

NBER WORKING PAPER SERIES

COERCIVE CONTRACT ENFORCEMENT:
LAW AND THE LABOR MARKET IN 19TH CENTURY INDUSTRIAL BRITAIN

Suresh Naidu
Noam Yuchtman

Working Paper 17051
<http://www.nber.org/papers/w17051>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2011

We thank Ryan Bubb, Davide Cantoni, Greg Clark, Melissa Dell, Oeindrila Dube, Stan Engerman, James Fenske, Camilo Garcia, Claudia Goldin, Larry Katz, Peter Lindert, and James Robinson. Research was funded by the Harvard Academy for International and Area studies, Columbia University/SIPA and Berkeley Haas Business School. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2011 by Suresh Naidu and Noam Yuchtman. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Coercive Contract Enforcement: Law and the Labor Market in 19th Century Industrial Britain
Suresh Naidu and Noam Yuchtman
NBER Working Paper No. 17051
May 2011
JEL No. J41,K31,N13,N43

ABSTRACT

British Master and Servant law made employee contract breach a criminal offense until 1875. We develop a contracting model generating equilibrium contract breach and prosecutions, then exploit exogenous changes in output prices to examine the effects of labor demand shocks on prosecutions. Positive shocks in the textile, iron, and coal industries increased prosecutions. Following the abolition of criminal sanctions, wages differentially rose in counties that had experienced more prosecutions, and wages responded more to labor demand shocks. Coercive contract enforcement was applied in industrial Britain; restricted mobility allowed workers to commit to risk-sharing contracts with lower, but less volatile, wages.

Suresh Naidu
Columbia University
School of International and Public Affairs
MC 3328
420 West 118th Street
New York, NY 10027
and NBER
sn2430@columbia.edu

Noam Yuchtman
Haas School of Business
University of California, Berkeley
Berkeley, CA 94720
and NBER
yuchtman@haas.berkeley.edu

Economists and economic historians often draw a bright line between free and forced labor. Forced labor is typically studied in the context of agricultural, preindustrial economies; free labor is seen as a crucial component of economic modernization and development, and is implicitly assumed in contemporary models of labor markets. However, “intermediate” labor market institutions – between free and forced labor – have been common throughout history.

Indeed, one sees shades of coercion in the world’s first industrial economy, in 19th century Britain. Until 1875, when it was repealed, Master and Servant law gave employers the ability to criminally (as opposed to civilly) prosecute and severely punish a majority of employees across industries for breach of contract in Great Britain.¹ Nor was this law left to rot in the books: there were over 10,000 Master and Servant prosecutions per year between 1858 and 1875 – more prosecutions than for petty larceny – and these occurred across Britain, especially in industrial northern England (see Figure 1, panels A and B).²

Our work theoretically and empirically studies the effects of Master and Servant law on contracting and wages in 19th century Britain. Guided by a model of contractual risk-sharing with limited commitment, which generates equilibrium contract breach and criminal prosecutions, this paper examines the economic causes and consequences of criminal prosecutions under Master and Servant law. We use a panel dataset on prosecutions of workers in English and Welsh districts, and exogenous, sector-specific labor demand shocks, to estimate the response of prosecutions for breach of contract to changing labor demand.³ We find that criminal prosecution of workers, rather than being a vestige of medieval common law, was actively used in the leading industrial sectors of 19th century Britain. In addition, we examine the effect of the repeal of criminal prosecutions in 1875. We find that wages in counties with high levels of prosecutions per capita rose faster after repeal than wages in other counties, and that wages were more responsive to labor demand shocks following repeal, consistent with a shift away from long-term, risk-sharing contracts after penal sanctions were abolished.

A large literature has associated the legal institutions underlying a labor market with the responses of employers and employees to labor market shocks (e.g., Botero et al., 2004, and Caballero et al., 2004). In contemporary common-law labor markets, especially in the United States, employment relations are typically characterized as “employment at will,”

¹Master and Servant law covered employees in a wide variety of sectors, from farm workers, to coal miners, to textile mill workers, to shoemakers, and beyond (although white collar workers and managers were excluded). See Appendix 1A, and especially Table A1. The term “Servant” thus had far broader application in the law than in contemporary parlance.

²Statistics come from *Judicial Statistics, England and Wales*. To place these prosecution figures into context, *Judicial Statistics, England and Wales* reports 14,353 Master and Servant cases and 11,986 cases of larceny of less than 5 shillings in 1875.

³The districts are more disaggregated than British counties. Our dataset contains 52 English and Welsh counties and a total of 219 districts.

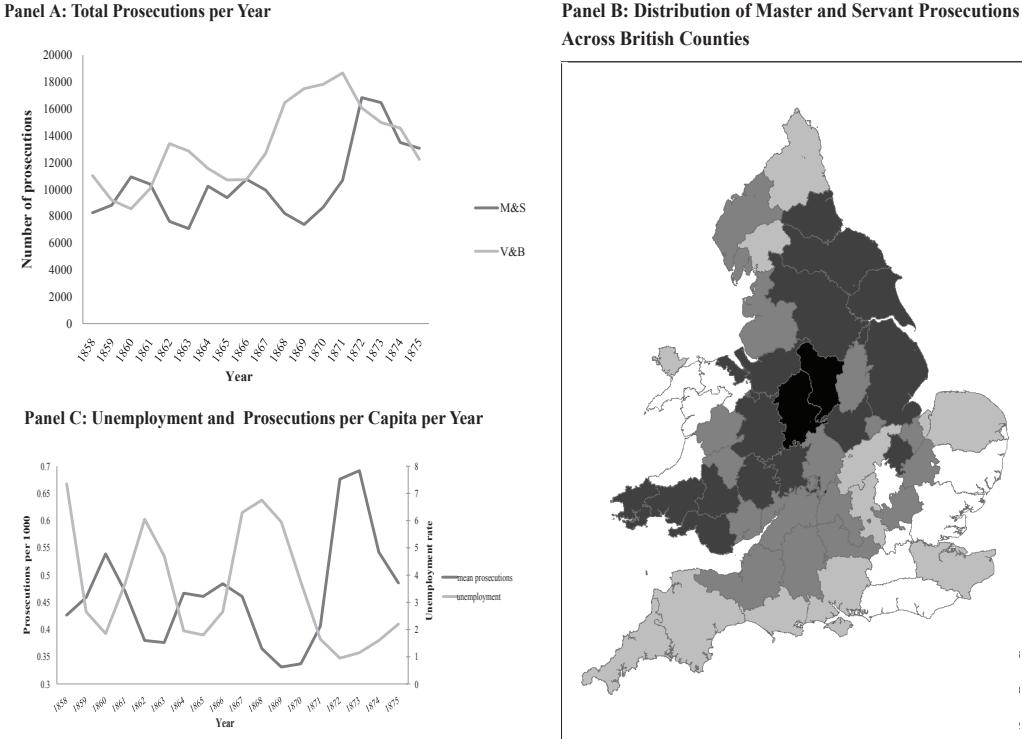


Figure 1: Panel A, on the top left, shows the total number of Master and Servant prosecutions per year, with the number of vagrancy and begging prosecutions also plotted. Panel B, on the right, shows the average number of Master and Servant prosecutions per 1,000 inhabitants of each county, per year, over the period 1858–1875. Panel C, on the bottom left, shows the average number of Master and Servant prosecutions per 1,000 inhabitants of each county, across England and Wales, for each year over the period 1858–1875; this is plotted alongside the unemployment rate by year, over the period 1858–1875. The sources are *Judicial Statistics, England and Wales* (Panels A, B, and C) and the Beveridge unemployment series reported in Steinfeld (2001) (Panel C).

and contracts can be exited by employer or employee without criminal sanctions.⁴ In this context it is natural to expect prices and quantities to adjust quickly to changes in underlying fundamentals, as in Blanchard and Katz (1992); however, various types of labor market regulation can alter the response of wages and employment to shocks at both the micro and macro levels (Blanchard and Wolfers, 2000). In this paper we demonstrate, both theoretically and empirically, that when contract breach is penalized with criminal sanctions, labor demand shocks need not be directly reflected in wages paid. Instead, employers can respond to potential contract breach by threatening to criminally prosecute employees, rather than renegotiating wages, as they do in models of implicit contracting in the absence of employee commitment (e.g., Harris and Holmstrom, 1982, and Beaudry and DiNardo, 1991).

⁴Malcomson (1997) has argued that employment in contemporary Britain is not truly “at will.” However, the legal penalties for contract breach, especially against employees, in Britain today are limited and are far from the criminal sanctions of the 19th century. There exist financial penalties for early termination of labor contracts, for both employees and (more often) employers, in U.S. and U.K. labor markets. Non-compete clauses in contracts, direct descendants of the Master and Servant laws that we study, prevent employees from moving to competitor firms (see Marx et al., 2007).

Economic historians and development economists have long studied legal restrictions on labor mobility. The overwhelming focus of the literature has been agricultural (see Bobonis and Morrow (2010) and Naidu (2010) for recent empirical examples).⁵ Economic historians have focused on agricultural slavery in the U.S. South (Fogel and Engerman, 1974, and Wright, 2006) and serfdom (Brenner, 1976, Domar and Machina, 1984), while development economists have studied bonded labor in contemporary agricultural settings (Bardhan, 1983, Sadoulet, 1992, and Mukherjee and Ray, 1995). However, the use of legal restrictions on labor mobility in modern, industrial labor markets has received little scholarly attention.

Perhaps a reason for this gap in the literature is the common belief that free, unconstrained labor markets are prerequisites for industrial development (Marx, 1877). However, studies by Steinfeld (1991, 2001), Steinberg (2003), and Hay and Craven (2004) argue that labor market “coercion” – the criminal prosecution of workers for breach of contract, with punishments including imprisonment, forced labor, whipping and orders of specific performance – was commonplace in Victorian British industry.⁶ Figure 1, panel B, suggests that criminal prosecutions were widely applied across 19th century Britain. Historical evidence of the importance of criminal prosecutions under Master and Servant law can be seen in the attention paid to them by Parliament: Parliamentary Commissions issued reports on Master and Servant law in 1865, 1866, 1874, and 1875.⁷ Steinfeld (2001) argues that employers prosecuted workers more often in response to tight labor markets. Following Steinfeld (2001, p. 77), we examine the time series relationship between the average number of Master and Servant prosecutions and the national unemployment rate. These two series can be compared in Figure 1, panel C, and the results are quite suggestive: prosecutions and the unemployment rate move in opposite directions throughout the period for which we have data.

Our theoretical analysis of contracting in the shadow of Master and Servant law and our empirical tests will more rigorously examine the relationship between economic conditions and prosecutions. The model and empirical results suggest that Master and Servant law allowed workers to insure themselves against labor market risk by allowing them to credibly commit to stay with an employer despite a higher outside wage; when employees did breach their contracts in hope of higher wages, employers used prosecution to retain labor. The

⁵An exception is the work of Goldin (1976), who studies urban slavery in the American South.

⁶What is meant here by “coercion” is *ex post* coercion of an employee to remain in a contract, not *ex ante* coercion to enter service. This sort of coercion can be welfare-improving for both employers and employees, as it allows employees to commit to long-term contracts, which may be highly valued. von Lilienfeld-Toal and Mookherjee (2010) show that while bonded labor has partial equilibrium benefits for credit-constrained agents (the ability to commit), there may be general equilibrium costs to higher bond limits (changed terms in agents’ relationships with their principals).

⁷That the law of Master and Servant fundamentally shaped relationships within firms was seen by Coase (1937, p. 403), who wrote, “We can best approach the question of what constitutes a firm in practice by considering the legal relationship normally called that of ‘master and servant.’”

elimination of penal sanctions for breach of contract in 1875 was associated with shorter contracts and higher, but more volatile, wages.

In what follows, we discuss labor law in Victorian Britain in Section 1. We present a model of contracting, contract breach, and prosecution in Section 2. In Section 3, we estimate empirical models motivated by the theory, examining the economic determinants prosecution under Master and Servant law, and the economic outcomes associated with the elimination of penal sanctions for breach of labor market contracts in 1875. In Section 4, we summarize our findings and conclude.

1 Master and Servant Law in Victorian Britain

Labor market coercion in Britain (both *ex ante* and *ex post*) was first codified in the 1351 Statute of Laborers, following the demographic shock of the Black Death in 1348.⁸ Yet Victorian labor law was not merely carried-forward ancient law: between the enactment of the Statute of Laborers and the abolition of penal sanctions in 1875, criminal prosecution of British workers for breaching their contracts had been reaffirmed many times over, and was even extended to cover new categories of employees (Appendix 1A, in particular Table A1, provides a historical overview of the enactment of Master and Law).⁹

Most notably, the 1823 Master and Servant Act used “broad language that could be read to cover the overwhelming majority of manual wage workers,” and allowed British employers to “have their workmen sent to the house of correction and held at hard labor for up to three months for breaches of their labor agreements.”¹⁰ Because of its broad scope and harsh consequences, the 1823 Act was an effective and widely used means of punishment for breach of labor contracts. In 1867, a reformed Master and Servant Act changed the punishment for breach, from immediate imprisonment to criminal fines. However, employees continued to face orders to return to their employers (i.e., “specific performance”), and those who could not pay their fines or resisted returning to their employer still faced the threat of jail.¹¹

⁸For histories of British labor law, and the Master and Servant laws in particular, see Steinfeld (2001) and Hay (2004). Contemporary discussions of Master and Servant law include Macdonald (1868) and Holdsworth (1873).

⁹Master and Servant acts were eventually transplanted throughout the Empire, and affected employers and employees around the world. See, Botero et al. (2004) for a discussion of the transplanting of British legal institutions, and its legacy for labor market regulation.

¹⁰Steinfeld (2001), pp. 47-48. Hard labor included work at the treadmill and the crank; whipping was also occasionally used as a punishment. The 1823 Master and Servant Act is 4 Geo. IV c. 34.

¹¹In our empirical analysis of the economic determinants of prosecutions under Master and Servant law (Section 3), we pool prosecutions throughout the 1858-1875 period. In Appendix 2, Table A4, we find that prosecutions responded similarly to economic shocks before and after the 1867 reform.

1.1 Enforcement of Master and Servant Law

In Victorian times, until 1875, the 1823 Master and Servant Act (and its revision in 1867) governed the relationship between employers and employees who were bound by a legal contract.¹² Steinfeld (2001, p. 50) describes the legal procedure through which workers were prosecuted: “A typical case would begin with an employer filing a complaint against a worker. The worker would be arrested . . . and brought before a justice of the peace. There, a settlement would be arranged. The justice would threaten the worker with penal confinement if he refused to return to his employer, and the worker would usually agree to go back.”

Master and Servant law could have been used to incentivize workers to serve out long-term contracts, to incentivize worker effort¹³, or to punish workers for organizing against their employers. We coded the cause of every case appearing in an 1874 Parliamentary Report on Master and Servant law, and find that the vast majority of cases were prosecutions of workers for exiting their contracts early.¹⁴ Across districts, the modal fraction of Master and Servant cases brought against employees for absconding from their employer was 100%. In the median district, the fraction of cases brought for absconding was over two-thirds.¹⁵ The typical goal of a prosecution was to use the threat of incarceration and hard labor to prevent workers from leaving an employer, and to pursue and punish those who were not deterred.

The threat of prosecution was credible; not only were prosecutions common (see Figure 1), but they were also largely successful: Hay (2004, Table 2.1) provides evidence on the success rate of masters’ prosecutions after 1800 from seven different sources; in three of them, masters won all of the cases they brought, and no source shows masters winning less than 70% of their cases.¹⁶

Master and Servant prosecutions occurred in the industries most closely associated with the Industrial Revolution. Testimony before Lord Elcho’s Commission (1866) often focused on mining, iron production, and manufacturing, and points to the role that labor market

¹²The requirements for a binding contract in this period are discussed in Holdsworth (1873); they were not particularly stringent, for example, only contracts for service of greater than one year were required to be in writing; shorter contracts, whether written or unwritten, *were binding* despite only oral agreement. Contracts varied in length from two weeks, to one month, to one year, or more in the late 19th century.

¹³A large fraction of workers were paid piece-rates in all of the industries we consider. Piece-rates would directly link output to effort, and so should already provide adequate incentives for worker effort. See Huberman (1996) for cotton, Fitzgerald (1988) for iron, and Church (1986) for coal

¹⁴The report is the *First report of the commissioners appointed to inquire into the working of the Master and Servant Act, 1867* (1874).

¹⁵See Appendix 1A for details.

¹⁶In the *First report of the commissioners* (1874), masters won nearly all of the cases they brought as well. It is important to add that bringing a prosecution for breach of contract was relatively inexpensive, requiring just one appearance before a magistrate by the employer and fees of at most 40 shillings. See Macdonald (1868) and Holdsworth (1873).

conditions played in the employee's decision to breach a contract and the employer's decision to prosecute.¹⁷ Use of the law seems closely tied to the business cycle. One witness, when asked about the cause of prosecutions in the pottery industry, said "I attribute the increase to the present prosperous state of trade; the manufacturers bind the men to those annual agreements, and they take every little breach of contract," and later describes a specific case as follows: "[A worker] wanted to change his employer, but could not do so. The paucity of hands has increased the value of labor, and the workmen can get in many instances more advantageous terms by leaving their present employ, but those [yearly] contracts [in pottery] prevent their leaving."¹⁸ Finally, examination of higher court opinions reveals that imprisonment and orders of specific performance were viewed as legitimate punishments for contract breach up until 1875, as we document in Appendix 1B.

1.2 Unions and the 1875 Repeal of Criminal Sanctions

In Section 2, we model Master and Servant law as a mechanism that allowed employees to commit to long-term contracts, which in turn allowed for risk sharing between employers and employees. Thus, our focus is on the voluntary entry into contracts that could be coercively enforced. Indeed, it is clear that in some circumstances, workers demanded long-term contracts, despite their penal enforcement. Church (1986, pp. 260-261) writes of a labor dispute in 1844 in which "the coalowners substituted a monthly contract for the annual bond, to which the miners reacted by proposing a bond of six-months' duration," preferring the greater wage security of a long-term contract.¹⁹ In Parliamentary testimony, witnesses reported that, at an iron works, "men did not like . . . to be liable to be turned away at any time," and that employees would not like a system of "minute contracts" (essentially employment at will), because they "would require greater security for the maintenance of their employment."²⁰

Employees entered long-term contracts because employers generally fulfilled their obligations under them; this was in part due to the threat of (civil) prosecution of employers for breach of contract, but also to employers' paternalistic behavior toward their employees.²¹

¹⁷ *Report of the Select Committee on Master and Servant* (1866). Witnesses before the Commission included the President of the North of England Institute of Mining Engineers, the manager of an iron company, and the Secretary to the United Trades Committee, among others. Employers from across industries expressed their satisfaction with the law before the Commission.

¹⁸ *Report of the Select Committee on Master and Servant* (1866), pp.60- 61.

¹⁹ Church (1986, p. 261) also describes "[T]he restoration of annual binding in Durham – at the miners' request – during the boom of 1854."

²⁰ *Report of the Select Committee on Master and Servant* (1866), p. 68 and p. 94.

²¹ In the *First report of the commissioners appointed to inquire into the working of the Master and Servant Act, 1867* (1874), we found that around 3% of cases were brought against employers; nearly all of these ended with the employer fulfilling his contractual obligations.

The textile, iron, and coal industries all had strong traditions of paternalism, with employers nurturing reputations for maintaining the welfare of workers during slumps in product demand (Huberman, 1986, Fitzgerald, 1988, and Church, 1986).²² In short, our assumption that employers could commit to keep workers employed despite cyclical downturns is consistent with both historical evidence, as well as theoretical models of labor hoarding (e.g., Holmstrom, 1983). Long-term contracts insured workers against labor market fluctuations, and strong mechanisms for contract enforcement (i.e., prosecutions under Master and Servant law) allowed workers to credibly commit to stay with an employer even when labor markets were tight.

But this begs the question: what made these contracts less desirable in the second half of the 19th century, which led employees to push for the repeal of penal sanctions? On the one hand, technological progress and higher wages should have allowed for greater savings, and decreased the need to insure via long-term contracts. The growth of “friendly societies” and trade unions in the 19th century also substituted for the insurance provided by long-term contracts, by providing assistance to workers when they were ill and by covering funeral expenses, among other services (Webb and Webb, 1902). However, this raises another question: why was an effort made to *repeal* penal sanctions, when (in our model, at least) a voluntary decision not to engage in long-term contracting would have vitiating penal sanctions even had they been legal?

The answer lies in the growth of a powerful trade union movement throughout the 1800s, together with the legal devices used by employers to regulate it. The repeal of penal sanctions had to be done politically, both because individual employers could not commit *not* to use Master and Servant against union activity, and because criminal sanctions for contract breach impaired collective action by workers; the costs of the latter had to be internalized by politically organized groups.

The 19th century common law regarding trade unions and strikes was often ambiguous: unions existed and strikes occurred throughout the 19th century, though both were at times harshly treated by the legal authorities.²³ Unions were not secure prior to their unambiguous legalization in the Trade Union Act of 1871.²⁴ However, despite establishing unions’ legality, the 1871 Act was passed alongside the Criminal Law Amendment Act, which criminalized union activity whenever the behavior of the individuals involved was illegal.²⁵ An early 20th century legal text describes the effect of the 1871 reforms as follows: “[W]hile a strike was

²²Employers in the mid-19th century often housed their employees as well, keeping employees nearby with subsidized housing during business cycle troughs in order to economize on recruitment costs during peaks.

²³See Webb and Webb (1902) for a discussion.

²⁴34 and 35 Vict. c. 31.

²⁵The Criminal Law Amendment Act is 34 and 35 Vict. c. 32.

lawful, practically anything done in pursuance of a strike was still criminal.”²⁶ Unions had strong incentives to achieve the repeal of Master and Servant law’s penal sanctions.

Strengthened by the 1871 Trade Union Act and political reforms such as the Reform Act of 1867, unions did press for the abolition of criminal sanctions under Master and Servant law.²⁷ That members of Parliament saw the repeal of penal sanctions under Master and Servant law as linked to the regulation of unions is clear from the records of debates: for example, in 1875, Joseph Cowen, MP, asked the Home Secretary, “if it is the intention of the Government to introduce a Bill this Session, to amend the Criminal Law Amendment Act, the Master and Servants Act, and the Law with respect to Conspiracy?”²⁸ It is thus not surprising that the Employers and Workmen Act of 1875, which made breach of labor contracts by employees a *civil* offense, was passed alongside legislation regulating union behavior, the Conspiracy and Protection of Property Act.²⁹ Thus, the repeal of penal sanctions under Master and Servant law was part of the process of legalizing unions throughout the 19th century, though it affected contracting for both union members and non-members.

Finally, why did Parliament pass a law in the interest of workers? In fact, the 1875 Employers and Workmen Act was the product of an intense political campaign waged by the Trades Union Congress (TUC). The Liberal Gladstone government, in 1874, responded to political protests organized by the TUC by inviting labor leaders to consult on the reform of Master and Servant law. Politicians seeking election in 1874 campaigned on the repeal of Master and Servant law’s penal sanctions in newly enfranchised working class environments. The threat of independent TUC-backed candidates and the promise of trade-union votes generated political support for repeal among candidates from both parties (Curthoys, 2005, p. 209). Even Conservative party candidates pledged themselves to the TUC program, sometimes more enthusiastically than Liberal candidates. The result was that the Conservative Disraeli government that was formed after the 1874 election repealed criminal sanctions for contract breach the next year, over the opposition of the employers who constituted a large portion of their political base.

2 Contracting Under Master and Servant Law

We model labor market contracting under the shadow of Master and Servant law, as well as the possibility of *ex post* breach of contract, prosecution, and punishment for breach as a

²⁶Tillyard (1916), page 312.

²⁷The Reform Act is 30 and 31 Vict. c. 102.

²⁸HC Deb 04 March 1875 vol 222 c1177.

