogeneity, all the effects of convict labor are confirmed, since the direct effect of the distance to Cincinnati on the outcomes should be a least as high as the effect of their biggest correlate - population at 1880 to explain away the IV coefficient.

Table 3.5: Convict labor and intergenerational mobility: Sub-sample analysis

	I	II	III	IV	V	VI	VII
Outcome:	Explanatory variable: Log value of convict labor output						
Absolute Upward Mobility	-2.552***	-2.185**	-1.694**	-1.626	-1.003*	-2.024***	-2.352***
(1982-1984)	(0.982)	(0.939)	(0.680)	(1.081)	-0.592	(0.690)	(0.850)
Relative Upward Mobility	0.0303*	0.0214	0.0211*	0.0308*	0.0328**	0.0232**	0.0215**
(1982-1984)	(0.0169)	(0.0152)	(0.0119)	(0.0175)	(0.0161)	(0.0107)	(0.0107)
# States	41	29	30	39	30	33	38
Sample	Full Sample	w/o North-East	w/o South non-CSA	w/o ex-CSA	w/o South	w/o Mid-West	w/o Great Plains & Far West

All columns contain constant. Robust clustered by state standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1

3.5 Concluding Remarks

In this paper, I investigate long-run consequences of convict labor on Intergenerational Mobility (reviewed by Black and Devereux (2011) and Solon (1999)). Using data from the seminal works by Chetty et al. (2014a,b), I show that underlying changes in welfare distribution among wage earners and capital owners exacerbated local inequality of opportunities. I find that exposure to convict labor in 1886 explains a significant portion in spatial variation in intergenerational mobility in the 1980s. In such exposed counties, absolute upward mobility (the expected rank of children from families at any given percentile $p = 0.25$ of the national parent income distribution) is lower, while relative upward mobility (the difference in outcomes between children from top- vs. bottom-income families within the county) is higher. This finding contributes to the discussion of neighborhood effects on intergenerational mobility (Jencks and Mayer (1990), and Sampson et al. (2002)) and is in line with Chetty and Hendren (2014), who found that much of the spatial variation in intergenerational mobility is driven by place. Overall, this example of welfare redistribution supports the views of Karl Marx, as described in "Das Kapital" and "Manifest der Kommunistischen Partei."

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