²⁹The Employers and Workmen Act is 38 and 39 Vict. c. 90 and the Conspiracy and Protection of Property Act is 38 and 39 Vict. c. 86.

simple extension of contracting models in which risk-neutral employers, who can commit to contractual terms, insure risk-averse employees (e.g., Baily, 1974, Azariadis, 1975, Harris and Holmstrom, 1982, and Beaudry and DiNardo, 1991). After signing a contract, an employee observes a realization of an outside spot market wage drawn from a uniform distribution over $[0, 1]$.³⁰ Unlike the standard models, in which employees can exit firms for higher outside wages without penalty, in the simple game we set up, the employee faces the possibility of criminal prosecution for contract breach. The risk-neutral employer hires one unit of labor, producing revenue $\pi > 1$ and pays wages w . The employee maximizes his utility, given by $u(w) - c_s$, where w is the wage received and c_s is the cost borne if the employee is punished under Master and Servant law. We assume that the function $u()$ is increasing and concave, and that $u(0) = 0$. We also assume that the costs of punishment enter an employee's decision-making linearly and separably.³¹

2.1 Agents and Timing

Our model has the following structure, shown as an extensive-form game tree in Figure 2:

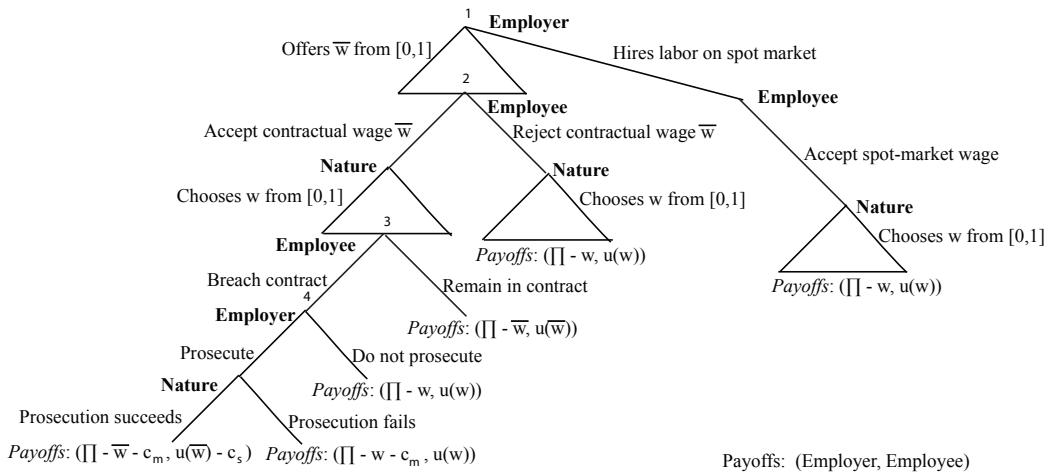


Figure 2: Game Tree.

- In node 1 in Figure 2, the employer either offers an employee a contract specifying a pre-committed wage³² \bar{w} to work for one period or hires labor on the spot market at an uncertain wage. If the contract is not offered, the employee takes the outside wage

³⁰This choice of distribution is made merely for convenience; the results do not hinge on it.

³¹Our results depend on the assumption of risk-aversion, and the linearity of punishment greatly simplifies the analysis.

³²This follows the implicit contracts literature, e.g., Beaudry and DiNardo (1991).

and the employer hires labor at the outside wage, receiving payoffs $u(w)$, and $\pi - w$, respectively.³³

- In node 2, the risk-averse employee decides whether to accept the offered contractual wage. If the employee chooses not to accept the contractual wage, he takes the outside wage and the employer hires labor at the outside wage, receiving payoffs $u(w)$, and $\pi - w$, respectively.
- Next, an observable, exogenous productivity shock determines the spot market wage.
- In node 3, the employee has to choose whether to breach the contract. If he chooses to remain in the contract, his payoff is the utility received from the contractually-specified wage, $u(\bar{w})$ and the employer receives $\pi - \bar{w}$.
- In node 4, reached if the employee chose to breach the contract, the employer must decide whether to prosecute under Master and Servant law. If the employer chooses not to prosecute an employee who broke the contract, the employee receives the outside wage, and thus $u(w)$, while the employer receives $\pi - w$. If the employer chooses to prosecute, he incurs a cost c_m (indicating the cost of prosecution to the “master”).³⁴ It is important to note that prosecution was not always successful; it usually was (see Section 1), but it might be difficult to locate an employee who left, or to prove that a binding contract was agreed to. Thus, we allow prosecution to succeed with some fixed, exogenous probability $q < 1$.
- With probability q , the prosecution is successful: the payoff to the employee is $u(\bar{w}) - c_s$ (recall that c_s was the cost to the “servant” of being prosecuted successfully), while the payoff for the employer is $\pi - \bar{w} - c_m$.³⁵ The employee suffered his punishment and was then legally obligated to return to work at the contractual wage (see Section 1).³⁶
- With probability $(1 - q)$, the prosecution fails: the employee receives $u(w)$, while the employer receives $\pi - w - c_m$ (he chooses to hire labor at the outside wage w , and must also pay the cost of prosecution c_m).³⁷

³³Note that the employer will always hire a worker from the spot market because, by assumption, $\pi > 1$.

³⁴Prosecution was not costless to employers; in addition to monetary costs, appearing and testifying before a county magistrate or justice of the peace required some time and effort from employers.

³⁵We call this a “failed” breach of contract: the employee breached the contract, but failed to leave the employer due to successful prosecution under Master and Servant law.

³⁶It is also important to note that while employees only suffered the consequences of prosecution when it was successful, employers paid their cost of prosecution regardless of its success. Finally, it is historically accurate to assume that $c_m < c_s$: while employers wasted their time, money, and effort in prosecuting an employee, they were hardly subjected to the pains awaiting a convicted employee.

³⁷With the cost of prosecution sunk, the employer will choose to hire a worker from the spot market,

2.2 Optimal Strategies and Equilibrium

We focus on a pure strategy subgame-perfect equilibrium. For the employer, a strategy is of the form $(offer, \bar{w}, R(w))$: the employer chooses whether to offer a contract; the stipulated wage \bar{w} if a contract is offered; and, whether to attempt to retain the worker by prosecuting for breach of contract as a function of the outside wage w . For the employee, a strategy is of the form $(accept(\bar{w}), B(w, \bar{w}))$: the employee chooses whether to accept the contractual offer \bar{w} ; then, conditional on the contractual offer, the employee will choose whether to breach the contract as a function of the outside wage and the contractual wage.

We solve the model by backward induction. Comparing the employer's payoffs from prosecuting a breach with those from not prosecuting, one can find that the employer's decision to prosecute is given by:

$$R(w) = 1 \iff w > \bar{w} + \frac{c_m}{q} \quad (1)$$

Thus, the employer will choose to prosecute ($R(w) = 1$) if and only if the outside wage is sufficiently above the contractual wage (see Figure 3 for a graphical depiction). Equation (1) specifies the employer's optimal strategy in node 4 in Figure 2, the final subgame.

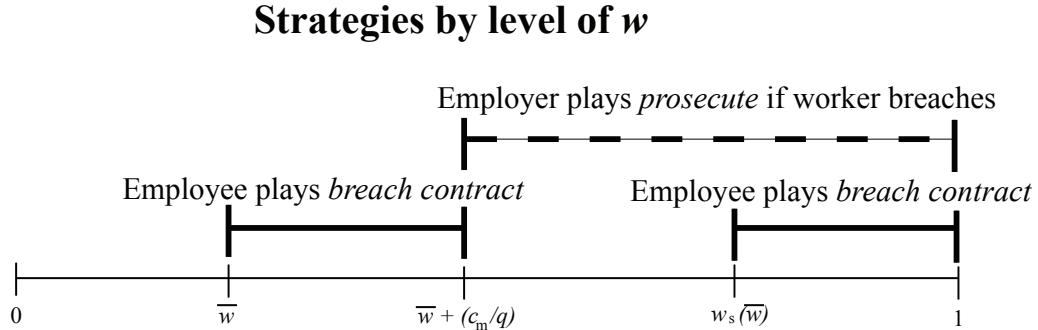


Figure 3: Strategies according to the value of the spot market wage.

Looking ahead to the employer's choice of $R(w)$, the employee chooses to breach the contract if his expected payoff from breach exceeds the expected payoff from staying. His choice is given by the following:

$$B(w, \bar{w}) = 1 \text{ if } u(\bar{w}) < u(w)(1 - R(w)) + u(w)R(w)(1 - q) + (u(\bar{w}) - c_s)R(w)q \quad (2)$$

Using equation (2), we can show that

again, because $\pi > 1$.

$$B(w, \bar{w}) = 1 \text{ if } \bar{w} < w \leq \bar{w} + \frac{c_m}{q} \quad (3)$$

If the outside wage is less than the contractual wage, the employee never breaches the contract: there is no incentive to do so ($B(w, \bar{w}) = 0$). Equation (3) shows that there is a range of w such that $B(w, \bar{w}) = 1$ while $R(w) = 0$. In this range, breach, while profitable for the worker, is too costly to prosecute for the employer.

If w is high enough that the employee knows that the employer will prosecute (that is, $w > \bar{w} + \frac{c_m}{q}$), the employee faces the choice between earning the contractual wage with certainty, and breaching the contract, risking punishment. The employee will choose to breach the contract even when $R = 1$ if the following holds:

$$u(w) > \frac{u(\bar{w}) - q(u(\bar{w}) - c_s)}{1 - q} \quad (4)$$

Thus, the employee chooses to breach the contract ($B(w, \bar{w}) = 1$) if the outside wage is large enough, relative to the cost and likelihood of being successfully prosecuted and retained. We can define w_s , the cut-off wage at which the employee decides to breach a contract despite the employer's credible threat of prosecution, implicitly as a function of \bar{w} :

$$u(w_s) = u(\bar{w}) + \frac{qc_s}{1 - q} \quad (5)$$

Using (3), (4) and (5), we can now explicitly specify the employee's optimal strategy $B(w, \bar{w})$ (see Figure 3):

$$B(w, \bar{w}) = \begin{cases} 0 & \text{if } w \leq \bar{w} \\ 1 & \text{if } \bar{w} < w \leq \bar{w} + \frac{c_m}{q} \\ 0 & \text{if } \bar{w} + \frac{c_m}{q} < w \leq w_s(\bar{w}) \\ 1 & \text{if } w_s(\bar{w}) < w \leq 1 \end{cases} \quad (6)$$

Equation (6) specifies the employee's optimal strategy in node 3 in Figure 2.

In our analysis of an equilibrium contract, we focus on the case in which $w_s(\bar{w}) > \bar{w} + \frac{c_m}{q}$, though our results do not depend on it. We assume the following:

$$\text{Assumption 1: } u(\bar{w} + \frac{c_m}{q}) < u(\bar{w}) + \frac{qc_s}{1 - q} \quad (7)$$

for any $\bar{w} \in [0, 1]$. This condition, which requires c_m to be sufficiently smaller than c_s , guarantees that $w_s(\bar{w}) > \bar{w} + \frac{c_m}{q}$ for all \bar{w} , as it, together with (5) immediately implies that $u(\bar{w} + \frac{c_m}{q}) < u(w_s(\bar{w}))$.

It is, in general, difficult to obtain closed-form expressions for risk premia (with the exception of CARA preferences); thus, we use implicit risk premia throughout. We denote by r_s the risk premium associated with the spot market gamble, and it is defined by $u(\frac{1}{2} - r_s) =$

$$\int_0^1 u(w)dw.$$

The following proposition establishes the existence of an equilibrium contract.

Proposition 1: Assume (7). If $r_s - (c_m + qc_s) > 0$ is sufficiently large, then there exist a \bar{w} that satisfies the employee's and the employer's participation constraints, and a pure-strategy subgame perfect Nash equilibrium with the employer's strategy (*make offer*, \bar{w} , $R(w)$) and the employee's strategy (*accept*, $B(w, \bar{w})$).

Proof: See Appendix 3.

The intuition behind the proof is straightforward. When the risk premium associated with the spot market is sufficiently high, then it becomes mutually beneficial to sign a contract *ex ante*. In this case, the employee is sufficiently risk averse that the benefits of insurance under a long-term contract outweigh the potential punishment under Master and Servant law. The employee's risk aversion allows the employer to charge a high implicit insurance premium (i.e., the contractual wage is relatively low).

A final question is whether reasonable parameter values generate equilibrium contracts, with breach and prosecution – that is, are the assumptions we have made in the model likely to have held in practice in 19th century Britain?

As a back of the envelope evaluation, we consider the case of CRRA utility, with several values of the coefficient of relative risk aversion.³⁸ We then set parameter values of $q = 0.75$, $c_m = 0.025$, and $c_s = 0.1$. The value of q is chosen to match the success rate of prosecutions in Hay (2004, Table 2.1). The cost to the employer of at most 40 shillings for a prosecution was perhaps 1-2 weeks of a coal miner's wage, or around 2-4% of a year's salary.³⁹ Because the average wage in our model is 0.5 on the spot market, one can view 0.025 as a reasonable employer's cost parameter, including his costs of time and effort. The employee's cost could have been three months in prison, though usually it was less severe; a cost of around 20% of the average spot market wage seems reasonable.⁴⁰

Using these parameter values we generate precisely the behavioral patterns described in our model: the cut-off values are as we have assumed them to be; contracts are signed, contract breach occurs when outside wages are high enough, and prosecution occurs as well.⁴¹

³⁸As a baseline, we assume the coefficient of relative risk aversion is 0.95. Our results are qualitatively similar if we set the coefficient of relative risk aversion equal to 0.25, 0.5, and 1.5.

³⁹See Bowley (1900), pp. 107-109. Because Master and Servant cases were summarily decided, legal and time costs to employers bringing cases were low.

⁴⁰In fact, the cost to the employee could have been much lower, if he was merely forced to serve out the contract. As seen above, lower costs of punishment make an equilibrium risk-sharing contract more likely, *ceteris paribus*, so we view our choices of costs as conservative.

⁴¹With a coefficient of relative risk aversion of 0.95 and the parameter values in the text, our simple exercise generates prosecutions in 8% of spot market wage draws. With $c_s = 0.05$ (not unreasonable, given the common outcomes of orders of specific performance or fines, rather than prison), our model generates prosecutions in 40% of wage draws.

Though our model is an extreme simplification, it captures many of the basic elements of contracting in 19th century Britain.⁴²

2.3 Predictions: Labor Demand Shocks, Wages, and Prosecutions

While the relationship between labor demand shocks (outside wages) and prosecutions in our model is clear, the relationship between labor demand shocks and observed wages is ambiguous when penal sanctions for contract breach exist.

Proposition 2: When a Nash equilibrium as defined in Proposition 1 exists, positive labor demand shocks are associated with more prosecutions.

Proof: See Appendix 3.

This result can be seen in Figure 3, as prosecutions are observed only when w is sufficiently large that employees are willing to breach their contracts and employers are willing to prosecute.

Proposition 3: When a Nash equilibrium as defined in Proposition 1 exists, the relationship between labor demand shocks and observed wages is non-monotonic in the presence of Master and Servant prosecutions.

Proof: See Appendix 3.

There are both upward and downward rigidities to wage adjustment under our assumed parameter values. For example, moderate, positive labor demand shocks may result in higher observed wages, as employees breach their contracts, but employers do not find it worthwhile to prosecute. Larger, positive labor demand shocks may result in no change in the observed wage because a credible threat of prosecution can prevent workers from breaching their contracts.

2.3.1 The Consequences of Repeal

The 1875 repeal of Master and Servant law's penal sanctions eliminated employers' ability to criminally sanction a would-be departing worker and retain his labor via *ex post* coercion.⁴³

⁴²Note that we have not analyzed a fully dynamic contracting model between employers and employees, where future sanctions could endogenously enforce contracts; we leave analysis of the impact of Master and Servant law in this case to future work. We have also restricted attention to a partial equilibrium setting. A theoretical analysis of labor market coercion in general equilibrium can be found in Acemoglu and Wolitzky (2011) and a model of bonded labor contracts in general equilibrium can be found in von Lilienfeld-Toal and Mookherjee (2010).

⁴³The qualitative difference between civil and criminal enforcement of contracts stemmed from several sources. First, arrest warrants were no longer issued for workers who left their employers, making it less likely that an employee would be brought back to his employer; second, orders for specific performance were no longer available under summary justice; third, the threat of prison was likely much more effective in inducing an employee to return to work than a fine. Criminal sanctions were not just more costly than civil ones, they also bound wealth-constrained workers who might escape civil sanctions with limited liability.

In the absence of such coercion, our model implies that employees will not stay with the firm in the event of a high wage in the spot market. Thus, binding contracts are not offered in the post-repeal equilibrium, and all labor is sold on the spot market.

That the 1875 repeal reduced the prevalence of long-term, binding contracts is well-supported by the historical evidence. Steinfeld (2001, p. 227) writes that, “Once reform of contract remedies [i.e., the repeal of penal sanctions] had reduced the ability of employers to enforce labor agreements, they would have less incentive to enter contracts for a term even if labor had then wanted them. . . . [T]he outcome of reform would only be to speed up the movement to employment at will, bringing about the demise of both penal sanctions *and* binding contracts.”⁴⁴ Tillyard (1916, p. 325) writes that after 1875, summary justice by the magistrates no longer included the “powers to enforce performance for unexpired periods of service,” and that “contracts of service [were] determinable more and more by very short notice.” Thus, we find it reasonable to model repeal as a reduction in the probability that a worker is successfully prosecuted and retained. Specifically, we assume that post-repeal, $q = 0$, and obtain the following proposition.⁴⁵

Proposition 4: When a Nash equilibrium as defined in Proposition 1 exists, then post-repeal (i.e., $q = 0$) no long-term contracts are signed, average wages rise, and the correlation between labor demand shocks (the spot market wage) and the observed wage increases.

Proof: See Appendix 3.

Long-term contracts are not signed, because it is not in the interest of the employer to offer a contractual wage that is only paid when it is *greater* than the spot market wage (the employee would leave the employer whenever the spot market wage exceeded the contractual wage). Without successful prosecutions, insurance against labor market fluctuations cannot be profitably provided, and the employer will simply hire labor on the spot market. The absence of risk-sharing contracts increases the average observed wage, as employees no longer accept lower wages in exchange for insurance, and increases the responsiveness of the observed wage to labor demand shocks, as observed wages now completely reflect conditions in the spot market for labor.

We can use our model to bound the incidence of the welfare losses from the repeal of penal sanctions.⁴⁶ The actual distribution of the surplus from signing the contract depends on parameter values. If the employer is able to extract all of the surplus from the contract before repeal, then the employer’s loss is 0.11 in higher expected labor costs (a wage increase

⁴⁴Emphasis in the original.

⁴⁵Note that we implicitly assume (as our model has only one period) that there was not an immediate shift toward long-term contracts supported by reputation following the repeal of penal sanctions.

⁴⁶Note that we are able to account for only those welfare changes that directly result from the loss of long-term contracts. We thus do not include in this exercise any general equilibrium effects of repeal, or welfare gains to employees from legal, effective unions as a result of the repeal of penal sanctions.

of over 25%), but there is no welfare change for the employee. If there is perfect competition among employers, and employees extract all of the surplus prior to repeal, then the fall in the employee's utility is 0.2 in certainty-equivalent wages, while the employer's welfare is unchanged.

We next test the model's predictions about the effect of labor demand shocks on Master and Servant prosecutions, and on wages, before and after the repeal of penal sanctions for breach of contract.

3 Empirical Evidence on Prosecutions and Wages

3.1 The Data

To estimate the relationship between labor demand and Master and Servant prosecutions, we combine data from a variety of historical sources.⁴⁷ We use district-level information on criminal prosecutions for labor-market-related criminal offenses (Master and Servant, anti-vagrancy, and anti-begging) in each year from *Judicial Statistics, England and Wales*, covering the years 1858-1875.⁴⁸ Prosecutions data are merged to data on county characteristics, such as population, population density, occupational structure, proportion urban, and illiteracy, from UK censuses between 1851 and 1911, as well as county-level production of iron ore in 1855.⁴⁹ In some specifications we use information on membership in the Amalgamated Society of Engineers (ASE) as an indicator of union membership at the county-year level. We also use the data on members of the ASE to calculate a strike rate and an unemployment rate, which we also include in some specifications as controls. In addition, we use several time series on prices, collected from *British Historical Statistics*, *The British Coal Industry*, and Robson's (1957) *The Cotton Industry in Britain*. In particular, we collected time series of the pithead price of coal, the price of pig iron, and the price of cotton textiles, relative to the price of raw cotton.⁵⁰ Finally, we construct dummy variables identifying a district as urban or rural, Welsh, coal-producing, and pig-iron producing.

Because some of the variables used vary at the district level, and others at the county level, we use two datasets in our analysis of the effect of labor demand shocks on Master and Servant prosecutions. The main dataset contains a panel of observations at the district-year

⁴⁷For a more detailed discussion of the data used and the various sources, please see Appendix 4.

⁴⁸Note that while prosecutions for Master and Servant violations were surely significant prior to 1858, disaggregated statistics on them are not available for these years; the end date of the analysis is determined by the abolition of criminal prosecutions under the Master and Servant Act in 1875.

⁴⁹From *Minerals* (1856).

⁵⁰We thank Greg Clark for suggesting the use of relative textile prices in our analysis.

level, with county-level variables being applied to all districts within a given county.⁵¹ The second dataset contains a panel of observations at the county-year level, with district-level variables (for example, Master and Servant prosecutions) aggregated to the county level. Summary statistics of the variables used in our analysis of the link between labor demand shocks and prosecutions are presented in Table 1, panel A.

Table 1: Summary Statistics

Panel A: Prosecutions Analysis				Panel B: Repeal Analysis			
Variable	Obs	Mean	Std. Dev.	Variable	Obs	Mean	Std. Dev.
District Panel Data				County Panel Data			
Master and Servant Prosecutions	3942	47.72	120.30	Log County Wage Index	2860	4.46	0.14
Vagrancy Prosecutions	3942	60.62	156.30	Union Membership	2860	63.37	67.85
Urban Dummy	3942	0.74	0.44	Population Density	2860	1.41	6.64
County Panel Data				Proportion Urban	2860	52.34	56.29
Master and Servant Pros./1000	936	0.46	0.36	Log Income	2860	2.52	0.33
Vagrancy Prosecutions/1000	936	0.62	0.40	Population	2860	476.72	740.14
Population	936	412.38	595.91	Illiteracy rate	2740	0.17	0.11
Union Membership	936	52.34	56.29	Strike rate	1954	0.00	0.01
Illiteracy rate	900	0.25	0.07	ASE Unemployment rate	1954	0.03	0.03
Strike rate	640	0.00	0.01				
ASE Unemployment rate	640	0.02	0.03				
Cross-Sectional County Data				Cross-Sectional County Data			
Frac. Employed in Textiles in 1851	52	0.05	0.07	Log Average Prosecutions per 1,000 people, in 1858-1875 period	52	-0.98	0.72
Iron County Dummy	52	0.48	0.50	Frac. Employed in Textiles in 1851	52	0.05	0.07
Coal Producing County Dummy	52	0.38	0.49	Iron County Dummy	52	0.48	0.50
Population Density 1851	52	0.96	4.15	Coal Producing County Dummy	52	0.38	0.49
Income 1851	52	10.48	2.88	Population Density 1851	52	0.96	4.15
Wales Dummy	52	0.25	0.44	Income 1851	52	10.48	2.88
Proportion Urban	52	0.12	0.19	Wales Dummy	52	0.25	0.44
Log Iron Ore Production	52	5.05	5.52	Proportion Urban	52	0.12	0.19
Distance to Lancashire	52	160.82	86.94				
Time-Series Data				Time-Series Data			
Log Cotton Price Ratio	18	0.72	0.28	Log Cotton Price Ratio	55	0.94	0.25
Log Coal Price	18	4.04	0.29	Log Coal Price	55	4.03	0.24
Log Iron Price	18	4.11	0.25	Log Iron Price	55	3.99	0.22
				Log Steel Price	40	4.85	0.45

Sources: See Appendix 4.

Our analysis of the repeal of penal sanctions examines wage levels and the relationship between labor demand shocks and wages, before and after 1875. The baseline wage index we constructed varies at the county-year level.⁵² Because the variables of interest (wages and industry-specific labor demand) are measured at the county level, we use county-year level data in our analysis of the effects of repeal. This analysis will also cover a longer time period, as we are no longer limited to the years for which we observe Master and Servant prosecutions.⁵³ In our analysis of the relationship between wages and labor demand shocks we include controls for steel price shocks in some specifications.⁵⁴ Summary statistics of the

⁵¹Standard errors in our regressions are always clustered at the county level.

⁵²We discuss the baseline wage index in detail in Appendix 4, and present results from a variety of alternative wages indices in Appendix 2.

⁵³While we have prosecutions data only for the 1858-1875 period, we can construct a panel of wages and prices for the period 1851-1905.

⁵⁴Data are from McCloskey (1973).

variables used in our analysis of the consequences of repeal are presented in Table 1, panel B.

3.2 Labor Demand Shocks and Master and Servant Prosecutions

To identify a causal relationship between labor market conditions and Master and Servant prosecutions, we consider the effects of exogenous, industry-specific labor demand shocks. In our analysis, we use shocks to the prices of cotton textiles, pig iron, and coal as exogenous changes in the marginal revenue product of labor (i.e., labor demand shocks).⁵⁵ The coal prices and iron prices we use are simply the output prices of the coal mining and iron-producing sectors, respectively. The cotton textile price we use is the ratio of the price of cotton textiles per pound (output) to the price of raw cotton per pound (the major non-wage input). Increases in these prices indicate that the marginal revenue product of labor is high in the three industries.

Proposition 2 leads us to expect greater Master and Servant prosecutions in coal-producing districts when coal prices are high; greater prosecutions in pig iron-producing districts when pig iron prices are high; and greater prosecutions in districts with a high fraction of textile workers when textile prices are high.⁵⁶ Note that our data do not allow us to distinguish prosecutions in sectors experiencing increased output prices from prosecutions in other sectors in the same district, perhaps as a response to the rising labor demand in the affected sector. We view increased prosecutions in the affected sector, as well as other sectors, as the aggregate response of contract breach and prosecution to a sector-specific labor demand shock. Also, to the extent that labor demand shocks spill over into districts in counties without the affected industry, our results (which compare prosecutions in districts in counties with the affected industry to districts in counties without) will be biased toward no effect of labor demand shocks on prosecutions.

We test these hypotheses by estimating the following model:

$$Prosecutions_{dct} = \beta_1 Industry_c \times \log(IndustryPrice_t) + \delta_d + \delta_t + \sum_{t=1858}^{1875} \beta_t X_{c,1851} + \beta_2 \log(pop_{ct}) + \epsilon_{dct}$$

The dependent variable is the number of prosecutions in district d in county c at time

⁵⁵The variation in output prices can be seen as exogenous with respect to individual employers (which brought prosecutions) to the extent that output prices were set in competitive markets, and not by small numbers of firms. The textile, iron, and coal industries in the second half of the 19th century all seem to have fit this requirement.

⁵⁶We use the fraction of a county's workers in textile production in 1851 (males only) as an indicator of textile production in a county; we use county-level dummy variables as indicators of production of iron and coal due to the more ambiguous census occupational categories relevant to these industries (for details, see Appendix 4). Our results are, however, robust to other indicators of industrial location. Note that throughout we use the term "textile prices" to refer to the relative output price of textiles.

t ; the explanatory variable of interest is an interaction between a measure of an industry's presence in county c times the log of the price of the industry's output (recall that fixed county characteristics, including the presence of an industry in 1851, apply to all districts in the relevant county, and are thus absorbed by the district fixed effects). The industries are coal mining, for which the measure of presence at the county level is a dummy variable, and the price is the pithead price of coal; textile production, for which the presence measure is the fraction of employed men who were in the textile industry in the 1851 census, and the price is the ratio of the price of cotton textiles to the price of raw cotton; and pig iron production, the presence of which is indicated by a dummy variable, and for which the price is the price of pig iron. We control for year and district fixed effects, and the log of the population of the county in which the district is located. In some specifications, we add time-varying effects of counties' initial (1851) economic conditions.⁵⁷

In Table 2, columns 1-3, we present results of estimating the model for each industry individually (without the time-varying controls).⁵⁸ In every case positive labor demand shocks are associated with more prosecutions: column 1 shows that a higher cotton textile price, which should increase labor demand in the textile industry, is associated with more prosecutions in counties with a larger fraction of employees in the textile industry.⁵⁹ Columns 2 and 3 show that higher output prices in the coal and iron sectors are associated with more prosecutions, precisely in counties where the relevant industry is prevalent.

The coefficients in column 6, indicate that a 25% increase in coal, iron, or textile prices (approximately one standard deviation for all the industries' prices in our sample) is predicted to increase Master and Servant prosecutions by around 10 in a county with the highest amount of employment in textiles (28%), 16 in iron producing counties, and almost 7 in coal producing counties. These are large effects, relative to a mean of 48 prosecutions per district per year.

One can see the three patterns of industry-specific prices and industry-specific prosecutions in the three graphs of Figure 4. These plot the series of coefficients on an industry-presence times year interaction, from a regression predicting Master and Servant prosecutions

⁵⁷Using population levels, rather than logs, does not change our results. The population of county c at time t is linearly interpolated between census years. The time-varying controls for initial conditions are interactions between year dummies and each county's 1851 population density, the 1851 proportion of workers in manufacturing, the 1851 fraction of the county's population that was urban, and a dummy indicating that the county is in Wales.

⁵⁸Including the time-varying controls does not affect our results; we omit them here for brevity.

⁵⁹We have also considered exogenous variation in raw cotton input prices alone, rather than using the ratio of output to input prices. Under the assumption that raw cotton and labor are complementary inputs in textile production, one would expect fewer prosecutions when cotton input prices are high (as this implies that labor demand is lower). The results using this alternative indicator are very similar to those using the ratio of output to input prices, so we omit these results for brevity.

Table 2: Reduced Form Sectoral Shocks on Master and Servant Prosecutions

	OLS						2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Textiles 1851 X Log(Cotton Price Ratio)	210.9*** (42.39)			159.3*** (42.02)	145.5*** (46.24)	141.2*** (39.05)	147.2*** (45.04)	127.8* (64.94)
Iron County X Log(Iron Price)		76.03*** (22.90)		51.98** (19.48)	64.58** (27.84)	67.27** (33.18)	90.64* (46.71)	89.83* (49.25)
Coal County X Log(Coal Price)				68.32*** (15.90)	41.25*** (10.11)	35.63** (14.31)	27.50*** (8.428)	25.22* (14.92)
Log(Population)	145.5*** (50.52)	124.8*** (42.20)	73.26* (36.68)	79.13** (35.09)	41.84 (36.18)	54.69 (115.2)	83.75** (36.70)	39.21 (38.10)
F-statistic p-value on joint significance				0.000	0.000	0.000	0.000	0.000
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Time-Varying Controls	N	N	N	N	Y	Y	N	Y
County-Specific Trends	N	N	N	N	N	Y	N	N
N	3942	3942	3942	3942	3942	3942	3942	3942

Dependent variable is absolute number of master and servant prosecutions. Standard errors, clustered on county, included in parentheses. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. Columns (1) through (6) are estimated using OLS; columns (7) and (8) use 2SLS, where distance to Lancashire is used as an instrument for employment share in textiles and iron ore production is used as an instrument for pig iron production. First stage results from columns (7) and (8) are presented in the Appendix. * p<0.1, ** p<0.05, *** p<0.01

(conditional on year and district fixed effects and county population), as well as the series of the industry-specific log output price. It is clear from the figures that prosecutions in districts with a given industry are strongly correlated with industry-specific output prices.⁶⁰

One might be concerned that our individual industry regressions merely capture the same effect, in the same counties, three times. For example, one can see in Figure 4 that iron and coal prices followed very similar patterns, and these industries were often located in the same counties. To check whether each industry-level labor demand shock is associated with increased prosecutions, holding fixed shocks in the other industries, in column 4 we examine changes in the three output prices together, by including industry price-industry presence interactions for all three industries in the same model. We find that all of the coefficients maintain their sign, and all are statistically significant, suggesting that each industry shock is independently affecting prosecutions. A joint test of the three labor demand shocks is significant well below 1%.

We next, in column 5, allow for year-specific effects of each county's initial population density, initial fraction of the population working in manufacturing, and initial fraction of the population that is urban, and we allow Wales to experience different year-specific shocks.

⁶⁰We examine the relationship between industry price shocks and prosecutions in a more general model that includes lagged and leading price shocks in Appendix 2, Table A3. We find that contemporaneous price shocks strongly predict prosecutions, while leading price shocks (perhaps indicative of reverse causality concerns) do *not* significantly predict prosecutions.

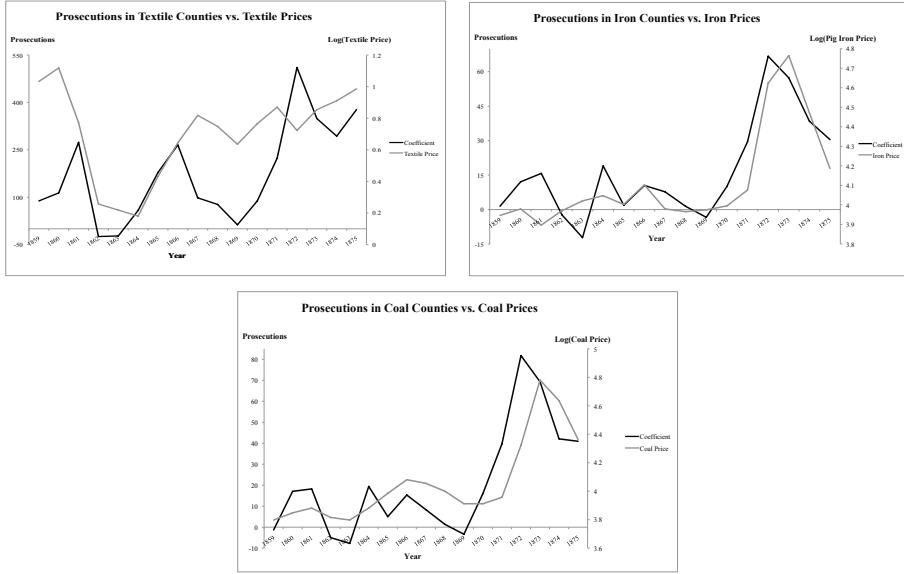


Figure 4: Labor demand shocks (textile output prices relative to raw cotton prices, pig iron prices, and coal prices) plotted alongside Master and Servant prosecutions in textile, iron, and coal producing counties. Coefficients are from a regression of Master and Servant prosecutions on district and year fixed effects, log of population, and the interaction between an industry presence variable and year fixed effects; the interaction coefficients are plotted above.

Again, the labor demand shocks are associated with a significant increase in prosecutions for each industry and the joint test of the demand shocks' significance is highly significant. In column 6, we include linear, county-specific time trends. All of the demand shocks remain highly significant.⁶¹

In columns 7 and 8, we address concerns that the spatial distribution of industry is determined by unobserved variables, such as local legal practices, that also affect the responsiveness of prosecutions to price changes.⁶² We do this by constructing exogenous determinants of industry location for pig iron and textile production (the production of coal is determined by geographic factors and is thus arguably exogenous).⁶³ As an exogenous determinant of pig iron production, we use the county's production of iron ore in 1855.⁶⁴ Ore production would largely have been determined by fixed geographic factors (while pig iron production using ore would have been far more mobile), and the presence of iron ore does in fact predict the presence of the iron industry.⁶⁵ For textiles, we use a county's distance from Lancashire, as it had been the seat of the English textile industry since the 1700s, and this distance variable is strongly correlated with the employment share of textiles in 1851.

⁶¹In a specification we omit for brevity, we also allow for district-specific trends in prosecutions, and the labor demand shocks remain positive and highly significant, individually and jointly.

⁶²See Glaeser and Ellison (1999) on industry agglomeration.

⁶³Our strategy is similar to Badiani (2010).

⁶⁴The source of the data is *Minerals* (1856). See Appendix 2 and Appendix 4 for details.

⁶⁵The cross-sectional relationships between the exogenous and endogenous industry location variables are presented in Appendix 2, Table A2.

Note that these county characteristics are cross-sectional variables; to obtain instruments for labor demand shocks in the textile and iron industries, we interact the exogenous industry location variables with the corresponding price series. The first stage relationships between the instruments and the endogenous labor demand shock variables are very strong (they are reported in Appendix 2, Table A2). As can be seen from Table 2, columns 7 and 8, using these instruments in a two-stage least squares model generates estimates that are individually and jointly significant. Magnitudes of the coefficients are similar to those found using OLS as well.⁶⁶

As a robustness check, we next estimate several specifications using our county-level panel. As noted above, in this dataset district-level prosecutions data are aggregated to the county level. One noteworthy difference between this dataset and that used above is that we can now normalize prosecutions by (interpolated) county population. Additionally, because we have almost no observations with zero prosecutions at the county-year level, we can use the log of prosecutions per capita as an alternative outcome variable to further test the sensitivity of our results to outliers. We estimate an empirical model analogous to that used with the district-level data, but which uses county, rather than district, fixed effects (and uses several variations on the outcome variable).

In Table 3, columns 1-2, we present results using the level of prosecutions as the outcome, as we had used in district-level analysis. We present results with and without time varying controls, and they are consistent with the district level data: in general we find large and significant effects of labor demand shocks on prosecutions.⁶⁷ The only exception is that while the coal-industry demand shock is still large and positive, it is no longer statistically significant in the specification with time-varying controls. However, the joint test of the labor demand shocks is significant in both specifications as well.

In Table 3, columns 3-4, we use prosecutions per capita as our outcome variable.⁶⁸ In these specifications, again, we generally find large, positive and statistically significant effects

⁶⁶We acknowledge that the instruments used might not be excludable, even though they are exogenously determined by geographic characteristics: one might be concerned about county-year specific unobservable variables that are correlated with the instruments (e.g., sharp changes in county politics in areas with ore production), and that are correlated with the outcomes. While we cannot rule out a violation of the exclusion restrictions in our 2SLS specification, we view this as a useful robustness exercise.

⁶⁷One might have worried that the district-level results above were driven by the sorting of employers and/or employees across districts within a county in response to labor market conditions. However, county-level results similar in magnitude to the district-level results would suggest that such sorting did not confound our analysis. Indeed, each county contains four districts, on average, so the magnitudes of the coefficients in our county-level regressions are quite similar to those found in the district-level analysis.

⁶⁸In fact, the outcome is prosecutions per 1,000 inhabitants of a county. Because county population is in the denominator of the outcome variable, we exclude the population control in these specifications and in the specifications reported in columns 5-6, which use log prosecutions per capita as the outcome. Including the population control does not alter any results.

Table 3: County Level Robustness:
 Reduced Form Sectoral Shocks on Master and Servant Prosecutions

	Number of Prosecutions		Prosecutions Per Capita		Log(Prosecutions Per Capita)	
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction Textiles 1851 X Log(Cotton Price Ratio)	1641.7** (711.0)	1431.0* (733.6)	0.780** (0.371)	0.867** (0.391)	1.780*** (0.647)	1.670** (0.755)
Iron County X Log(Iron Price)	186.0** (91.73)	404.9** (198.4)	0.295** (0.121)	0.318 (0.193)	0.360* (0.184)	0.320* (0.178)
Coal County X Log(Coal Price)	234.7*** (78.16)	90.16 (85.59)	0.286*** (0.0948)	0.289** (0.120)	0.296** (0.143)	0.248 (0.164)
Log(Population)	417.7** (171.4)	177.0 (107.6)				
F-statistic p-value on joint significance	0.030	0.076	0.000	0.000	0.001	0.007
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Time-Varying Controls	N	Y	N	Y	N	Y
N	936	936	936	936	930	930

Dependent variable at the top of each column. Standard errors, clustered on county, included in parentheses. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. * p<0.1, ** p<0.05, *** p<0.01

of positive labor demand shocks on prosecutions. The iron demand shock is not significant with time-varying controls (though it is large and positive), but the joint test is significant in both specifications as well.

Finally, in Table 3, columns 5-6, we use the log of prosecutions per capita as the outcome. We find results similar to those above: positive labor demand shocks significantly increase prosecutions. As in column 2, the coal demand shock is not quite statistically significant when the time varying controls are included (though it is large and positive), but the joint tests of the labor demand shocks are still significant at the 1% level across specifications. Owing to the log-log specification, the coefficients in these specifications are naturally interpretable as elasticities, once taking into account the industry presence term in the interaction that makes up our labor demand shocks. The coefficient estimates in column 6 imply that in the county with the highest employment share in textiles, the elasticity of prosecutions with respect to the textile price is around 0.47; in an iron producing county, the elasticity of prosecutions with respect to the pig iron price is 0.32; and, in a coal producing county, the elasticity of prosecutions with respect to the coal price is 0.25.

3.3 Threats to Identification and Interpretation

Our analysis attempts to link changes in *employer and employee* behavior to changes in labor market conditions. However, one must consider the effect of economic changes on criminal prosecutions in general, or on the behavior of magistrates: the behavior of state actors,

rather than private actors, may change in response to economic shocks.⁶⁹ If local constables or magistrates changed their behavior in response to economic fluctuations, this might drive changes in Master and Servant prosecutions. Concerns of this sort can be partially addressed by examining the response of anti-vagrancy prosecutions to the labor demand shocks we have considered.⁷⁰ Anti-vagrancy prosecutions, like those under the Master and Servant Act, were largely targeted toward the relatively unskilled. However, while Master and Servant prosecutions were brought by employers in response to employee breach of contract, anti-vagrancy prosecutions were brought by local law enforcement officials. If either the constabulary's or magistrates' behavior were driving the Master and Servant results, one would expect to see similar responses to labor demand shocks in anti-vagrancy prosecutions.⁷¹

To examine the response of anti-vagrancy prosecutions to labor demand shocks, we estimate specifications similar to those in Table 2, columns 4 and 5, but use anti-vagrancy prosecutions as the outcome. We present the results in Table 4, columns 1-2, and find that estimated coefficients on the labor demand shocks are very small, and statistically insignificant, both individually and jointly.⁷² Prosecutions resulting from employee and employer behavior responded to labor demand shocks, while those that involved only the local police and magistrates did not.

The rise of organized labor in the early 1870s is an important potential confound. For example, Webb and Webb (1902, Appendix V) show that the Durham Miners' Association membership increased from 1,899 in 1870 to 38,000 in 1875, and that other unions also grew rapidly around this time. It is possible that increased organized labor led to increased wages, increased prices, and increased prosecutions, all in the industries (and areas) in which they were located. Exogenous increases in worker strikes are also potential concern, and so in column 3 of Table 4, we include controls for union membership (membership in the ASE) as well as the fraction of members receiving strike pay (the "strike rate").⁷³ While the sample size falls, the effects of the labor demand shocks remain individually and jointly significant, and very close to the corresponding estimates in Table 2, column 4.⁷⁴

⁶⁹Marinescu (2008) finds that judges change their decisions in wrongful termination cases in response to economic conditions.

⁷⁰We always examine anti-vagrancy and anti-begging prosecutions in tandem, but describe the prosecutions as "anti-vagrancy" for the sake of brevity.

⁷¹Admittedly, this exercise is imperfect, because the total number of vagrants may have been smaller when labor demand in a particular industry was greater.

⁷²In Appendix 2, Table A11, we present all of the specifications from Table 2, but using anti-vagrancy prosecutions as the outcome. All of the labor demand shock coefficient estimates are small and statistically insignificant.

⁷³We have also controlled for time-varying effects of a county's initial level of union membership, which yields similar results.

⁷⁴The sample size falls because the "strike rate" is undefined for counties with zero union membership. If we assign those counties with missing strike rates a strike rate of "0," our results are unchanged.

Table 4: Legal Institutions, the Rise of Unions, and Labor Market Characteristics

	<i>Vagrancy and Begging</i>					<i>Labor Market Institutions</i>			
	<i>Prosecutions</i>		<i>Controlling for Unions' Activity and Conditions</i>			<i>Town and Country</i>			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Fraction Textiles 1851 X Log(Cotton Price Ratio)	32.69 (78.37)	44.34 (74.08)	160.7*** (43.89)	125.4*** (40.76)	130.3*** (43.34)	150.8*** (29.77)	148.0*** (41.11)	136.5*** (45.87)	350.3** (135.1)
Iron County X Log(Iron Price)	-14.71 (30.18)	12.83 (9.836)	61.25** (23.71)	67.15** (31.32)	73.14* (36.29)	59.93** (22.97)	68.03** (28.77)	10.74 (10.15)	136.7** (58.40)
Coal County X Log(Coal Price)	-23.12 (28.48)	-11.29 (8.989)	37.09*** (11.30)	45.55* (22.97)	44.19 (27.20)	33.19*** (11.25)	34.76** (13.03)	18.09** (8.397)	145.2*** (45.75)
Log(Population)	164.5 (117.7)	15.21 (19.31)	104.4* (52.62)	36.65 (22.82)	63.01 (43.80)	111.5* (59.08)	77.08 (55.10)	34.37 (26.61)	249.9** (113.5)
F-statistic p-value on joint significance	0.839	0.276	0.000	0.008	0.022	0.000	0.000	0.013	0.002
Last Year in sample period	1875	1875	1875	1871	1871	1875	1875	1875	1875
Sample of districts	All	All	All	All	All	All	All	Urban	Rural
Controls for Union Membership and Strike Rate	N	N	Y	N	Y	Y	Y	N	N
Controls for Illiteracy Rate and Unemployment Rate	N	N	N	N	N	Y	Y	N	N
Time-Varying Controls	N	Y	N	N	N	N	Y	N	N
N	3942	3942	3341	3066	2592	3328	3328	2898	1044

Dependent variable in columns (1) and (2) is the number of vagrancy and begging prosecutions in a district. Dependent variable in columns (3) through (9) is the number of master and servant prosecutions. Standard errors, clustered on county, included in parentheses. All regressions include district and year fixed effects. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. * p<0.1, ** p<0.05, *** p<0.01

One might also be concerned that much of the variation in labor demand comes from the large increase in coal and iron prices beginning in 1872 (see Figure 4). This was a period of economic expansion in Britain: if our results were entirely driven by just one business cycle expansion, especially one in which labor market institutions were changing, one would be concerned about the interpretation of our results. In column 4 of Table 4, we estimate the specification from Table 2, column 4, but excluding the 1872-1875 period that followed the passage of the Trade Union Act of 1871, and during which coal and iron prices soared. In this specification, we remove from our sample the period of highest output prices – the period in which labor demand was greatest (see also the unemployment rate in Figure 1) – along with one quarter of our observations. Our estimates remain individually and jointly significant, with the coefficients on coal and iron somewhat higher, and the coefficient on textiles somewhat lower, than in Table 2. In Table 4, column 5, we both restrict the sample and include the union membership and strike rate controls, and despite the one-third reduction in our sample, the coefficients on textiles and iron remain large and significant, while coal remains large and is marginally insignificant, and the coefficients are jointly significant at under 5%. The results controlling for union membership, for the pre-1872 period, indicate that our findings in the baseline regressions are both robust and capture the general response of Master and Servant prosecutions to labor demand shocks – they were driven neither by a single business cycle, nor by the rise of organized labor.

One might still worry that our results are, in fact, driven by changing local labor market conditions across time and space. For example, changes in the skill composition of the workforce might affect output prices and also affect prosecutions, if skilled workers' outside options differ from those of unskilled workers (changes in the skill composition might also re-

flect other, harder to observe, variation in local economic conditions). In addition, employers may have brought prosecutions in response to local shortages in labor supply, rather than sector-specific labor demand shocks. To address these concerns, in Table 4, column 6, we control for measures of the skill and scarcity of the local labor force, by including a control for county illiteracy and a control for the fraction of union members unemployed.⁷⁵ Including these controls does not change our results: all of our labor-demand shock coefficients are large and statistically significant. In Table 4, column 7, we add time-varying controls, and our results are, again, practically unchanged.

Because the literature on labor market coercion has been focused on rural labor markets, it is of interest to know whether the coercive contract enforcement we have studied – while taking place in industrial Britain – was strictly a rural phenomenon, or was also applied in more competitive, urban labor markets.⁷⁶ To test for differential responses to labor demand shocks between urban and rural areas, we split our district-level sample into two: cities and boroughs (“urban”) and all others (“rural”). We estimate the baseline model from Table 2, column 4, on the rural and urban samples separately, and present the results in Table 4, columns 8 and 9.

We find that while the response of prosecutions to labor demand shocks is larger and more significant in rural districts, there is a significant effect of textile and coal industry shocks in urban districts, too, with the joint tests are significant at almost 1% in the urban sample.⁷⁷ These results are strong evidence that labor market coercion existed not only in rural Britain as it industrialized, but also was a widely used response to labor demand shocks in urban areas, especially where textile production was located.

3.4 Repeal of Penal Sanctions and Average Wages

In 1875, the penal aspects of Master and Servant law were abolished.⁷⁸ Our model suggests that without penal sanctions to keep workers in their contracts, average wages should rise,

⁷⁵Both the illiteracy and unemployment rate controls vary at the county-year level (the illiteracy data are interpolated between census years). We also include our union membership and strike rate controls in Table 4, columns 6 and 7.

⁷⁶This analysis also tests whether voluntary entry into labor contracts, which was surely typical in urban areas, was consistent with the use of *ex post* coercion (as in our model). Finding Master and Servant prosecutions used as a response to labor demand shocks only in rural labor markets would suggest that employer market power (and perhaps *ex ante* coercion) played an important part in the use of Master and Servant law.

⁷⁷In a specification examining the effect of the iron industry labor demand shock alone (leaving out the textile and coal industry labor demand shocks), the estimated coefficient is positive and significant in both the rural and urban sub-samples.

⁷⁸Importantly, this changed the penalty for breach of contract by the employee, but not by the employer (breach by the latter was, and remained, a civil offense). Thus, changes in wage levels and the response of wages to labor demand shocks after 1875 cannot be attributed to a change in the cost of firing workers.

and the responsiveness of wages to labor demand shocks should increase. We now consider the first of these predictions: wages should have risen following the repeal of criminal prosecutions for breach of contract.

We use a county-year level panel dataset covering the years 1851-1905 for our analysis of the repeal of penal sanctions. For each county, we use the log of the average prosecutions per capita over the 1858-1875 period as an indicator of the intensity of use of Master and Servant prosecutions. We expect greater effects of repeal in counties with greater intensity of prosecutions because in these counties a widely used mechanism to keep workers with the firm needed to be replaced, while areas that relied less on Master and Servant prosecutions should have been less affected by the change in law. Cross-sectional variation also allows us to distinguish the effects of repeal of penal sanctions from other changes occurring in the British labor market in 1875.⁷⁹ To test whether repeal of penal sanctions increased wages, and whether this effect was concentrated in counties with more intensive use of Master and Servant prosecutions, we estimate the following model:

$$\log(wage_{ct}) = \beta_1 Post1875_t \times \log(MeanProsecutions_c) + \sum_{t=1851}^{1905} \beta_t X_{c,1851} + \beta_2 X_{ct} + \delta_c + \delta_t + \epsilon_{ct}$$

We regress log wages for a given county-year on the average use of Master and Servant prosecutions interacted with a post-repeal dummy variable; on year-specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy; on interpolated values (between census years) of county population, fraction urban, population density, income, and illiteracy. In some specifications, we control for union membership and the strike rate; we also include county and year fixed effects. The coefficient of interest is on the average prosecutions times post-repeal interaction: we expect a positive coefficient, which would indicate that repeal had a greater positive effect on wages in higher-prosecution counties.⁸⁰

We present our results on the effect of repeal on wage levels in Table 5. Column 1 presents a parsimonious specification with just county and year fixed effects and log population as controls. In this specification, the effect of the average number of prosecutions per capita in a county on wage levels is positive and significant, suggesting that a 70% increase in 1858-1875

⁷⁹Most importantly, wages secularly rose throughout the period under consideration – showing that wages grew after 1875 would not be a very demanding test of our hypothesis that some component of wage growth was due to the abolition of penal sanctions under Master and Servant law.

⁸⁰The variation in wages that we identify here relies on initial variation in occupational distributions across counties, which generates different cross-sectional effects of time-series variation in industry wages, as well as variation in those wages for which we have a panel dataset (builders' wages and coal miners' wages). Concerned about the potential for aggregation or imputation bias in our construction of our wage index, we constructed a variety of alternative indices, and all of our results in Tables 5, 6, and 7 are confirmed using them. Appendix 2, Tables A5 and A6 confirm the robustness of our results in Tables 5 and 7. For brevity, we omit an analogous robustness table for Table 6, as the results are very similar across wage indices and we already include other robustness checks for Table 6 in the Appendix.

prosecutions per capita, roughly 1 standard deviation, before repeal resulted in an almost 1.5% increase in wages following repeal. Column 2 adds our union membership control and controls for year specific effects of initial conditions, with a small fall in our coefficient of interest, to 0.013, implying that the same 1 standard deviation results in roughly 1% increase in post-repeal wages. Column 3 adds interpolated census data controls and allows recessions to have county-specific effects on wages, with little effect on our coefficient of interest.

Table 5: Effect of Repeal on Wage Levels, by Average Prosecutions

	OLS							Arellano-Bond
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Post-1875 X Log(Average Prosecutions)	0.0206** (0.0082)	0.0130* (0.0072)	0.0122* (0.0061)	0.0030** (0.0013)	0.0053*** (0.0017)	0.0073*** (0.0024)	0.0026** (0.0013)	0.0133** (0.0053)
Population Density			-0.0570 (0.0583)		-0.0105 (0.00805)	-0.00453 (0.0124)	-0.00722 (0.00625)	-0.0455* (0.0274)
Proportion Urban			-0.0488 (0.0461)		0.0009 (0.0022)	0.0038 (0.0023)	-0.0012 (0.0018)	0.0010 (0.0047)
Log(Income)			0.0291 (0.0312)		0.0042 (0.0035)	0.0034 (0.0038)	0.0037 (0.0030)	0.0194 (0.0136)
Log(Population)	0.1050*** (0.0279)	0.0559** (0.0219)	0.0944** (0.0389)	0.0113*** (0.0038)	0.0177*** (0.0059)	0.0158* (0.0090)	0.0123*** (0.0046)	0.0511 (0.0343)
Union Membership			0.170 (0.1080)	0.0881 (0.0955)	0.0648** (0.0282)	0.0170 (0.0172)	0.0234 (0.0235)	0.0606** (0.0298)
Lagged Log(Wage)					0.861*** (0.0198)	0.849*** (0.0125)	0.837*** (0.0111)	0.836*** (0.0110)
Time-Varying Controls	N	Y	Y	N	Y	Y	Y	Y
Labor market controls	N	N	N	N	N	Y	N	N
Post-1875 X county controls	N	N	N	N	N	N	Y	N
County-specific recession effect	N	N	Y	N	Y	Y	Y	Y
N	2860	2860	2392	2808	2392	1685	2392	2392

Dependent variable is log county wage. Standard errors (in parentheses) are clustered by county, except in the case of the Arellano-Bond estimator, where robust GMM standard errors are reported. All regressions include county and year fixed effects. Proportion urban, log income and log population are interpolated between census years. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. Labor market controls are a county's unemployment rate, the rate of union members on strike, and the fraction of the population illiterate. County controls are 1851 union membership, an indicator for coal producing county, an indicator for iron producing county, and the fraction of the county's male workforce employed in textile production in 1851. The county-specific effect of a recession is a recession indicator (taken from peaks and troughs between 1860 and 1905 noted in Ford, 1981) interacted with a set of county dummy variables. * p<0.1, ** p<0.05, *** p<0.01

Column 4 repeats the parsimonious specification of column 1, but includes a lag of the log wage, in order to control for potentially persistent features of past wages, which our model suggests could operate via long-term contracts.⁸¹ The coefficient estimate in column 4 is statistically significant, and implies a smaller effect on wages: a one standard deviation increase in prosecutions before repeal results in a 0.2% effect on post-repeal wages, controlling for any persistent effects of past wages. In column 5, we add the full set of controls from column 3, and estimate that a one standard deviation increase in prosecutions before repeal results in a 0.35% increase in post repeal wages.

One might worry that the estimated effect of repeal in high-prosecution counties captures a differential impact of repeal on counties with particular labor market institutions or char-

⁸¹Including a lagged dependent variable can induce the well-known Nickell bias (Nickell, 1981), but given the long time-dimension of our panel (55 years), this bias will be close to 0 and should not be a serious concern.

acteristics (e.g., the skill composition of the labor force), other than a differential impact due to changing contracts. Thus, in column 6, we add our labor market control variables: the strike rate, unemployment rate, and illiteracy as controls, with the result that the coefficient increases to 0.007.⁸²

As a final, stringent check of whether our post-repeal interaction with prosecutions captures a change in wages due to some other county characteristic, we add to the specification in column 5 interactions of the post-repeal dummy with initial county characteristics: our coal county and iron county indicators, and the employment share of textiles, and the county union membership level in 1851. In column 7, we present the results: even allowing for differential wage changes after 1875 in counties with varying initial conditions, we estimate a post-repeal higher wage in high-prosecution counties.⁸³

We next use the Arellano-Bond estimator with the specification in column 5 to address concerns about biases in our lagged dependent variable models. We present results from this specification in column 8, and find a coefficient of roughly 0.013, somewhat larger than in the other specifications with lags, but similar to columns 2 and 3.

An important concern with our analysis of repeal in high- and low-prosecution counties is that the number of prosecutions in a county was not exogenously determined. While we have controlled for a variety of county characteristics using various specifications, one is naturally concerned that wages may have followed different trends in high- and low-prosecution counties, and that the post-repeal interaction is merely capturing these different patterns. Thus, we estimate our empirical model of the effects of repeal from Table 5, column 5, but include interactions between prosecutions and dummy variables for five-year time periods (1851-1855, 1856-1860, etc.), instead of simply an interaction between prosecutions and a post-repeal dummy variable. In Figure 5, we plot the coefficients on these dummy variables around the time of repeal, along with the 95% confidence intervals around them. In the figure, it is clear that a large number of prosecutions in a county is initially not associated with significantly greater wages in any five year period – until the 1876-1880 period just after the repeal of Master and Servant law’s penal sanctions.⁸⁴ Though we cannot rule out the possibility that some unobserved change occurred in high-prosecution counties concurrently with the repeal of Master and Servant’s penal sanctions, our results suggest that repeal of penal sanctions did raise wages.⁸⁵

⁸²This specification, like column 5, includes the union membership control as well.

⁸³The coefficient is smaller, but remains significant at the 5% level.

⁸⁴Although the 1876-1880 coefficient is not significantly greater than the three coefficients from the pre-repeal period, it is larger than all of them; the 1881-1885 and 1886-1890 coefficients are significantly greater than the pre-repeal coefficients. Finally, the sum of the three post-repeal coefficients is significantly larger than the sum of the three pre-repeal coefficients.

⁸⁵Note that the specification in Table 5, column 7, rules out the possibility that these sharp changes were

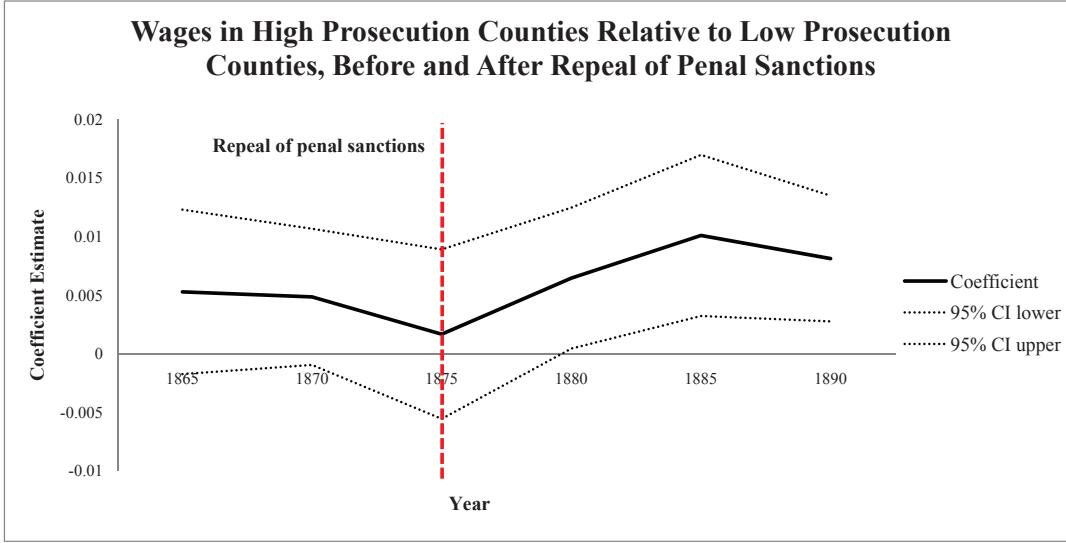


Figure 5: Wages in high prosecution counties, relative to low prosecution counties, before and after repeal of penal sanctions. Figure plots coefficients (and their 95% confidence intervals) from a regression of wages at the county-year level on interactions between the log of a county's average Master and Servant prosecutions per capita over the 1858-1875 period and dummy variables for five-year time periods. The coefficients from these interactions are plotted. Control variables in the regression are year and county fixed effects, county-specific recession effects, controls for county characteristics (population, population density, proportion of population that is urban, and income all interpolated between census years), year-specific controls for initial county characteristics (population density, income, proportion urban, and a Wales dummy), membership in the Amalgamated Society of Engineers, measured at the county-year level, and one-year lagged wage.

3.5 Labor Demand Shocks and Wages Following Repeal

Our model predicts that wages should have responded weakly or non-monotonically to labor demand shocks when Master and Servant law's penal sanctions were in effect, and that the repeal of penal sanctions should have made wages more responsive to, and monotonically increasing in, these shocks (see Propositions 3 and 4, in Section 2). As a first step toward evaluating these hypotheses, in Figure 6, we show nonparametric graphs of log wage residuals on our three industries' labor demand shock residuals, separately by industry, for the 1851-1875 and 1876-1905 periods (inclusive). Except for the iron industry graph in the post-repeal period, the residuals are the deviations of wages and industry shocks from the values predicted by year and county fixed effects, the log of population, and the year-specific effects of county characteristics in 1851.

The effect of iron industry shocks post-repeal is subject to a particular omitted variable concern. During the 1870s, important technical changes occurred in the production of metal, with the vastly increased use of the Bessemer process for producing steel, a higher-quality substitute for iron.⁸⁶ In 1883, the ratio of steel to iron production was 14%, growing to

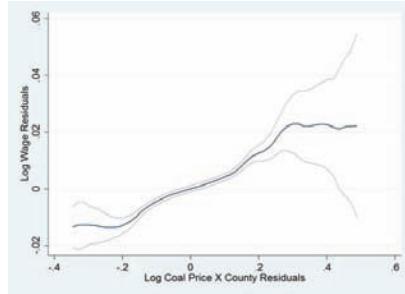
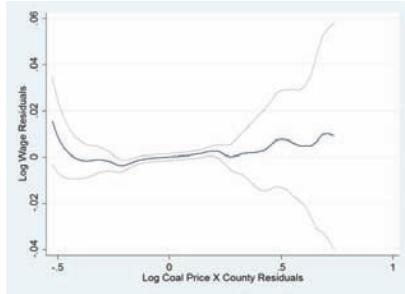
strongly correlated with the presence of our three industries of interest or with the initial level of unionization in a county.

⁸⁶While invented in the 1850s, it took decades before the Bessemer steel mills were widely adopted and for the price of steel to be driven down far enough for it to be an effective replacement for iron.

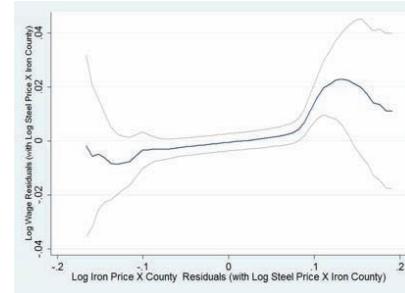
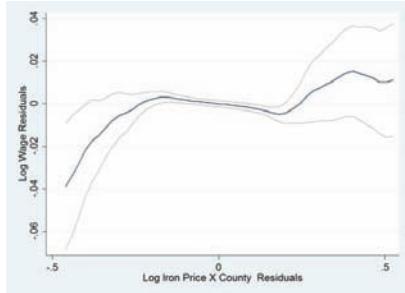
Pre Repeal Lowess Graphs

Post Repeal Lowess Graphs

Coal



Iron



Textiles

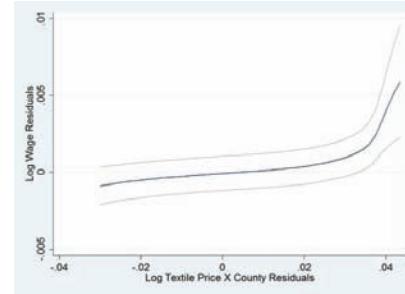
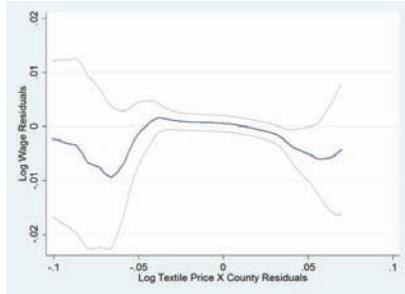


Figure 6: Wage residuals plotted against industry labor demand shock residuals before and after the repeal of penal sanctions. Control variables in the regressions are year and county fixed effects, log population, and year-specific controls for initial county characteristics (population density, income, proportion urban, and a Wales dummy). In the case of iron following repeal, we also include a control for iron county specific effects of the log steel price, as discussed in the text.

62% by 1905 (Orsagh, 1961). One worries that, beginning in the 1870s, technical progress in steel production may have driven *down* the price of iron, while driving *up* the wages of workers in iron producing areas. To address this concern, we collected steel price data from McCloskey (1973) and, in the post-repeal iron industry graph, we included a control for the effects of steel prices in iron producing counties (in addition to the controls used in the other graphs).⁸⁷

⁸⁷In Appendix 2, Figure A3, we show the post-1875 iron shock graph without controlling for steel price shocks in iron counties. The graph is less consistently monotonic than the one in Figure 6, as one would

The non-parametric graphical evidence is consistent with the hypothesis that wages responded non-monotonically to labor demand shocks prior to the 1875 repeal of penal sanctions for contract breach. Our 19th century wage indices are certainly noisy and measured with error, but nonetheless, all of the pre-repeal figures have both a downward sloping component as well as an upward sloping component. This is in contrast to the post-repeal period, where all of the graphs show strong, positive, and nearly uniformly monotonic correlations between wage residuals and industry shock residuals.⁸⁸ In sum, the graphical evidence in Figure 6 is consistent with the predicted wage-labor demand shock relationships from Propositions 3 and 4.

To complement the graphical evidence, we more formally examine the relationship between labor demand shocks and wages, both pre- and post-repeal of penal sanctions. Parameter values in our model and the distribution of residual labor demand shocks relative to contractual wages – both impossible for us to observe – will determine the precise shape of the relationship between labor demand shocks and wages. We take a very conservative approach in our analysis: based on Proposition 3, we simply predict that there should be a weak, or non-existent, linear relationship between labor demand shocks and wages pre-repeal, because of both upward and downward wage rigidities, and regions of non-monotonicity. Based on Proposition 4, we predict a strong, positive linear relationship between labor demand shocks and wages post-repeal.

We thus split the data into the 1851-1875 and 1876-1905 periods (inclusive), and regress wages on labor demand shocks for each period, controlling for county and year fixed effects, log population, and the time-varying effects of the baseline characteristics.⁸⁹ In Table 6, columns 1-4, we present regression results examining the effect of industry-specific labor demand shocks pre-repeal (individually, then jointly). All of the pre-repeal coefficients on the labor demand shocks are small and insignificant.⁹⁰ In columns 5-8, we present the same specifications post repeal (adding the effect of steel price shocks in iron counties in columns 6 and 8). The three industries' labor demand shocks are all large, positive, and statistically

expect given our concerns about the impact of increased steel production. We do not control for steel price shocks pre-repeal because we lack steel price data prior to 1864; however, including these shocks generates very similar results, based on fewer observations.

⁸⁸The only range of shocks associated with a negative slope are in the upper tail of the iron industry shock residual, but this is very imprecisely estimated.

⁸⁹In the post-repeal period, we control for the effects of steel prices on iron county wages whenever iron industry labor demand shocks are included as explanatory variables. In Appendix 2, Table A10, we present results excluding the steel price shocks from the iron price shock specifications.

⁹⁰In Appendix 2, Table A9 we examine the robustness of these results. Excluding the time-varying controls affects the coefficients, but does not change the qualitative finding that there is a weaker positive relationship between labor demand shocks and wages pre-repeal. Including steel price shocks as controls in the pre-repeal period also does not change our results (the sample size shrinks because of missing steel price data prior to 1875).

significant in the post-repeal period when estimated individually. When estimated jointly, the coal and iron shocks are large and significant, while the textile shock coefficient is of moderate size, but no longer significant. The joint test on the three shocks is significant well below 1%.⁹¹ Consistent with the Bessemer diffusion discussed above, lower steel prices are associated with significantly higher wages in iron-producing counties, as one would expect if the growth of the steel industry drove up the wages of workers in metal producing jobs.

Table 6: Wage Responses to Labor Demand Shocks, Pre- and Post-Repeal of Penal Sanctions

	Pre-Repeal				Post-Repeal			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Textiles 1851 X Log(Cotton Price Ratio)	-0.0071 (0.109)			-0.0017 (0.107)	0.278*** (0.0951)			0.102 (0.0925)
Iron County X Log(Iron Price)		-0.0028 (0.0214)		-0.0081 (0.0215)		0.175*** (0.0633)		0.126** (0.0494)
Coal County X Log(Coal Price)			0.0149 (0.0205)	0.0167 (0.0203)			0.101*** (0.0176)	0.105*** (0.0196)
Iron County X Log(Steel Price)						-0.168** (0.0638)		-0.158** (0.0619)
Log(Population)	0.0517 (0.0368)	0.0520 (0.0356)	0.0459 (0.0349)	0.0460 (0.0349)	0.124*** (0.0409)	0.118*** (0.0380)	0.102*** (0.0342)	0.0946*** (0.0314)
F-statistic p-value on joint significance				0.852				0.000
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Time-Varying Controls	Y	Y	Y	Y	Y	Y	Y	Y
N	1300	1300	1300	1300	1560	1560	1560	1560

Dependent variable is the log of the county wage. Standard errors, clustered on county, included in parentheses. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. * p<0.1, ** p<0.05, *** p<0.01

Figure 6 and Table 6 provide evidence consistent with Proposition 4. In order to confirm that our results are robust to controlling for the effect of pre-1875 prosecutions and pooling the pre- and post-1875 samples, we examine the response of wages to labor demand shocks using a specification analogous to that used in Table 5. To that model, we add as explanatory variables our industry-level demand shocks (output prices interacted with industry presence) interacted with a post-repeal dummy variable (plus the additional lower-level interactions). We also estimate a model that includes an interaction of our labor demand shocks, the post-repeal dummy, and the county's pre-repeal level of Master and Servant prosecutions. Proposition 4 predicts that the interaction between the post-1875 dummy and the labor demand shock variables will be positive and significant, indicating greater responsiveness of wages to labor demand shocks post-repeal; one would expect that this effect will be larger in districts that engaged in more prosecutions (where repeal would have had the greatest impact). Formally, we estimate the following regression:

⁹¹Estimating columns 6 and 8 without steel price shocks makes the iron shock coefficient negative, while the other coefficients do not change. Adding steel price shocks' effects in iron counties to columns 5 and 7 has no effect on our results. Finally, excluding the time-varying controls has no effect on our results as well. All of these results are available in Appendix 2, Table A10.

$$\begin{aligned}
\log(wage_{ct}) = & \beta_0 Post1875_t \times \log(MeanProsecutions_c) + \\
& \sum_{i=Industry} \beta_i Post1875_t \times Industry_{ic} + \beta_{2i} Industry_{ic} \times \log(IndustryPrice_{it}) \\
& + \beta_{3i} Post1875_t \times Industry_{ic} \times \log(IndustryPrice_{it}) \\
& + \beta_2 \log(pop_{ct}) + \sum_{t=1851}^{1905} \beta_t X_{c,1851} + \beta_2 X_{ct} + \delta_c + \delta_t + \epsilon_{ct}
\end{aligned}$$

In Table 7, we present the results of estimating this empirical model using several specifications. In column 1, we estimate the change in the wage's responsiveness to the industry demand shocks, including the labor demand shocks interacted with the post repeal dummy, the lower-order interactions, and county and year fixed effects and log population as explanatory variables. In this specification, wages responded significantly more to labor demand shocks in the textile and coal industries following repeal, though there is no effect of repeal on the wage response to iron industry shocks. The joint test of the three post-repeal interactions with industry shocks is significant at well below 1%.

Table 7: Reduced Form Sectoral Shocks on Wages, Pre- and Post-Repeal of Penal Sanctions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-1875 X Texile County X Cotton Price	0.498*** (0.087)	0.268** (0.120)	0.117*** (0.030)	0.106 (0.074)	0.0689* (0.041)	0.0176 (0.114)	0.180*** (0.049)
Post-1875 X Iron County X Iron Price	-0.026 (0.017)	0.123*** (0.027)	0.0044 (0.006)	0.0256*** (0.009)	-0.0027 (0.011)	0.0187 (0.0187)	0.0065 (0.011)
Post-1875 X Coal County X Coal Price	0.0663*** (0.022)	0.103*** (0.019)	0.0185** (0.009)	0.0262*** (0.009)	0.0314** (0.012)	0.0323*** (0.0114)	0.0250* (0.014)
Post-1875 X Texile County X Textile Price X Log (Average Prosecutions)							0.114* (0.058)
Post-1875 X Iron County X Iron Price X Log (Average Prosecutions)							0.0012 (0.006)
Post-1875 X Coal County X Coal Price X Log (Average Prosecutions)							0.0145 (0.016)
Log(Population)	0.0621** (0.024)	0.0692*** (0.023)	0.0081** (0.004)	0.0088*** (0.006)	0.0047 (0.00492)	0.00265 (0.00492)	0.0071* (0.004)
Lagged Log(Wage)			0.837*** (0.016)	0.826*** (0.024)	0.782*** (0.026)	0.767*** (0.0346)	0.838*** (0.015)
Post-1875 X Log (Average Prosecutions)	0.0031 (0.006)	0.0011 (0.004)	0.0003 (0.001)	0.0004 (0.002)	-0.0004 (0.001)	0.00133 (0.00151)	0.0181 (0.019)
F-statistic p-value on joint significance of triple interactions	0.000	0.000	0.002	0.002	0.050	0.029	0.003
Time-Varying Controls	N	N	N	N	Y	Y	N
Steel Price X Iron County Control	N	Y	N	Y	N	Y	N
Interpolated Controls	N	N	N	N	Y	Y	N
Union Membership Control	N	N	N	N	Y	Y	N
Trend X County Characteristics	N	N	N	N	Y	Y	N
N	2860	2080	2808	2080	2808	2080	2808

Dependent variable is the log of county wages. Standard errors, clustered on county, included in parentheses. All regressions include county and year fixed effects. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. The interpolated controls are interpolated population, income, proportion urban, and population density between census years. Linear time trends associated with county characteristics are the interaction of year with an indicator for iron county, an indicator for coal county, the fraction of the male workforce employed in textile production, and union membership in 1851. Union membership is from the Amalgamated Society of Engineers, measured at the county-year level. * p<0.1, ** p<0.05, *** p<0.01.

As discussed above, it may be important to control for the effects of changing steel prices on wages in iron counties, especially in the years after 1875. Thus, in column 2, we add to the specification in column 1 the effects of steel price shocks in iron producing counties, allowing these shocks to have a differential effect post-1875. In this specification, post-repeal wages respond significantly more to labor demand shocks in all three industries following repeal (the joint test is highly significant as well), entirely consistent with the predictions of Proposition 4.

In columns 3 and 4, we repeat the specifications from columns 1 and 2, but add one-year lagged wages as an additional control in each. Results are generally similar to columns 1 and 2: textile labor demand shocks are more strongly correlated with wages post repeal, though the effect is not quite statistically significant in column 4; coal shocks are significantly more strongly associated with wages post-repeal across specifications; iron shocks are significantly more strongly associated with wages following repeal when changes in steel prices are accounted for (and the joint tests of the three post-repeal interactions with industry shocks are again significant below 1%).

One might worry that the results presented thus far are estimated over a very long time period, over which secular changes in the labor market might occur that would affect wages and their response to labor demand shocks. One might be particularly concerned that economic conditions that affect wage volatility (e.g., workers' wealth or the availability of insurance (Krueger and Meyer, 2002)) evolved differently over time according to the industrial composition or unionization level in a county.⁹² Thus, in columns 5 and 6 we estimate our most stringent specification, adding to the specifications in columns 3 and 4 our time-varying controls, interpolated county characteristics, union membership, and linear trends interacted with our coal and iron industry indicators, the employment share of textiles in 1851 and the county's 1851 level of union membership. We again find that wages responded more to textile and coal industry labor demand shocks following repeal (though coefficients are smaller and less statistically significant). While iron shocks no longer have a larger effect on wages post-repeal, joint tests of the three post repeal interactions with industries' labor demand shocks are significant at 5% in both of these specifications.

⁹²In particular, one might be worried that the elasticity of labor supply changed across time, making a given labor demand shock's effect on wages larger, even in the absence of repeal. By controlling for secular changes in economic conditions in Table 7 columns 5 and 6, we try to capture the evolution of economic variables that affect the elasticity of labor supply. In addition, we more directly examine the elasticity of the labor supply curve in Appendix 2, Table A7. We find that to the extent that the labor supply elasticity changed after 1875, it became more *elastic*, which should lead to *smaller* wage responses to a labor demand shock post-1875, in the absence of the repeal of penal sanctions. One might also wonder if changes occurred along other dimensions such as the provision of insurance or unemployment. Though the data are imperfect, we examine changes in unemployment rates among members of the Amalgamated Society of Engineers in Appendix 2, Table A8 and Figure A2.

Finally, in column 7, we include interactions of the average level of prosecutions per capita prior to repeal with the post-1875 indicator and the industry-level labor demand shocks – essentially interacting 4 variables.⁹³ Because we include all of the lower order interactions, we omit the other controls except for log population, and county and year fixed effects. This specification tests whether the increased responsiveness of wages post-repeal was larger in counties that relied on Master and Servant law more pre-1875. While the only statistically significant “quadruple interaction” term is the post-1875 textiles price shock interacted with average prosecutions, all of the quadruple interactions are positive, and the coefficients on the triple interactions remain significant for both textiles and coal and insignificant (but positive) for iron, with a p-value on the joint triple interactions of 0.003 and on the quadruple interactions of 0.23 (the latter is not reported in the table). While this is an extremely demanding specification given our data, it is reassuring that despite the imprecision, the coefficients all have the signs predicted by the theory.

We find an additional interesting result: when one accounts for the effects of increased wage responsiveness to labor demand shocks, one finds that the interaction between prosecution intensity and the post-1875 dummy variable is no longer significantly associated with wage levels (see the last variable reported in Table 7). This suggests that greater wage growth in response to positive labor demand shocks played an important role in raising wages in areas with high levels of prosecutions, after those prosecutions ended.

4 Conclusion

Coercive legal restrictions on labor mobility existed in Britain well into the second half of the 19th century: workers could insure themselves against low wages by signing contracts binding them to firms, though the contracts were enforced by the threat of imprisonment and forced labor. This threat was made credible by the tens of thousands of prosecutions under Master and Servant law in the 1860s and 1870s.

We document that criminal prosecutions were widely applied by employers in response to labor demand shocks: a high marginal revenue product of labor led to greater numbers of prosecutions. We address concerns about endogeneity by using exogenous industry-specific output price shocks for independent variation in labor demand, and examining the resulting prosecutions specifically in areas where affected industries were concentrated. We find that positive labor demand shocks in the coal mining, iron, and textile industries all produced increased prosecutions, precisely in counties where those industries were located. Coercive

⁹³We omit a specification controlling for steel price shocks because the loss of sample size would hamper our ability to extract a signal while including all of the lower order interactions.

contract enforcement was widely used, even in urban England. We find further evidence suggesting that employers used penal sanctions as a substitute for paying higher wages in response to positive labor demand shocks, which supported long-term contracting: average wages in high prosecution counties, and the responsiveness of wages to labor demand shocks, increased after the 1875 elimination of criminal prosecutions under Master and Servant law.

Our results extend analyses of contracting beyond the context of employment at will, and shed light on a number of issues in historical labor economics. First, the widespread use of criminal prosecutions suggests that, indeed, employers valued the ability to legally bind workers even in a modern, industrial economy. Second, consistent with our model, contract enforcement was a more pressing concern for employers during periods of tight labor markets. Third, the abolition of criminal prosecutions under Master and Servant law eliminated the use of legal coercion as a response by employers to the threat of employee departure; thus, employers switched to raising wages in order to retain labor in response to high labor demand. Employees may have paid a price of their own in the loss of insurance provided by long-term contracts, though they were increasingly protected from risk by expanding trade unions.

Historical labor markets have rarely looked like textbook, perfectly competitive markets. Attempts to manage labor mobility have generated a wide variety of legal institutions, ranging from slavery to employment at will. We believe that the study of intermediate cases, such as 19th century Britain, the American South after the Civil War, and the post-emancipation British Caribbean, illuminates the role of legal institutions in securing the supply of effective labor, and represents a rich area for future work.

References

- Acemoglu, Daron and Alexander Wolitzky** (2011). “The Economics of Labor Coercion,” *Econometrica*, 79(2), pp. 555-600.
- Arellano, Manuel and Stephen Bond** (1991). “Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations” *Review of Economic Studies*, 58(2), pp. 277-297.
- Azariadis, Costas** (1975). “Implicit Contracts and Underemployment Equilibria,” *Journal of Political Economy*, 83(6), pp. 1183-1202.
- Badiani, Reena** (2010). “Factories and Farms: How Does Economic Growth Impact Rural Incomes and Educational Investment?” Duke University, mimeo.
- Baily, Martin N.** (1974). “Wages and Employment under Uncertain Demand,” *Review of Economic Studies*, 41(1), pp. 37-50.

Bardhan, Pranab (1983). “Labor-Tying in a Poor Agrarian Economy: A Theoretical and Empirical Analysis,” *Quarterly Journal of Economics*, 98(3), pp. 501-514.

Beaudry, Paul and John DiNardo (1991). “The Effect of Implicit Contracts on the Movement of Wages over the Business Cycle: Evidence from Micro Data,” *Journal of Political Economy*, 99(4), pp. 665-688.

Blanchard, Olivier Jean and Lawrence F. Katz (1992). “Regional Evolutions,” *Brookings Papers on Economic Activity*, 1992(1), pp. 1-75.

Bobonis, Gustavo J. and Peter Morrow (2010). “Labor Coercion and the Accumulation of Human Capital,” University of Toronto, Department of Economics, Working Paper 335.

Botero, Juan Carlos, Simeon Djankov, Rafael La Porta, Florencio Lopez de Silanes, and Andrei Shleifer (2004). “The Regulation of Labor,” *Quarterly Journal of Economics*, 119(4), pp. 1339-1382.

Bowley, Arthur L. (1900). *Wages in the United Kingdom in the Nineteenth Century*. London: C.J. Clay and Sons.

Bowley, Arthur L. and George H. Wood (1906). “The Statistics of Wages in the United Kingdom during the Nineteenth Century. (Part XIV). Engineering and Shipbuilding,” *Journal of the Royal Statistical Society*, 69(1), pp.148-196.

Brenner, Robert (1976). “Agrarian Class Structure and Economic Development in Preindustrial Europe,” *Past and Present*, 70, pp. 30-75.

Caballero, R.J. and Cowan, K.N. and Engel, E. and Micco, A. (2004). “Effective Labor Regulation and Microeconomic Flexibility,” National Bureau of Economic Research Working Paper.

Church, Roy (1986). *The History of the British Coal Industry, 1830-1913*. Oxford: Clarendon Press.

Coase, Ronald (1937). “The Nature of the Firm,” *Economica*, 4(16), pp. 386-405.

Curthoys, Mark C. (2004). *Governments, Labour, and the Law in mid-Victorian Britain: The Trade Union Legislation of the 1870s*. Oxford University Press, USA

Daunton, Martin (1980). “Miners’ Houses: South Wales and the Great Northern Coalfield, 1880–1914” *International Review of Social History*, 25(2), pp. 143–175.

Dell, Melissa (2009). “The Persistent Effects of Peru’s Mining Mita,” MIT, mimeo.

Domar, Evsey and Mark J. Machina (1984). “On the Profitability of Russian Serfdom,” *Journal of Economic History*, 44(4), pp. 919-955.

Ellison, Glenn and Edward L. Glaeser (1999). “The geographic concentration of industry: Does natural advantage explain agglomeration?” *The American Economic Review*, 89(2), pp. 311–316.

First report of the commissioners appointed to inquire into the working of the Master and Servant Act, 1867. P.P. 1874 (C.1094).

Fitzgerald, Robert (1988). *British Labour and Industrial Welfare, 1846-1939*. London: Croom Helm

Fogel, Robert W., and Stanley L. Engerman (1974). *Time on the Cross: The Economics of American Negro Slavery*. New York: Little, Brown.

Ford, A.G. (1981). “The Trade Cycle in Britain 1860-1914,” in R. Floud and D. McCloskey eds., *The Economic History of Britain since 1700, Vol. 2: 1860 to the 1970s*. Cambridge, UK: Cambridge University Press.

Frank, Christopher (2004). “Britain: The Defeat of the 1844 Master and Servants Bill,” in Hay, Douglas and Paul Craven eds., *Masters, Servants, and Magistrates in Britain and the Empire, 1562-1955*. Chapel Hill: UNC Press.

Goldin, Claudia (1976). *Urban Slavery in the American South, 1820-1860: A Quantitative History*. Chicago, IL: University of Chicago Press.

Harris, Milton and Bengt Holmstrom (1982). “A Theory of Wage Dynamics,” *Review of Economic Studies*, 49(3), pp. 315-333.

Hay Douglas (2004). “England, 1562-1875: The Law and Its Uses,” in Hay, Douglas and Paul Craven eds., *Masters, Servants, and Magistrates in Britain and the Empire, 1562-1955*. Chapel Hill: UNC Press.

Hay, Douglas and Paul Craven (1993). “Master and Servant in England and the Empire: A Comparative Study,” *Labour/Le Travail*, 31, pp. 175-184.

Hay, Douglas and Paul Craven (2004). *Masters, Servants, and Magistrates in Britain and the Empire, 1562-1955*. Chapel Hill: UNC Press.

Hechter, M. (1976). *U.K. County Data, 1851-1966 [computer file]*. Colchester, Essex: UK Data Archive [distributor], SN: 430.

Holdsworth, W.A. (1873). *The Law of Master and Servant*. London: Routledge and Sons.

Holmstrom, Bengt (1983). “Equilibrium Long-Term Labor Contracts,” *Quarterly Journal of Economics*, 98(Supplement), pp. 23-54.

Huberman, Michael (1996). *Escape from the Market: Negotiating Work in Lancashire*. New York: Cambridge University Press.

Judicial Statistics, England and Wales, 1858-1875.

Krueger, Alan and Bruce D. Meyer (2002). “Labor supply effects of social insurance,” *Handbook of Public Economics*, Volume 4, pp. 2327–2392.

von Lilienfeld-Toal, Ulf and Dilip Mookherjee (2010). “The Political Economy of Debt Bondage,” *American Economic Journal: Microeconomics*, 2(3), pp. 44-84.

Macdonald, Alexander (1868). *Handybook of the Law Relative to Masters, Workmen, Servants, and Apprentices*. Glasgow: William Mackenzie.

Malcomson, James M. (1997). “Contracts, Hold-Up, and Labor Markets,” *Journal of Economic Literature*, 35(4), pp. 1916-1957.

Marinescu Ioana (2008a), “Are Judges Sensitive to Economic Conditions? Evidence from U.K. Employment Tribunals,” University of Chicago, mimeo.

McCloskey, D.N. (1973). *Economic maturity and entrepreneurial decline: British Iron and Steel, 1870-1913*. Harvard University Press

Marx, Karl (1887). *Capital Vol. 1*. Moscow: Progress Publishers.

Marx, Matt, Debbie Strumsky, and Lee Fleming (2007). “Noncompetes and Inventor Mobility: Specialists, Stars, and the Michigan Experiment,” Harvard Business School, mimeo.

Minerals. Return of the quantities of coal, iron ore, copper ore, lead ore, tin ore, silver, zinc ores, salt, and other minerals, raised in each of the mining counties of the United Kingdom, for the years 1854 and 1855 respectively. P.P. 1856 (328).

Mitchell, Brian R. (1988). *British Historical Statistics*. New York: Cambridge University Press.

Mukherjee, Anindita and Debraj Ray (1995). “Labor Tying,” *Journal of Development Economics*, 47(2), pp. 207-239.

Naidu, Suresh (2010). “Recruitment Restrictions and Labor Markets: Evidence from the Post-Bellum U.S. South,” *Journal of Labor Economics*, Vol. 28(2), pp. 413-44.

Nickell, Stephen (1981). “Biases in dynamic models with fixed effects,” *Econometrica*, 49(6), pp. 1417–1426.

Oman, Nathan B. (2009). “Specific Performance and the Thirteenth Amendment,” *Minnesota Law Review*, 93(6), pp. 2020-2099.

Orsagh, T.J. (1961), “Progress in Iron and Steel: 1870–1913,” *Comparative Studies in Society and History*, 3(2) pp. 216-230.

Ransom, Roger L. and Richard Sutch (1977). *One Kind of Freedom: The Economic Consequences of Emancipation*. Cambridge, UK: Cambridge University Press.

Report from the Select Committee on Master and Servant. P.P. 1865 (370).

Report from the Select Committee on Master and Servant. P.P. 1866 (449).

Robson, R. (1957). *The Cotton Industry of Britain*. London: Macmillan.

Sadoulet, Elisabeth (1992). “Labor-service Tenancy Contracts in a Latin American Context,” *American Economic Review*, 82(4), pp. 1031-1042.

Second and final report of the commissioners appointed to inquire into the working of the Master and Servant Act, 1867. P.P. 1875 (C.1157) (C.1157-I).

Southall, H.R., D.R. Gilbert, and I. Gregory (1998). *Great Britain Historical Database: Labour Markets Database, Trade Union Statistics, 1851-1918*, UK Data Archive [distributor], SN: 3712.

Southall, H.R., D.R. Gilbert, and I. Gregory (1999). *Great Britain Historical Database: Labour Markets Database, Statistics of Wages and Hours of Work, 1845-1913*, UK Data Archive [distributor], SN: 3710.

Southall, H.R., D. Dorling, E.M. Garrett, D.A. Gately, D.R. Gilbert, C. Lee, A. Reid, and M. Woollard (2004). *Great Britain Historical Database: Census Data: Occupational Statistics, 1841-1991*, UK Data Archive [distributor], SN: 4559.

Steinfeld, Robert J. (1991). *The Invention of Free Labor*. Chapel Hill: The University of North Carolina Press.

Steinfeld, Robert J. (2001). *Coercion, Contract, and Free Labor in the Nineteenth Century*. Cambridge, UK: Cambridge University Press.

Steinberg, Marc W. (2003). “Capitalist Development, the Labor Process, and the Law,” *American Journal of Sociology*, 109(2), pp. 445-495.

Tillyard, Frank (1916). *Industrial Law*. London: A. & C. Black, Ltd.

Webb, Sidney and Beatrice Webb (1902). *The History of Trade Unionism*. London: Longmans, Green, and Co.

Wright, Gavin (2006). *Slavery and American Economic Development*. Baton Rouge: LSU Press.

Appendix 1A: Enactment and Enforcement of Master and Servant Law

The early labor market regulations (the Statute of Laborers and the 16th century Statute of Artificers) most clearly applied to agricultural workers.¹ The development of the industrial economy necessitated a clarification of the legal relationship between employer and employee in new sectors of the economy. Uncertainty regarding the scope of the early labor laws resulted in a series of enactments that extended the penal enforcement of labor contracts (see Table A1 for the timing of important labor law enactments in Britain).² In the 19th century, the 1823 Master and Servant Act used “broad language that could be read to cover the overwhelming majority of manual wage workers.”³

Table A1 : Master and Servant Acts and Related Legislation

Year	Act	Coverage or Action
1349/1351	Statute of Laborers (25 Edw. III st. 2)	All but artisans and landholders required to work for set wages
1562/1563	Statute of Artificers (5 Eliz. c. 4)	"
1720	7 Geo. I, stat. I, c. 13	Journeymen tailors
1722	9 Geo. I, c. 27	Journeymen shoemakers
1747	20 Geo. II, c. 19	Artificers, handicraftsmen, miners, colliers, and others
1813	53 Geo. III, c. 40	Repeals wage setting provisions of 1563 statute
1823	Master and Servant Act (4 Geo. IV c. 34)	Codifies the general use of penal sanctions for contract breach
1844	Failed Master and Servant Act Reform	Attempts to extend and clarify 1823 Act
1867	Lord Elcho's Act (30 and 31 Vict. c. 141)	Fines become standard punishment
1871	Trade Union Act (34 and 35 Vict. c. 31)	Officially legalizes unions
1871	Criminal Law Amendment Act (34 and 35 Vict. c. 32)	Makes union activity illegal when individual behavior illegal
1875	Employers and Workmen Act of 1875 (38 and 39 Vict. c. 90)	De-criminalizes contract breach
1875	Conspiracy and Protection of Property Act (38 and 39 Vict. c. 86)	Regulates union behavior

Still, in 1844, an attempt was made to further extend (and clarify) the 1823 Master and Servant Act. Organized labor moved strongly against the proposed reform; Frank (2004)

¹The Statute of Laborers is 25 Edw. III st. 2; the Statute of Artificers is 5 Eliz. c. 4.

²Master and Servant laws were specifically extended to cover journeymen tailors in 1720, journeymen shoemakers in 1722, woolcombers and weavers in 1725, individuals in the leather trades in 1740, and to a broad range of workers in 1747 (artificers, handicraftsmen, miners, colliers, keelmen, pitmen, glassmen, potters, and others). See Steinfeld (2001, p. 42, n. 14).

³Steinfeld (2001), pp. 47. The 1823 Master and Servant Act is 4 Geo. IV c. 34.

writes that dozens of workers' meetings were held, and petitions were presented to Parliament against the reform bill, which failed to pass.

Another attempt at revising Master and Servant law was made in 1867; this time, it was successful.⁴ The 1867 reform had an ambiguous effect on the severity of punishment for breach of contract. On the one hand, the reform removed some of the coercive teeth from the 1823 Act: it made fines the standard punishment for breach of contract, moving labor contract breach toward civil procedure, and away from criminal. On the other hand, the 1867 law allowed for an order of specific performance of a contract's terms – a magistrate could simply order an employee to go back to work.⁵ Moreover, for employees who could not pay their fines, imprisonment was the penalty; severe, coercive sanctions remained a potential consequence of breach of contract by the employee.

Historians have written on the penal enforcement of contracts in industry. Frank (2004) writes, “The penal clauses of master and servant law were a particular grievance for miners in Northumberland and Durham, where mine owners used it to support their system of labor contracting and labor discipline.” Both Steinberg (2003, p. 475) and Steinfeld (2001, p. 67) cite cases involving prosecution of iron workers.⁶ Huberman (1996, p. 53) describes textile mills using Master and Servant prosecutions to retain labor and elicit greater worker effort, writing, “[The Horrockses Mill] regularly prosecuted operatives for quitting work without notice, for absenteeism, and for other acts of indiscipline . . . and many of the leading mills [in Preston] shared its labor market strategy.”

Frank (2004, p. 418) also suggests that the proceedings were far from impartial: “The *Potters’ Examiner*,” he writes, “objected that ‘The powers of the manufacturers will become omnipotent, as the magisterial benches are nearly wholly filled by themselves.’”⁷ Steinberg

⁴Known as Lord Elcho’s Act, the 1867 Master and Servant Act is 30 and 31 Vict. c. 141.

⁵This is in contrast with modern law in the United States, where an order for the specific performance of a labor contract is generally viewed as a form of involuntary servitude, and thus a violation of the thirteenth amendment to the U.S. Constitution. See Oman (2009) for a discussion.

⁶These cases are discussed by witnesses before Lord Elcho’s Commission as well. *Report of the Select Committee on Master and Servant* (1866), testimony of Mr. John W. Ormiston.

⁷The quote comes from an article published April 6, 1844.

(2003, p. 458) writes that “by the mid-Victorian period . . . [w]orkers and their sympathizers frequently bemoaned the elite stranglehold on the law.” Others shared this view: Lord Elcho’s Parliamentary Commission on Master and Servant (in 1866) acknowledged inequality in Master and Servant proceedings, especially in mining.⁸

Thus, the law was broadly, and successfully, applied by employers across industries. In principle, Master and Servant law could have served multiple purposes: it could have been used to prevent shirking (as in a simple principal-agent model), it could have been used to prevent strikes, and it could have been used to retain labor when workers had signed long-term contracts (i.e., to prevent absconding). Our focus in this paper is on the use of Master and Servant law for the last of these purposes.

It is difficult to know exactly how Master and Servant law was applied in practice, because detailed information on individual cases is generally not available. We discovered one valuable source (*First report of the commissioners appointed to inquire into the working of the Master and Servant Act, 1867*, 1874) that provides descriptions of several hundred individual cases from 60 districts from across Britain between 1867 and 1874. We coded these cases as having been brought for one of the three reasons noted above (or for another reason, though the vast majority were brought for shirking, for organized labor activity, or for early termination of the contract by the employee). In addition, we coded the cases conservatively so as not to bias numbers in favor of our focus in the paper: in cases where absconding from the employer was mentioned, but repeated misbehavior was mentioned as well, we always coded the case as belonging to the shirking category.

Having coded the cases, we calculate for each district that reported case information the fraction of Master and Servant cases brought for each of the three reasons. We then examine the distribution of shares of cases of different types across districts. We find (see Figure A1), that nearly 50% of districts had *100% of their cases* brought against workers for absconding; the median district had around 70% of its cases brought for workers’ absconding.

⁸Macdonald (1868), p. 184; *Report of the Select Committee on Master and Servant*, (1866).

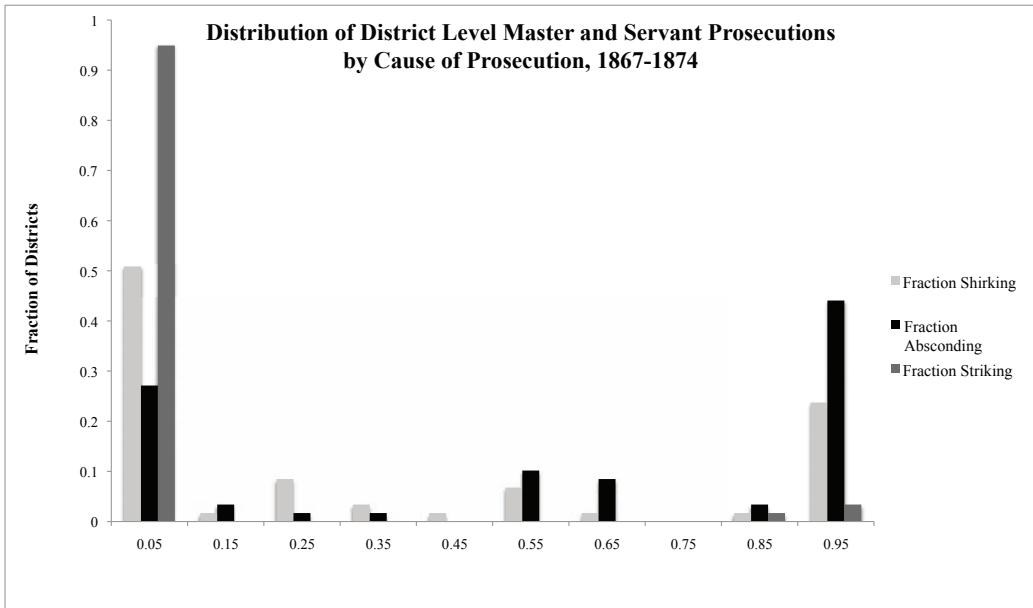


Figure A1: Distribution of types of Master and Servant cases across British districts, 1867-1874. Graph shows the fraction of districts with cases of a certain type occurring a particular fraction of the time. (The middle bar in the bin labeled “0.95” shows that nearly 50% of districts had between 90% and 100% of their cases brought against workers for absconding from their employers.) The case information comes from the *First report of the commissioners appointed to inquire into the working of the Master and Servant Act, 1867* (1874).

Prosecution for shirking (broadly defined) is of less significance: over half of districts had exactly 0% of their cases brought for shirking. Prosecutions for organized labor activity were even less common: the vast majority of districts had none of these. Anecdotal evidence from Parliamentary Reports and these more systematic data, though imperfect, strongly suggest that the main use of Master and Servant law was to punish workers for absconding, precisely the focus of our model.

Appendix 1B: Summaries of Master and Servant Cases Reaching Appellate Courts

In the case of *Unwin and others versus Clarke* (1 QB 417, April 28, 1866), the court decided that imprisonment for breach of contract did not terminate the contract, and that further imprisonment was available as a punishment if a worker did not return to his master's employment. The worker was required to serve out his contract, or he would be sent repeatedly to prison. In *Cutler versus Turner and another* (9 QB 502, June 3, 1874), the court made it clear that until the repeal of penal sanctions for breach of labor contracts, imprisonment was seen and used as a legitimate punishment of employees who breached their contracts.

Summary of *Unwin and others versus Clarke*, 1 QB 417, April 28, 1866

A workman entered into a contract with a master to serve him for the term of two years; he absented himself during the continuance of the contract from his master's service, and under 4 Geo. 4, c. 34, s. 3, he was summoned before justices, convicted, and committed [to prison]. After the imprisonment had expired, and while the term still continued, he refused to return to his master's service, and was again summoned before justices, when he stated that he considered his contract determined by the commitment [that is, he believed his contract was terminated due to his having served time in prison]; the justices found that he bona fide believed that he could not be compelled to return to his employment, and dismissed the summons. Held, that although the servant had not returned to the service, yet, as the contract continued, he had been guilty of a fresh offence, for which, notwithstanding his conviction and imprisonment, he could be again convicted; and that his bona fide belief that he could not be compelled to return to his employment did not constitute a lawful excuse for his absence.

Summary of *Cutler versus Turner and another*, 9 QB 502, June 3, 1874

The appellant, in 1871, agreed to serve the respondents as a fire-iron forger for five years. On the 1st of April, 1873, he was summoned under the Master and Servant Act, 1867 (30 &

31 Vict. c. 141), for absenting himself from the respondents' service, and was, on the 13th of May, ordered to pay £11 8s. to them as compensation for the breach of contract, which sum was paid. Not having returned to his employment, the appellant was again summoned and, on the 7th of July, ordered to fulfill his contract and to give security for its fulfillment, and in default to be imprisoned for a term not exceeding three months. The appellant did not comply with the order and underwent three months imprisonment. On his liberation he continued to absent himself, and was again summoned for absenting himself from the respondents' service, and ordered, on the 18th of November, to pay £11 14s. to them as compensation. Held, that, upon the true construction of s. 9 of the Act, the orders of the 13th of May and the 7th of July did not annul the contract of service, and were no bar to the subsequent summons and order of the 18th of November; and that that order was rightly made.

Appendix 2: Additional Results

We first present results of regressions related to our use of iron ore production as an instrument for pig iron production and distance to Lancashire as an instrument for the share of employment in textiles (see Table 2, columns 7 and 8). In Table A2, columns 1-4, we show the results from the first-stage regressions of the endogenous variables on their instruments, with and without time varying controls.⁹

Table A2: First Stage and Reduced Form Results Using Geographic Instrumental Variables

Outcome:	First Stage Regressions				Cross Sectional Relationship		Reduced Form	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Iron Shock	Textile Shock	Iron Shock	Textile Shock	Iron County	Fraction in Textiles	MS Prosecutions	MS Prosecutions
Log(Ore Output) X Log(Iron Price)	0.0513*** (0.0115)	-0.0001 (0.0004)	0.0530*** (0.0107)	0.0001 (0.0004)			4.634* (2.314)	4.781* (2.572)
Distance to Lancashire X Log(Cotton Price Ratio)	-0.0017 (0.0010)	-0.0060*** (0.0016)	-0.0026** (0.00102)	-0.0042*** (0.0012)			-1.037** (0.400)	-0.770** (0.313)
Coal County X Log(Coal Price)	0.124 (0.116)	0.0081 (0.0051)	0.116 (0.128)	0.0045 (0.0048)			37.65*** (11.37)	37.82** (16.07)
Log(Ore Output)					0.0472*** (0.0111)	-0.0009 (0.0019)		
Distance to Lancashire					-0.0269*** (0.0077)	-0.0043*** (0.0010)		
Coal County					-0.131 (0.174)	0.0269 (0.0255)		
F-statistic: p-value of panel instruments	0.0000	0.0006	0.0000	0.0008				
F-statistic: p-value of cross-sectional instruments					0.0000	0.000356		
F-statistic: p-value of joint industry shocks							0.0000	0.0004
Time-Varying Controls	N N	N 3942	Y 3942	Y 3942	N 219	N 219	N 3942	Y 3942

Dependent variable noted above each column. Standard errors, clustered on county, included in parentheses. All columns except (5) and (6) include district and year fixed effects. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. * p<0.1, ** p<0.05, *** p<0.01

The two dependent variables are the two endogenous labor demand shocks: our standard iron county shock, the interaction of a pig iron production dummy variable with the log of the pig iron price, and our standard textile county shock, the interaction of the county's employment share in textile production with the log textile output to input price ratio. The two explanatory variables of interest are the two instruments *for labor demand shocks*: in pig iron producing areas, the instrument is the interaction of iron ore output with the

⁹Table A2, columns 1 and 2 are the two first stage regressions from Table 2, column 7; Table A2, columns 3 and 4 are the two first stage regressions from Table 2, column 8.

log of the pig iron price; in textile producing areas, the instrument is the interaction of a county's distance to Lancashire with the log textile output to input price ratio. We control for district and year fixed effects and the log of population, as well as the coal labor demand shock variable (these controls are all included in the second stage, so we include them in the first stage as well). In columns 3 and 4, we include time varying controls as in Table 2, column 8.

As we would expect, the *iron ore X log iron price* variable is positive and highly significant in column 1, where the iron county labor demand shock is the outcome. This reflects the much greater likelihood that pig iron will be produced where iron ore is available.¹⁰ In column 1, both the textile instrument and the coal county shock are insignificant. In column 2, the textile county labor demand shock is the outcome variable and, consistent with our expectations, we find that the textile instrument is negative and significant. This reflects the falling employment share in textiles as the distance to Lancashire increases. In column 2, both the the iron instrument and the coal county shock are insignificant. In columns 1 and 2, the joint F-statistic on the two instruments is significant at well below the 1% level.

In columns 3 and 4, the we estimate the specifications from columns 1 and 2, but add the time-varying effects of baseline county characteristics as controls. Again, we find that our proposed instruments are strongly correlated with the endogenous regressors in the direction expected. The only notable difference is that the distance to Lancashire interacted with the log textile output price ratio now predicts the iron shock as well (see column 3). We acknowledge that this finding of a correlation between distance to Lancashire and iron ore production is potentially a threat to our identification strategy. We hope that our control for iron county labor demand shocks in our 2SLS regressions partials out any effect of distance to Lancashire on master and servant prosecutions, other than through its effect on textile production (though of course, we cannot prove this is true).

¹⁰It also reflects the mechanical correlation driven by the same time-series variation in industry prices in both the dependent and independent variable. This point applies to columns 2-4 as well, and is taken up again below.

Much of the strength of the relationship between the instruments and the endogenous regressors in Table A2, columns 1-4, is a mechanical correlation driven by the same time-series variation in industry prices in both the dependent and independent variables. In Table A2, columns 5 and 6, we thus examine the *cross-sectional* relationship between the endogenous industry presence variables (the iron county dummy and the employment share in textiles) and the geographic instruments – the production of iron ore and the distance to Lancashire. We include district and year fixed effects and also include the coal county dummy variable as a control. The relationship between the cross-sectional instruments and the endogenous regressors is strong, significant, and of the expected sign.¹¹. Again, the joint significance of the instruments is well below the 1% level.

Finally, in Table A2, columns 7 and 8, we report the reduced form regressions corresponding to the 2SLS regressions reported in Table 2 (one column with and one column without time varying controls). Both the iron ore interaction with log iron price and the distance to Lancashire interacted with the log textile price ratio are strong, significant predictors of district level prosecutions, with the expected signs.

One might wonder if high prosecutions in a particular place and time cause *future* movements in prices (our measure of labor demand). We thus examine whether prosecutions are significantly correlated with future prices, conditional on lagged and contemporary prices. Also of interest is whether the contemporary demand shocks remain strong predictors of prosecutions after one partials out lagged and leading price variation.

Table A3 explores the dynamics of the relationship between master and servant prosecutions and labor demand shocks, adding two leads and two lags of labor demand shocks to the main specification's contemporaneous shocks. Given the short length of our panel (1858-1875) we lose a number of observations, as well as some of the independent variation needed to precisely estimate any one coefficient. We estimate the following model:

¹¹This is true whether we include time varying controls or not; we present results without controls for brevity.

$$\begin{aligned}
Prosecutions_{dct} = & \sum_{k=-2}^2 \beta_{Industry,k} Industry_c \times \log(IndustryPrice_{t+k}) \\
& + \delta_d + \delta_t + \sum_{t=1858}^{1875} \beta_t X_{c,1851} + \beta_2 \log(pop_{ct}) + \epsilon_{dct}
\end{aligned}$$

In Table A3, we present the resulting coefficients on the various labor demand shocks, $\beta_{Industry,k}$. In column 1, we exclude the time varying controls, $\beta_t X_{c,1851}$ from the regression; in column 2, we show results including them. In both columns, all of the leads are *not* significantly different from zero, either individually or jointly, rejecting the hypothesis that prosecutions significantly affect future prices. Generally, the magnitudes of the leading coefficients are small as well, though the textile industry shocks are quite large and imprecisely estimated.

Coal exhibits evidence of a negative one-year lagged effect of prices on prosecutions, which may reflect relatively long contracts in the industry linking low prices (and wages) in one year to greater breach and prosecution if prices are high in the following year.¹² There is also some weak evidence of a delayed positive effect of iron prices on prosecutions, when the controls are added, and the lagged effects are jointly significant.

The contemporaneous labor demand shocks, while only individually significant in the coal industry, all have large coefficients of positive sign, and are jointly significant with and without controls. These results, when taken together, suggest that while the industry-specific labor demand shocks may have effects on prosecutions over multiple years, the contemporaneous effect stands out, and it does not seem to be the case that future industry shocks “predict” prosecutions.

We next consider the effects of the 1867 reform of Master and Servant law. It is natural to wonder whether the change had an effect on the estimated relationship between labor de-

¹²We also find a negative lag in the iron industry, though it is not significant. The insignificant (positive) lag in the textile industry might be the result of shorter contracts in this relatively urban industry.

Table A3: Leading and Lagged Labor Demand Shocks' Effects on Prosecutions

<i>Contemporaneous labor demand shocks</i>	(1)	(2)
Textiles	438.9 (938.2)	448.3 (625.6)
Iron	72.01 (48.51)	76.57 (52.97)
Coal	132.7*** (41.46)	138.4** (61.85)
F-test: p-value same-year labor demand shocks on prosecutions	0.0003	0.0147
<i>Lagged labor demand shocks</i>		
Textiles lagged 1 year	164.6 (553.4)	115.5 (524.4)
Textiles lagged 2 years	-193.1 (128.9)	-190.5 (151.5)
Iron lagged 1 year	-13.46 (18.85)	-6.175 (18.77)
Iron lagged 2 years	13.37 (31.99)	55.18* (30.11)
Coal lagged 1 year	-207.8** (83.38)	-213.1** (90.22)
Coal lagged 2 years	72.44 (66.54)	137.5 (182.6)
F-test: p-value preceding years' labor demand shocks on prosecutions	0.0015	0.0007
<i>Leading labor demand shocks</i>		
Textiles leading 1 year	-310.6 (354.6)	-329.8 (328.2)
Textiles leading 2 years	-86.80 (344.7)	40.56 (333.1)
Iron leading 1 year	-12.97 (20.84)	-16.21 (26.44)
Iron leading 2 years	22.00 (26.31)	43.19 (39.21)
Coal leading 1 year	11.57 (34.37)	18.26 (40.04)
Coal leading 2 years	7.732 (9.654)	19.49 (33.80)
F-test: p-value following years' labor demand shocks on prosecutions	0.838	0.769
Time-Varying Controls	N	Y
N	3066	3066

Dependent variable is absolute number of master and servant prosecutions. Standard errors, clustered on county, included in parentheses. Both regressions include district and year fixed effects. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. * p<0.1, ** p<0.05, *** p<0.01

mand shocks and prosecutions. To test this, we estimate the basic district-level prosecutions specifications from Table 2, columns 1-4, separately on the 1858-1867 (inclusive) and the

1868-1875 (inclusive) time periods. Results for both sub-samples, presented in Table A4, are consistent with estimates from the full sample: all of the labor demand shocks are positive, and most are statistically significant across specifications.

Table A4: Labor Demand Shocks and Master and Servant Prosecutions, Before and After 1867 Master and Servant Act

	Years: 1858-1867				Years 1868-1875			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Textiles 1851 X Log(Cotton Price Ratio)	107.3*** (35.61)			106.3*** (34.89)	639.0*** (142.9)			524.9** (119.5)
Iron County X Log(Iron Price)		4.161 (30.37)		5.395 (31.05)		68.02*** (22.33)		57.94** (21.68)
Coal County X Log(Coal Price)			49.28* (25.72)	47.16* (26.92)			52.03*** (15.34)	18.15* (10.51)
Log(Population)	89.46*** (31.72)	69.18** (25.99)	30.49 (28.89)	51.36* (25.78)	323.5*** (94.10)	356.9*** (98.26)	275.8** (107.5)	207.9*** (76.33)
F-statistic p-value on joint significance					0.0121			0.0001
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
N	2190	2190	2190	2190	1752	1752	1752	1752

Dependent variable is absolute number of master and servant prosecutions. Standard errors, clustered on county, included in parentheses.

* p<0.1, ** p<0.05, *** p<0.01

In the pre-reform period (columns 1-4), textile and coal labor demand shocks are significant when included alone as explanatory variables or when all three industries' labor demand shocks are included. In the latter specification, the joint test of the labor demand shocks in the three industries is significant as well. The exception to this pattern is that labor demand shocks in the iron industry have a relatively small, statistically insignificant effect in the pre-reform period. One possible explanation for the small coefficient is the small amount of variation in iron prices over the 1858-1867 period, around *half* of the variation in coal prices, and *one-sixth* of the variation in textile prices.¹³

In the post-reform period (columns 5-8), each of the labor demand shocks is estimated to have a large, statistically significant effect across specifications. When the three industries' shocks are included in the same specification, the joint test of their significance is highly significant as well. Overall, we view the results in Table A4 as supporting our analysis of the entire 1858-1875 period, and also suggesting that the reform of Master and Servant law

¹³The standard deviations of iron, coal, and textile prices, looking only at the time-series variation between 1858 and 1867 (inclusive), are .056, .112, and .353, respectively.

in 1867 did not greatly change its use or function.

As we note in the text, one might naturally be concerned that the particular method and sources used in the construction of our wage index biases our estimates. To address several specific questions about the robustness of our results on wage variability and wage level changes following the repeal of penal sanctions, we estimate the main specifications from Tables 5 and 7 using a variety of alternative wage indices. We have estimated analogous tables checking the robustness of Table 6 to various wage indices, and they resoundingly confirm the results in Table 6.¹⁴

In Table A5, we estimate the change in wage levels in high- versus low-prosecution counties pre- versus post-repeal¹⁵; in Table A6, we estimate the change in the response of wages to labor demand shocks.¹⁶

Table A5: Alternative Wage Indices: Effect of Repeal on Wage Levels, by Average Prosecutions

	Interpolated Weights		Smiths' Wages		Male and Female Shares		Unskilled Workers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-1875 X Log(Average Prosecutions)	0.0186** (0.0086)	0.0063*** (0.0023)	0.0194** (0.0075)	0.0054*** (0.0016)	0.0204** (0.0085)	0.0048*** (0.0017)	0.0210** (0.0084)	0.0054*** (0.0018)
Time-Varying Controls	N	Y	N	Y	N	Y	N	Y
Interpolated Controls	N	Y	N	Y	N	Y	N	Y
Union Membership	N	Y	N	Y	N	Y	N	Y
County-specific recession effect	N	Y	N	Y	N	Y	N	Y
N	2860	2392	2808	2340	2860	2392	2860	2392

Dependent variable is the log of county wages, where wages are calculated using different methods and sources depending on the column. Please refer to the text and data appendix for a discussion of the different wage indices. Standard errors, clustered on county, included in parentheses. All regressions include county and year fixed effects. Regressions in the even-numbered columns include lagged wages. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. The interpolated controls are interpolated population, income, proportion urban, and population density between census years. Union membership is from the Amalgamated Society of Engineers, measured at the county-year level. The county-specific effect of a recession is a recession indicator (taken from peaks and troughs between 1860 and 1905 noted in Ford, 1981) interacted with a set of county dummy variables. * p<0.1, ** p<0.05, *** p<0.01.

First, one might be concerned about our use of 1851 county-level occupational distributions to weight the various occupation-specific wage series from which we construct the wage index. We thus construct a wage index using the same occupation-specific wage series, but

¹⁴Because we already include two robustness tables checking our results from Table 6 in this appendix (below), we omit these additional checks for brevity.

¹⁵The specifications in Table A5 are identical to Table 5, columns 1 and 5 in the text, other than the use of various wage indices in Table A5 in place of the standard wage series outcome used in Table 5. Our results are robust to a wide range of additional specifications.

¹⁶The specifications in Table A6 are identical to Table 7, columns 1, 2, and 5 in the text, other than the use of various wage indices in Table A6 in place of the standard wage series outcome used in Table 7. As with the wage levels robustness checks, our results are robust to a wide range of additional specifications.

using occupational distributions that vary at the county-year level as weights (specifically, using occupational distributions interpolated between census years). We find our results qualitatively unchanged using this wage index: wages are estimated to rise (significantly) in high-prosecution counties following the repeal of penal sanctions and wages respond more to labor demand shocks in textiles and coal following repeal.¹⁷

Next, one might worry that we use a small number of occupation-specific wage series in constructing our wage index. We thus constructed another alternative wage index in which we include an additional occupation-specific wage series: the wages of iron smiths.¹⁸ In our construction of a baseline wage index, we did not include this series because the closest occupational category in the census that might be used to weight this series covers all workers producing metal goods – for these workers iron might be an input of production, rather than output. While we would not feel comfortable using metal workers' occupational share to weight smiths' wages in our baseline index, we feel that as a check of the robustness of our results, it is reasonable to add smiths' wages using metal workers' shares as weights. Thus, in Table A5, columns 3 and 4, and Table A6, columns 4-6, we present estimates as in Table A5, columns 1 and 2, and Table A6, columns 1-3, respectively, but using a wage index with 1851 occupational distributions as weights (as in the baseline), and including smiths' wages. We find that our estimated effects in both Tables look very similar to results using our baseline wage index, both in terms of magnitudes and statistical significance.¹⁹

Another question is whether the occupational share weights – which in the baseline wage index were based on the male working population – ought to have considered the female occupational distribution as well.²⁰ We thus construct another alternative wage index using

¹⁷See Table A5, columns 1 and 2, and Table A6, columns 1-3. The textile labor demand shock in Table A6, column 3, is no longer statistically significant, though it is actually larger than the coefficient estimated using the same specification and the original wage index.

¹⁸Smiths' wages come from Bowley and Wood (1906). These wage data are available only through 1904, so our regressions include one fewer year of observations using this alternative wage index.

¹⁹As in Table A6, column 3, the textile labor demand shock in Table A6, column 6, is no longer statistically significant, though it is very similar in magnitude to the coefficient estimated using the same specification and the original wage index.

²⁰Master and Servant law applied to women as well as men, and some important sectors, such as agriculture and especially textiles and clothing production, employed many women.

Table A6: Alternative Wage Indices: Reduced Form Sectoral Shocks on Wages, Pre- and Post-Repeal of Penal Sanctions

	Interpolated Weights			Smiths' Wages			Male and Female Shares			Unskilled Workers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-1875 X Textile County X Cotton Price	0.601*** (0.0937)	0.439*** (0.163)	0.104 (0.0717)	0.482*** (0.0780)	0.240** (0.111)	0.0521 (0.0388)	0.561*** (0.0752)	0.255** (0.110)	0.0611* (0.0322)	0.505*** (0.0920)	0.265** (0.123)	0.0758* (0.0422)
Post-1875 X Iron County X Iron Price	-0.0313 (0.0244)	0.175*** (0.0355)	-0.0028 (0.0148)	-0.0279* (0.0156)	0.118*** (0.0251)	0.0005 (0.0088)	-0.0216 (0.0151)	0.101*** (0.0269)	-0.0012 (0.0098)	-0.0243 (0.0179)	0.127*** (0.0278)	-0.00330 (0.0112)
Post-1875 X Coal County X Coal Price	0.134*** (0.0267)	0.153*** (0.0249)	0.0538*** (0.0159)	0.0677*** (0.0191)	0.0965*** (0.0165)	0.0244** (0.0092)	0.0592*** (0.0198)	0.0958*** (0.0181)	0.0275** (0.0114)	0.0620** (0.0232)	0.103*** (0.0197)	0.0331** (0.0126)
Iron County X Log(Steel Price) Control	N	Y	N	N	Y	N	N	Y	N	N	Y	N
Time-Varying Controls	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Interpolated Controls	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Union Membership Control	N	N	Y	N	N	Y	N	N	Y	N	N	Y
Trend X County Characteristics	N	N	Y	N	N	Y	N	N	Y	N	N	Y
N	2860	2080	2808	2808	2028	2756	2860	2080	2808	2860	2080	2808

Dependent variable is the log of county wages, where wages are calculated using different methods and sources depending on the column. Please refer to the text and data appendix for a discussion of the different wage indices. Standard errors, clustered on county, included in parentheses. All regressions include county and year fixed effects. Regressions in columns (3), (6), (9), and (12) include lagged wages. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. The interpolated controls are interpolated population, income, proportion urban, and population density between census years. Linear time trends associated with county characteristics are the interaction of year with an indicator for iron county, an indicator for coal county, the fraction of the male workforce employed in textile production, and union membership in 1851. Union membership is from the Amalgamated Society of Engineers, measured at the county-year level. * p<0.1, ** p<0.05, *** p<0.01.

occupational shares from the total population (male and female) in 1851 as weights for the baseline wage series. In Table A5, columns 5 and 6, and Table A6, columns 7-9, we present estimates using this wage index. As with our other alternative wage indices, this index produces results that are nearly identical to the baseline wage index estimates: wages are estimated to rise (significantly) in high-prosecution counties following the repeal of penal sanctions and wages respond (significantly) more to labor demand shocks in textiles and coal following repeal.

A final question is whether the more skilled occupational groups for which we have wage data drive our results. Because Master and Servant law did *not* apply to white collar employees, one would expect that dropping wage series for occupations most likely to include white collar workers should not change our results. We thus construct a final wage index that excludes the wage series for engineers and shipbuilders (and their weights: the occupational shares of instrument engineering, mechanical engineering, and shipbuilding) in order to restrict attention to the wages of clearly unskilled labor. In Table A5, columns 7 and 8, and Table A6, columns 10-12, we present estimates using this alternative wage index. The effects of repeal on wage levels and variation is, if anything, slightly higher with this wage series, suggesting that our result is not being driven by relatively skilled employees.

The differential increase in wages in high prosecution counties and increased responsiveness of wages to labor demand shocks following the repeal of Master and Servant in 1875 thus appears to be robust to a wide variety of wage indices. Though we acknowledge the

imperfection of our series, and the potential for bias in any one series, the consistency of our results across a range of indices is reassuring.

Another concern regarding our estimates of the change in the response of wages to labor demand shocks post-repeal of penal sanctions is that they may capture secular trends in variables that affect the elasticity of the labor supply curve (e.g., wealth, the availability of insurance, and so on). If the labor supply curve became more inelastic across time, labor demand shocks would have a larger effect on wages even in the absence of a change in the use of long-term contracts. Our first method of examining this issue was controlling for a variety of time trends associated with particular county characteristics (see Table 7, columns 5 and 6). If secular changes that led to more inelastic labor supply were associated with counties' economic or institutional characteristics (as one might expect), these controls should reduce the estimated impact of repeal on the relationship between labor demand shocks and wages. However, we found that controlling for (linear) secular changes specific to counties producing coal, iron, and textiles, or specific to counties with high levels of initial unionization, did not change our conclusions regarding the effect of repeal on the response of wages to labor demand shocks.

Here we directly examine the elasticity of labor supply before and after 1875. We regress log unemployment²¹ on log wages, on a post-1875 indicator, and the interaction of log wages and the a post-1875 indicator.²² We find that pre-1875, changes in wages are *not* strongly associated with significant changes in unemployment – subject to caveats regarding our unemployment series, this suggests a very inelastic labor supply curve (see Table A7). The post-1875 indicator-log wage interaction is significant and *negative*, indicating that the labor supply curve is *flattening*, with a given wage increase decreasing unemployment *more*

²¹Calculated from our trade unions data; please see Appendix 4 for a discussion of the shortcomings of this unemployment measure.

²²We also include district and year fixed effects and log population as controls; in one specification we add time varying controls as well. In a simple, partial equilibrium supply and demand framework, the resulting OLS estimates can be structurally interpreted as labor supply elasticities, under the admittedly strong assumption that the (conditional) variation in wages is orthogonal to all other determinants of unemployment.

Table A7: Labor Supply Elasticity Before and After Repeal of Penal Sanctions

	(1)	(2)
Post-1875 X Log(County Wage)	-1.629* (0.814)	-2.326* (1.160)
Log(County Wage)	0.470 (0.790)	0.633 (0.816)
Log(Population)	1.063*** (0.365)	1.156*** (0.391)
District Fixed Effects	Y	Y
Year Fixed Effects	Y	Y
Time-Varying Controls	N	Y
N	1609	1609

Dependent variable is the log of the number of unemployed members of the Amalgamated Society of Engineers (ASE), measured at the county-year level. Both regressions control for the log of the number of members in the ASE. Standard errors, clustered on county, included in parentheses. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. * p<0.1, ** p<0.05, *** p<0.01

following repeal. This is additional evidence that a steepening of the labor supply curve of workers cannot explain our results.

If we are willing to push our unemployment data a bit further, we can test some auxiliary predictions of our model. Suppose that the employee in our model has a reservation wage of ϵ , where $0 < \epsilon < \bar{w}$ (that is, the employee's reservation wage is less than the equilibrium long-term contractual wage when Master and Servant law's penal sanctions are in effect). If the employee enters the spot labor market he will choose unemployment rather than accept a wage below ϵ .

Before 1875, long term contracts are signed, and because employers behave paternalistically (and because they are subject to formal and informal costs if they breach a contract), there is no unemployment before 1875. However, following the repeal of penal sanctions, long-term contracts unravel and the employee is forced to enter the spot market. With probability ϵ , he will be unemployed. This simple extension of our model suggests that there will be increases in unemployment where long-term contracts (supported by Master and Servant prosecutions) were replaced by spot market employment. One would thus expect

that districts that used Master and Servant law extensively should have experienced greater increases in unemployment following the repeal of penal sanctions.

To the extent that our unemployment data capture unemployment in the labor market as a whole, and not just the fraction of union members who are unemployed, we can test this hypothesis. In practice, we estimate the specifications used to test for higher wages in high-prosecution counties post-repeal of penal sanctions (presented in Table 5), but we replace the log county wage outcome with the county's unemployment rate.²³ We present the results of estimating these specifications in Table A8. Across specifications, we find that unemployment rates increased in high-prosecution counties relative to low prosecution counties following the repeal of penal sanctions.

Table A8: Effect of Repeal on the Unemployment Rate, by Average Prosecutions

	OLS							Arellano-Bond
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Post-1875 X Log(Average Prosecutions)	0.0096*** (0.0033)	0.0092** (0.0037)	0.0102*** (0.0036)	0.0070*** (0.0023)	0.0076*** (0.0026)	0.0083*** (0.0028)	0.0060* (0.0030)	0.0084*** (0.0028)
Population Density			0.0374 (0.0369)		0.0279 (0.0259)	0.0308 (0.0250)	0.0295 (0.0243)	0.0146 (0.0236)
Proportion Urban			0.0208*** (0.0065)		0.0152*** (0.0042)	0.0120** (0.0055)	0.0158*** (0.0044)	0.0153*** (0.0042)
Log(Income)			-0.0027 (0.0106)		-0.0020 (0.0079)	-0.0029 (0.0082)	-0.0034 (0.0089)	-0.0030 (0.0079)
Log(Pop)	0.0283*** (0.0092)	0.0274** (0.0104)	0.0066 (0.0227)	0.0204*** (0.0064)	0.0041 (0.0165)	0.0015 (0.0166)	0.0020 (0.0156)	0.0203 (0.0164)
Union Membership			-0.0026 (0.0502)	-0.0112 (0.0453)	-0.0021 (0.0362)	-0.0028 (0.0351)	-0.0205 (0.0341)	-0.0001 (0.0350)
Lagged Unemployment				0.286*** (0.0492)	0.263*** (0.0455)	0.259*** (0.0479)	0.255*** (0.0462)	0.259*** (0.0405)
Time-Varying Controls	N	Y	Y	N	Y	Y	Y	Y
Labor market controls	N	N	N	N	N	Y	N	N
Post-1875 X county controls	N	N	N	N	N	N	Y	N
County-specific recession effect	N	N	Y	N	Y	Y	Y	Y
N	1954	1954	1687	1916	1681	1680	1681	1681

Dependent variable is the unemployment rate among members of the Amalgamated Society of Engineers (ASE) at the county-year level. Counties without any ASE members are not included in the regressions. Standard errors (in parentheses) are clustered by county, except in the case of the Arellano-Bond estimator, where robust GMM standard errors are reported. All regressions include county and year fixed effects. Proportion urban, log income and log population are interpolated between census years. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. Labor market controls are the rate of union members on strike and the fraction of the population illiterate. County controls are 1851 union membership, an indicator for coal producing county, an indicator for iron producing county, and the fraction of the county's male workforce employed in textile production in 1851. The county-specific effect of a recession is a recession indicator (taken from peaks and troughs between 1860 and 1905 noted in Ford, 1981) interacted with a set of county dummy variables. * p<0.1, ** p<0.05, *** p<0.01

One might worry that differential unemployment trends existed in high- and low-prosecution counties prior to the repeal of penal sanctions. We thus examine unemployment differences between high- and low-prosecution counties by 5-year periods (using the analogous speci-

²³We also replace the lagged wage control with the lagged unemployment rate.

fication to Figure 5). In Figure A2, one sees that there is no differential unemployment rate trend (and little difference in levels) between high- and low-prosecution counties before 1875. Immediately following repeal of penal sanctions there is a large (though not statistically significant) increase in high-prosecution counties' unemployment rates (relative to low-prosecution counties). The unemployment rate difference is sustained throughout the post-repeal period (the 1881-1885 coefficient is significant at 5% and the 1886-1890 coefficient is significant at 10%).

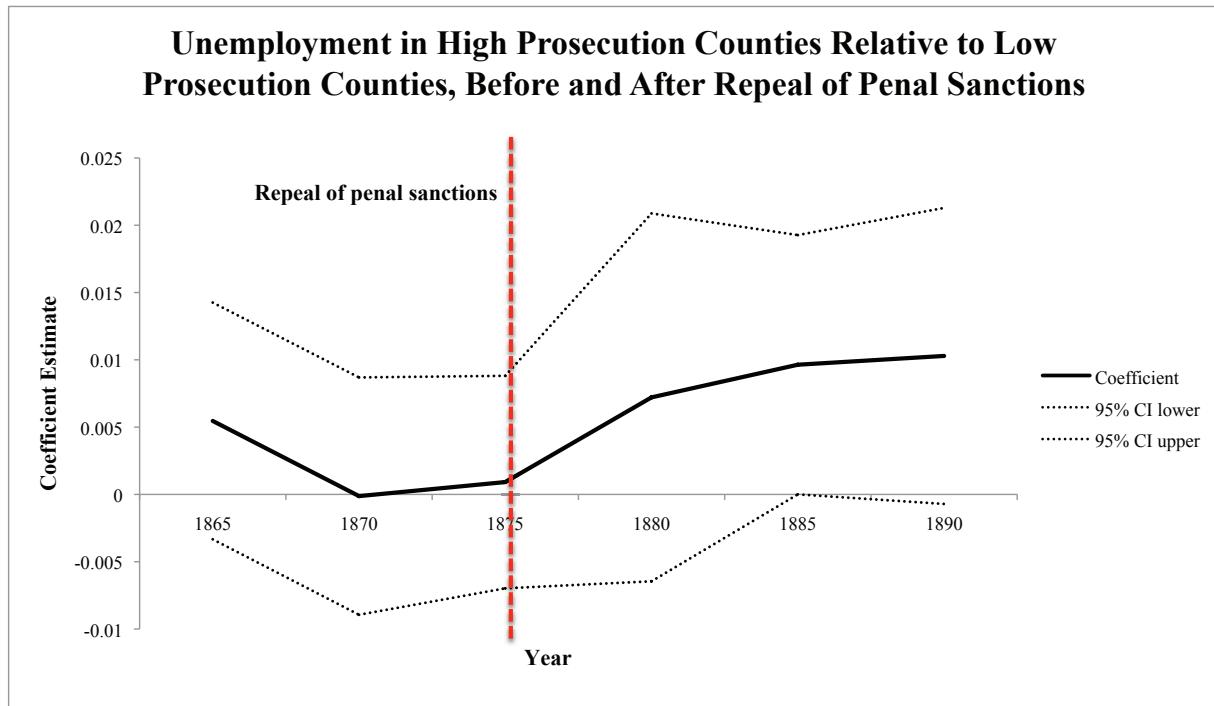


Figure A2: Unemployment rate (from the Amalgamated Society of Engineers) in high prosecution counties, relative to low prosecution counties, before and after repeal of penal sanctions. Figure plots coefficients (and their 95% confidence intervals) from a regression of the unemployment rate at the county-year level on interactions between the log of a county's average Master and Servant prosecutions per capita over the 1858-1875 period and dummy variables for five-year time periods. The coefficients from these interactions are plotted. Control variables in the regression are year and county fixed effects, county-specific recession effects, controls for county characteristics (population, population density, proportion of population that is urban, and income all interpolated between census years), year-specific controls for initial county characteristics (population density, income, proportion urban, and a Wales dummy), membership in the Amalgamated Society of Engineers, measured at the county-year level, and one-year lagged unemployment.

Table A8 and Figure A2 provide suggestive evidence consistent with our hypothesis that unemployment should increase following repeal, as employers no longer offer wage insurance against negative labor market shocks. The repeal of criminal prosecutions caused employer paternalism to unravel, which may have increased unemployment. These results should be

taken with caution, however, given the shortcomings of our unemployment rate data.

As a final question, one might wonder if the higher wages and higher unemployment rates where Master and Servant prosecutions were more common might simply reflect a greater likelihood of employers firing their employees after 1875 (and resultant compensating differentials). It is important to note that while employees' punishment for breach sharply changed in 1875, for employers, punishment for breach of contract was, and remained, a civil offense. We thus believe that increased separation rates must have been caused by the change in employees' penalties for breach of contract, rather than an exogenous change in employer firing costs.

We next examine the relationship between labor demand and wages in the pre-repeal period in more detail. In Table 6, we showed that prior to 1875, labor demand shocks were insignificantly related to wages, which, along with Figure 6, supports our model's prediction of a weakly non-monotonic relationship between wages and labor demand shocks.

Table A9 shows additional estimates of the relationship between wages and our industry labor demand shocks, restricted to the sample of years 1851-1875 (inclusive). Columns 1-4 repeat the estimates presented in Table 6, columns 1-4, but now we do not include time varying controls. In this case, we find a large, significant, and *negative* effect of labor demand shocks in textiles on wages. The effects of labor demand shocks in iron are small and insignificant, while there is a statistically significant (though small), positive effect of labor demand shocks in coal. Columns 5-8 repeat the estimates presented in Table 6, columns 1-4, but now we control for the effect of steel price shocks in iron-producing counties.²⁴ As we found in Table 6, columns 1-4, there is no significant linear relationship between labor demand shocks and wages, across our three industries.

We next check the robustness of our finding, in Table 6, columns 5-8 (supported by

²⁴We lose observations here because steel price data are not available for the entire pre-repeal period.

Table A9: Pre-Repeal Response of Wages to Labor Demand Shocks, Additional Specifications

	Excluding Time-varying Controls				Adding Steel Price Shocks			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Textiles 1851 X Log(Cotton Price Ratio)	-0.173*** (0.0596)			-0.163*** (0.0574)	0.048 (0.1050)			0.0444 (0.1160)
Iron County X Log(Iron Price)		0.0154 (0.0154)		0.00306 (0.0146)		0.00539 (0.0127)		0.00517 (0.0100)
Coal County X Log(Coal Price)			0.0366** (0.0180)	0.0346* (0.0174)			0.00206 (0.0143)	-0.00101 (0.0137)
Log(Population)	0.0777* (0.0463)	0.0814* (0.0452)	0.0629 (0.0421)	0.0602 (0.0428)	-0.0669 (0.0526)	-0.0634 (0.0476)	-0.0645 (0.0475)	-0.0663 (0.0507)
F-statistic p-value on joint significance				0.015				0.914
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Time-Varying Controls	N	N	N	N	Y	Y	Y	Y
Iron County X Log(Steel Price) Control	N	N	N	N	Y	Y	Y	Y
N	1300	1300	1300	1300	520	520	520	520

Dependent variable is the log of the county wage. Standard errors, clustered on county, included in parentheses. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 population density, 1851 proportion urban, and a Wales dummy. Sample size falls in columns (5) through (8) due to missing steel price data prior to 1864. * p<0.1, ** p<0.05, *** p<0.01

Figure 6), that wages *did* respond significantly to labor demand shocks in the *post*-repeal period. Table A10 shows additional estimates of the relationship between wages and our industry labor demand shocks, restricted to the 1876-1905 (inclusive) sample. Columns 1-4 repeat the estimates presented in Table 6, columns 5-8, but now we do not include time varying controls. The results are similar to, and in fact stronger than, those in Table 6: labor demand shocks in textiles, iron, and coal are all strongly, significantly, and positively associated with wages in the post-repeal period. As we found in Table 6, Falling steel prices – indicative of technical change – are associated with rising wages in iron-producing counties. Columns 5-8 repeat the estimates presented in Table 6, columns 5-8, but now we include the steel price shocks in the textile and coal regressions, and exclude the steel price shocks in the iron regression and the regression including all three industries' labor demand shocks. The textile and coal coefficients are quite similar to those found in Table 6, columns 5-8 (significant and positive); when steel is not controlled for, the coefficient on iron becomes negative, again highlighting the importance of controlling for the Bessemer-induced fall in the price of steel.

Finally, we show, in Figure A3, the nonparametric plot of residual prosecutions on residual labor demand shocks in iron producing counties, post-1875, without accounting for the impact of steel prices on wages in iron producing counties. The figure confirms what we

Table A10: Post-Repeal Response of Wages to Labor Demand Shocks, Additional Specifications

	Excluding Time-varying Controls				Adding Steel Price Shocks to Coal and Textile Regressions; Removing Steel Price Shocks from Iron Regression			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Textiles 1851 X Log(Cotton Price Ratio)	0.373*** (0.0677)			0.235*** (0.0621)	0.228** (0.0886)			0.221** (0.0868)
Iron County X Log(Iron Price)		0.195*** (0.0371)		0.142*** (0.0263)		-0.00859 (0.0145)		-0.0472* (0.0279)
Coal County X Log(Coal Price)			0.0899*** (0.0171)	0.0862*** (0.0184)			0.114*** (0.0231)	0.107*** (0.0214)
Iron County X Log(Steel Price)			-0.159*** (0.0370)	-0.144*** (0.0345)	-0.0590** (0.0270)			-0.0837** (0.0360)
Log(Population)	0.137*** (0.0463)	0.146*** (0.0398)	0.118*** (0.0402)	0.121*** (0.0347)	0.122*** (0.0400)	0.124*** (0.0410)	0.0956*** (0.0322)	0.100*** (0.0341)
F-statistic p-value on joint significance					0.000			0.000
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Time-Varying Controls	N	N	N	N	Y	Y	Y	Y
N	1560	1560	1560	1560	1560	1560	1560	1560

Dependent variable is the log of the county wage. Standard errors, clustered on county, included in parentheses. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 population density, 1851 proportion urban, and a Wales dummy. * p<0.1, ** p<0.05, *** p<0.01

observe in Tables 7 and A10: the relationship between labor demand shocks in iron, and wages, is not strongly monotonic if one does not take into account the effect of changing steel prices.

Post-Repeal Effect of Iron Industry Labor Demand Shock Residuals on Wage Residuals (Without controlling for Iron County x Steel Price)

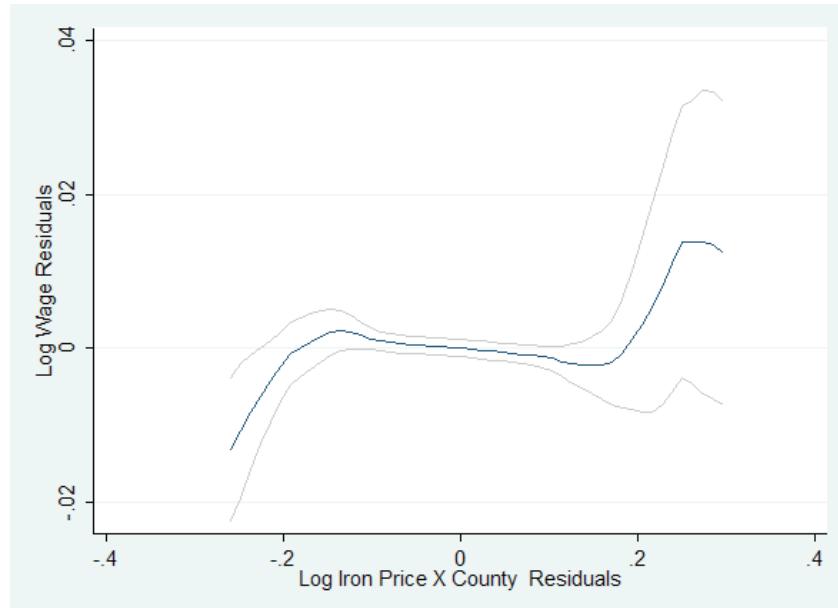


Figure A3: Wage residuals plotted against iron industry labor demand shock residuals after the repeal of penal sanctions. Control variables in the regressions are year and county fixed effects, log population, and year-specific controls for initial county characteristics (population density, income, proportion urban, and a Wales dummy).

Overall, our findings in Table 6 and Figure 6 in the text are quite robust: in the pre-repeal period, there was a weakly non-monotonic relationship between labor demand shocks and wages across industries. In the post-repeal period, there was a strong, monotonic relationship between labor demand in textiles and coal, and wages. The relationship between iron prices and wages is more complicated: iron prices may have been driven downward by the diffusion of the Bessemer process, which may have concurrently increased workers' wages. When we control for changes in steel prices, we find that iron prices are strongly and monotonically related to wages in the post-repeal period.

Table A11 replicates Table 2, but uses prosecutions for vagrancy and begging as the dependent variable. These results partially overlap with, and partially complement, those in Table 4, columns 1-2.²⁵ They are intended as a test of whether the legal system as a whole functioned differently depending on economic (i.e., labor market) conditions, perhaps owing to differential behavior by judges or constables during booms and busts. As we noted in the text, while Master and Servant prosecutions were brought by employers in response to employee breach of contract, anti-vagrancy prosecutions were brought by local law enforcement officials. If either the constabulary's or magistrates' behavior were driving the Master and Servant results, one would expect to see similar responses to labor demand shocks in anti-vagrancy prosecutions.

As can be seen in Table A11, across specifications, the effects of labor demand shocks on prosecutions for vagrancy are insignificant, with t-statistics typically less than 1, and are unstable in sign across columns, confirming the robustness of the results in Table 4, columns 1-2. Not all kinds of criminal prosecutions of poor, would-be workers responded to labor demand shocks, suggesting that our model's focus on employee breach and employer-initiated prosecutions is warranted.

²⁵Table 4 columns 1-2 are repeated in Table A11, columns 4-5.

Table A11: Reduced Form Sectoral Shocks on Vagrancy and Begging Prosecutions

	OLS						2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Textiles 1851 X Log(Cotton Price Ratio)	8.162 (86.27)			32.69 (78.37)	44.34 (74.08)	23.87 (66.71)	4.106 (72.75)	28.86 (77.91)
Iron County X Log(Iron Price)		-26.50 (43.28)		-14.71 (30.18)	12.83 (9.836)	-12.76 (12.11)	-35.95 (50.72)	1.437 (18.42)
Coal County X Log(Coal Price)				-28.34 (39.44)	-23.12 (28.48)	-11.29 (8.989)	0.898 (16.31)	-13.20 (25.07)
Log(Population)	132.6 (80.63)	143.0 (93.88)	166.3 (120.3)	164.5 (117.7)	15.21 (19.31)	91.37 (117.5)	162.0 (116.7)	16.45 (20.01)
F-statistic p-value on joint significance				0.839	0.276	0.705	0.878	0.860
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Time-Varying Controls	N	N	N	N	Y	Y	N	Y
County-Specific Trends	N	N	N	N	N	Y	N	N
N	3942	3942	3942	3942	3942	3942	3942	3942

Dependent variable is absolute number of master and servant prosecutions. Standard errors, clustered on county, included in parentheses. Time varying controls are year specific effects of 1851 income, 1851 population density, 1851 proportion urban, and a Wales dummy. Columns (1) through (6) are estimated using OLS; columns (7) and (8) use 2SLS, where distance to Lancashire is used as an instrument for employment share in textiles and iron ore production is used as an instrument for pig iron production. First stage results from columns (7) and (8) are presented in the Appendix. * p<0.1, ** p<0.05, *** p<0.01

Appendix 3: Proofs of Propositions

Proof of Proposition 1:

In order to induce the employee to sign a contract specifying a wage \bar{w} , with the risk of prosecution for breach of contract after the outside wage is determined, the employer must offer a wage that makes the employee at least as well off in expectation as he would be by simply taking the outside wage without signing the contract (the inducement to sign the contract *ex ante* is insurance against a bad draw on the spot market). Similarly, the employer must be at least as well off in expectation signing the contract as he would be hiring labor on the spot market.

Given our assumption about the distribution of outside wages (uniform on $[0,1]$), the employee expects to receive $\int_0^1 u(w)dw$ if he does not sign the contract. If he does sign the contract, he receives the following (see Figure 3 for a graphical depiction of the relevant ranges):

- The contractual wage \bar{w} if the outside wage is less than or equal to the contractual wage, and thus expected payoff $u(\bar{w})$.

- The outside wage w , if the outside wage is greater than the contractual wage, but less than the employer's prosecution-decision cut-off.
- If the employer's cut-off is below the employee's cut-off, there will be a range of outside wages greater than the employer's cut-off, but less than the employee's cut-off for which the employee receives the contractual wage \bar{w} with certainty – over this range, the employer would prosecute, and this credible threat keeps the employee from breaching the contract.
- Finally, for outside wages greater than the employer's and the employee's cut-off values, the employee receives the contractual wage less the cost of punishment if prosecution is successful, and the outside wage if it is unsuccessful.

Before proving the existence of a subgame perfect equilibrium, we define several terms, and make a simplifying assumption that allows us to focus on the case in which the employer's prosecution cut-off is less than the employee's breach cut-off.²⁶

We denote by $F(w|\bar{w})$ the lottery over *wages* (excluding the costs of being prosecuted) when the contractual wage is \bar{w} . We denote by r_s the risk premium associated with the spot market gamble, defined by $u(\frac{1}{2} - r_s) = \int_0^1 u(w)dw$. Likewise, we denote by $r_c(\bar{w})$ the risk premium associated with the analogous wage lottery $F(w|\bar{w})$. As in the main text, we assume the following:

$$u(\bar{w} + \frac{c_m}{q}) < u(\bar{w}) + \frac{qc_s}{1-q} \quad (8)$$

for any $\bar{w} \in [0, 1]$. This condition, which requires c_m to be sufficiently smaller than c_s , guarantees that $w_s(\bar{w}) > \bar{w} + \frac{c_m}{q}$ for all \bar{w} .

We can now prove the existence of a subgame perfect Nash equilibrium when $r_s - (c_m + qc_s) > 0$ is sufficiently large.

²⁶Our results do not depend on this assumption; it merely shortens our discussion of the model and its predictions.

We have constructed $B(w, \bar{w})$ and $R(w)$ already to be dominant strategies in each sub-game. It remains to be shown that there is a \bar{w} such that the employee will accept the contract, and such that the employer is better off offering a contract at \bar{w} than hiring on the spot market. For the first condition, we require that the employee's expected utility of accepting the contract is greater than his expected utility taking the spot market wage:

$$\int_0^1 u(w)dw \leq \int_0^1 u(w)dF(w|\bar{w}) - qc_s(1 - w_s(\bar{w})) \quad (9)$$

Offering the contract will be profitable for the employer if his expected payoff under the contract exceeds that under the spot market:

$$\pi - \frac{1}{2} \leq \pi - \int_0^1 wdF(w|\bar{w}) - c_m(1 - w_s(\bar{w})) \quad (10)$$

We need to show that there exists a \bar{w} such that both (9) and (10) hold.

We next write the certainty equivalent wage to a wage lottery $F(w|\bar{w})$ as $CE(\bar{w})$, so that $u(CE(\bar{w})) = E[u(w)|\bar{w}] = \int_0^1 u(w)dF(w|\bar{w})$. We can plug our definition of the certainty equivalent of the wage lottery associated with the contract into equation (9), then use the fact that the certainty equivalent of the lottery is the expected wage under the lottery less a risk premium, to re-write the employee's participation constraint as the following:

$$\int_0^1 wdF(w|\bar{w}) - r_c(\bar{w}) - qc_s(1 - w_s(\bar{w})) \geq \frac{1}{2} - r_s \quad (11)$$

As noted above, $r_c(\bar{w})$ and r_s are the risk premia associated with the contract \bar{w} and the uniform wage distribution on $[0, 1]$ in the spot market, respectively.

Equivalently, the employee requires the following:

$$\int_0^1 wdF(w|\bar{w}) - qc_s(1 - w_s(\bar{w})) - r_c(\bar{w}) + r_s \geq \frac{1}{2} \quad (12)$$

The employer's profitability constraint is satisfied if the following holds:

$$\int_0^1 wdF(w|\bar{w}) + c_m(1 - w_s(\bar{w})) \leq \frac{1}{2} \quad (13)$$

Thus, a sufficient condition for both constraints to be satisfied is:

$$\int_0^1 wdF(w|\bar{w}) - qc_s(1 - w_s(\bar{w})) - r_c(\bar{w}) + r_s \geq \frac{1}{2} \geq \int_0^1 wdF(w|\bar{w}) + c_m(1 - w_s(\bar{w})) \quad (14)$$

Suppose the employee's participation constraint is binding; then we require the following condition to hold:

$$\int_0^1 wdF(w|\bar{w}) - qc_s(1 - w_s(\bar{w})) - r_c(\bar{w}) + r_s \geq \int_0^1 wdF(w|\bar{w}) + c_m(1 - w_s(\bar{w})) \quad (15)$$

This can be rearranged to yield the following:

$$r_s - (c_m + qc_s)(1 - w_s(\bar{w})) \geq r_c(\bar{w}) \quad (16)$$

This condition is satisfied if $r_s - (c_m + qc_s)$ is sufficiently large, because $(1 - w_s(\bar{w})) < 1$.

A more intuitive form of the last inequality is the following:

$$r_s - r_c(\bar{w}) \geq (c_m + qc_s)(1 - w_s(\bar{w})) \quad (17)$$

It shows that mutually-beneficial contracts will be signed in equilibrium when the difference in the risk premia between the spot market and the contract is sufficiently high, relative to the costs to the two parties of enforcement by prosecution.

Under the assumptions specified, a \bar{w} exists that leaves both employers and employees at least as well off as entering the spot market. Because, in our model, the employer makes a contractual offer to the employee, the equilibrium contract wage will be the \bar{w} in the set of

mutually beneficial contracts that minimizes the employer's expected costs.

This concludes the proof.

Proof of Proposition 2: Proposition 2 follows immediately from the partition of the outside wage distribution induced by \bar{w} . If $w > w_s(\bar{w})$ then the worker leaves and the employer prosecutes. Otherwise we see no prosecutions. Note that for $w \in (\bar{w} + \frac{c_m}{q}, w_s(\bar{w})]$ no actual prosecutions occur, because the employee does not leave owing to the credible threat of prosecution.

Proof of Proposition 3: Define the observed wage $w^o(w)$ as a function of the spot market wage w . This is the wage observed on average given a realization of the spot market wage w . Thus we have:

$$w^o(w) = \begin{cases} \bar{w} & \text{if } w \leq \bar{w} \\ w & \text{if } \bar{w} < w \leq \bar{w} + \frac{c_m}{q} \\ \bar{w} & \text{if } \bar{w} + \frac{c_m}{q} < w \leq w_s(\bar{w}) \\ qw + (1 - q)\bar{w} & \text{if } w_s(\bar{w}) < w \leq 1 \end{cases} \quad (18)$$

Then Proposition 3 follows immediately from the observation that a labor demand shock that results in a spot market wage between $\bar{w} + \frac{c_m}{q}$ and $w_s(\bar{w})$ results in a *lower* observed wage than the observed wage resulting from a labor demand shock that produces a spot market wage between \bar{w} and $\bar{w} + \frac{c_m}{q}$.

Proof of Proposition 4: If $q = 0$, then employers never prosecute for positive c_m . Thus their expected wage bill for any contract \bar{w} is $\bar{w}^2 + (1 - \bar{w})(1 + \bar{w})/2$ which is minimized at $\bar{w} = 0$ and gives them an expected wage payment of $\frac{1}{2}$ – exactly the expected wage payment when entering the spot market. Thus employers never profit from contracted labor vis-a-vis the spot labor market, and a new equilibrium arises in which employers go to the spot market, rather than offer a contract.

Note that this implies the two other predictions. First, average wages rise after repeal, as the only wage observed is in the spot market. The average spot market wage is $\frac{1}{2}$,

and this must be greater than the average wage when prosecutions were available (under the assumption made that a contract was signed), because the employer's participation constraint implies that:

$$E[w|\bar{w}] < E[w|\bar{w}] + c_m(1 - w_s(\bar{w})) \leq \frac{1}{2} \quad (19)$$

Second, repeal increases the correlation of the observed wage and the spot market wage, and thus the labor demand shock. Note that, trivially, the observed wage (i.e., the spot market wage) responds 1 for 1 with respect to the spot market wage – the correlation of observed and spot market wages is 1 if prosecution is not available to employers. The correlation between observed and spot market wages is strictly less than 1 when prosecutions are available (under the assumption made that a contract was signed), as for any spot market wage less than the contractual wage, the observed wage does not change in response to the change in the spot market wage.

This concludes the proof.

Appendix 4: Data

In Section 3.1, we provide a brief description of the data used in our empirical analysis. Here, we provide a more detailed discussion of the sources of our data as well as the construction of variables.

Prosecutions

The prosecutions of labor-market-related criminal offenses (Master and Servant, anti-vagrancy, and anti-begging) come from *Judicial Statistics, England and Wales*, covering the years 1858–1875. These are recorded for each year at the district level under the headings “Servants, Apprentices, or Masters, Offenses relating to,” “Having no visible Means of Subsistence, &c.,” and “Begging.” We sum district-level data by county to generate county-level prosecutions for each year. The measure of anti-vagrancy prosecutions used in our empirical analysis is the sum of anti-vagrancy and anti-begging prosecutions in a district in each year.

County Characteristics

Our analysis of sector-specific labor demand shocks requires us to identify districts (in practice, counties) where iron, coal, and textile production were located in the second half of the 19th century. A continuous measure of industry presence is available for textile production: the share of the male labor force in the “textile” category in a county in 1851 (occupational distributions from British censuses are available on the UK data archive website, study 4559 (Southall et al., 2004). Because the census occupational categories that include coal mining and iron production also include employment in other sectors, we use dummy variables to indicate production of coal and of pig iron, respectively.²⁷ Our list of coal-producing counties comes from counties listed in Mitchell (1988), *Fuel and Energy*, 3 and *Fuel and Energy*, 5, compared with discussion and maps in Church (1986); counties that produced pig iron are identified from Mitchell (1988), *Metals*, 2.²⁸

²⁷Coal mining is grouped with other forms of mining, and iron production is grouped with other work related to metals.

²⁸The coal producing counties are: Carmarthenshire, Cheshire, Cumberland, Denbighshire, Derbyshire, Durham, Flintshire, Glamorganshire, Gloucestershire, Lancashire, Leicestershire, Monmouthshire, Northumberland, Nottinghamshire, Pembrokeshire, Somersetshire, Staffordshire, Warwickshire, Worcestershire, and Yorkshire. Pig iron producing counties are: Anglesey, Brecknockshire, Cardiganshire, Carmarthen-

To address concerns about the endogeneity of the location of textile and pig iron production, we use a county's iron ore production in 1855 as an instrument for pig iron production and distance to Lancashire as an instrument for the occupational share of textiles in a county in 1851. Iron ore production data come from *Minerals* (1856); distance to Lancashire is calculated as the average of every point in a county's shortest distance to the Lancashire border.

The county characteristics that we use as control variables in our analyses come from several sources. Each county's proportion urban, log income, log population, and fraction illiterate are available for the census years online, at the UK data archive website, study 430 (Hechter, 1976).²⁹ In our analyses, we either use 1851 values of these county characteristics, and allow them to have year-specific effects, or we linearly interpolate values between census years. To control for the effects of unionization on prosecutions or wages, we use data on membership in the Amalgamated Society of Engineers (ASE), measured at the county-year level. County-years with no branch membership listed are assigned values of zero. These data come from the UK data archive website, study 3712 (Southall et al., 1998). We also use the data on members of the ASE to calculate a strike rate and an unemployment rate, which we also include in some specifications as controls. The strike rate is the fraction of members receiving "contingent benefit" (i.e., strike pay), with the value undefined if a county year has no ASE members. The unemployment rate is the fraction of members receiving donation benefit (other than those members receiving contingent benefit); again, the value is undefined if a county has no ASE members in a given year.³⁰ Finally, we generate a recession indicator variable using dates of business cycle peaks and troughs between 1860 and 1914, taken from Ford (1981), and allow recessions to have county-specific effects in some of our

shire, Carnarvonshire, Cumberland, Denbighshire, Derbyshire, Durham, Flintshire, Glamorganshire, Lancashire, Leicestershire, Lincolnshire, Merionethshire, Monmouthshire, Montgomeryshire, Northamptonshire, Northumberland, Nottinghamshire, Pembrokeshire, Radnorshire, Shropshire, Staffordshire, and Yorkshire.

²⁹Illiteracy data for some counties are missing.

³⁰Along with the obvious concern that union members' unemployment rates may not be representative of workers as a whole, one is also concerned that the unemployment rate may reflect changes in the extension of unemployment benefits by labor unions, rather than changes in unemployment.

specifications.

Prices

We use three price time series to indicate sector-specific changes in labor demand. For the coal sector, we use the log of an index of the coal price at the pithead, taken from Church (1986), Table 1.9. To generate his index, Church uses prices received by eight to twelve collieries (depending on the year) across Britain, for years prior to 1882 (collieries include Middleton colliery, near Leeds, Elphinstone colliery, in Scotland, and Cannock Chase colliery in Staffordshire, among others; details are in Church's table notes). Beginning in 1882, he uses pithead prices compiled in *Mineral Statistics*, an official publication.

For the iron sector, we use the log of the price (in shillings per ton) of Scottish pig iron found in Mitchell (1988), *Prices*, 20.B. These prices were originally collected by Augustus Sauerbeck, and published in the *Journal of the Statistical Society* in 1886 ("Prices of Commodities and the Precious Metals"), then updated thereafter in the same publication. (Cleveland pig iron prices are listed in Mitchell (1988), but only beginning in 1865; prices of Scottish pig iron track those of Cleveland pig iron very closely throughout the period we study.)

For cotton textiles, we use the log of the ratio of the output price of cotton textiles to the price of raw cotton, in order to capture changes in the output price of textiles that were not merely due to fluctuations in cotton prices. The prices of cotton textile output (in pence per linear yard) come from Mitchell (1988), *Prices*, 19. The prices reported are the average value of cotton piece goods exported in each year, not actual prices received by individual textile producers. The original sources for these data are *The Cotton Trade of Great Britain*, by T. Ellison (for prices prior to 1885) and the *Annual Statement of Trade* (from 1885 on). We convert the price per linear yard of textile output into the price per pound of output using Robson's (1957) conversion factor of 5.47895 linear yards per pound of cotton piece good exports (this has no effect on the analysis, but makes the interpretation of the relative output price more straightforward). See Robson (1957), Table A.1, p. 333. The cotton input

prices (in pence per pound of “Upland or Middling American” cotton) come from Mitchell (1988), *Prices*, 18.B. The original sources are *Sessional Papers of Parliament* (1903), vol. 68, for years prior to 1903, and Augustus Sauerbeck, “Prices of Commodities and the Precious Metals,” *Journal of the Royal Statistical Society* (1904, then updated), for years after 1902.

We also control for steel price shocks in our analysis of the repeal of penal sanctions. Our steel price data are from McCloskey (1973), whose sources are Carr and Taplin’s *History of the British Steel Industry* (1962) and Burn’s *The Economic History of Steelmaking, 1867-1939* (1940).

Wages

We construct a panel dataset of wages at the county-year level by combining several wage datasets. First, we use a panel of wages at the city-year level taken from the U.K. Data Archive, study 3710 (Southall et al., 1999). These wages were initially collected by Southall, Gilbert, and Gregory from *Rates of Wages and Hours of Labour in various industries in the United Kingdom for a series of years*, a report of the Board of Trade Labour Department. We calculate a wage index for carpenters, painters, and bricklayers and average these to create a single builders’ wage index at the city-year level. We then assign to each county-year the average builders’ wage index value for the cities in that county in the relevant year.

Coal hewers’ wages at the region-year level are taken from an index found in Church (1986), Table 7.15. The regions are the following: Scotland, the Northeast of England, Cumberland, Lancashire and Cheshire, North Wales, Yorkshire, the East Midlands, the West Midlands, South Wales, and the Southwest of England. We assign each coal-producing county in our dataset to one of these regions. The original sources of these data for years prior to 1871 are a variety of official publications and academic publications (for example, a report by the Midland Mining Commission, and several PhD theses). Beginning in 1871, publications from the Board of Trade provide most of the information (“Rates of Wages and Hours of Labor in Various Industries in the United Kingdom, 1850-1905” and “Annual Returns of Rates of Wages and Hours of Labor, 1893-1913”), though Church supplements this

material with other information from both official and scholarly publications (for example, a publication by the Royal Commission on Labor).

Time series of agricultural workers' wages, engineers' wages, and cotton factory workers' wages are taken from Mitchell (1988), *Labour Force*, Table 25. The original sources for this table are articles published by A.L. Bowley and G.H. Wood in the *Journal of the Royal Statistical Society* in 1898 and 1899, and A.L. Bowley's *Wages and Income since 1860*.

We convert each wage dataset into an index with value 100 in the year 1900. Then, we combine the several wage indices to produce a panel of wages at the county-year level, using each county's occupational distribution in the 1851 British census (taken from the UK data archive website, study 4559 (Southall et al., 2004)) to weight each index in a given county-year. Specifically, we calculate a wage at the county-year level by weighting each wage index by men's employment in the relevant industry, relative to men's employment in all of the county's industries for which we have wage data. For example, a coal-producing county for which builders' wages are available would have its coal wage in a given year multiplied by the number of men in that county in 1851 working in mining, divided by the number of men in that county working in mining, agriculture, engineering of various sorts, textiles, and construction in 1851.

As noted in the text, we also constructed an alternative wage panel that allowed occupational shares in each county to vary over time. In this case, a coal-producing county for which builders' wages are available would have its coal wage in a given year multiplied by the *interpolated* number of men in that county working in mining in that year, divided by the *interpolated* number of men in that county working in mining, agriculture, engineering of various sorts, textiles, and construction in that year (where the interpolations are based on census data taken from the UK data archive website, study 4559 (Southall et al., 2004)).

Another alternative wage panel was constructed by including iron smiths' wages from Bowley and Wood (1906), and using the same weighting scheme as in our baseline panel. As noted above, the drawback of using iron smiths' wages is that there is no satisfactory

occupation share with which to weight the wage data: the census occupational category that is best suited includes metal workers of all types, those who produce metal and those who use it as an input. Next, we constructed another panel, using our baseline wage series, but using for weights the occupation shares for all workers, not only the men's. Finally, we constructed another panel constructed just as in our baseline panel, but dropping the wages of workers most likely to be white collar workers: engineers and shipbuilders.