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Coercive Contract Enforcement: Law and the Labor Market in Nineteenth Century Industrial Britain[†]

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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. (*JEL* J31, J41, K12, K31, N33, N43)

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 nineteenth 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 Until 1875, when it was repealed, Master and Servant law gave employers the ability to criminally (as opposed to civilly) prosecute and severely punish a majority of employees across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] This law was not 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 (see Figure 1, panels A and B).[2]

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[1] Master 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 online Appendix 1A, and especially Table A1. The term "Servant" thus had far broader application in the law than in contemporary parlance.

[2] Statistics 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 five shillings in 1875.

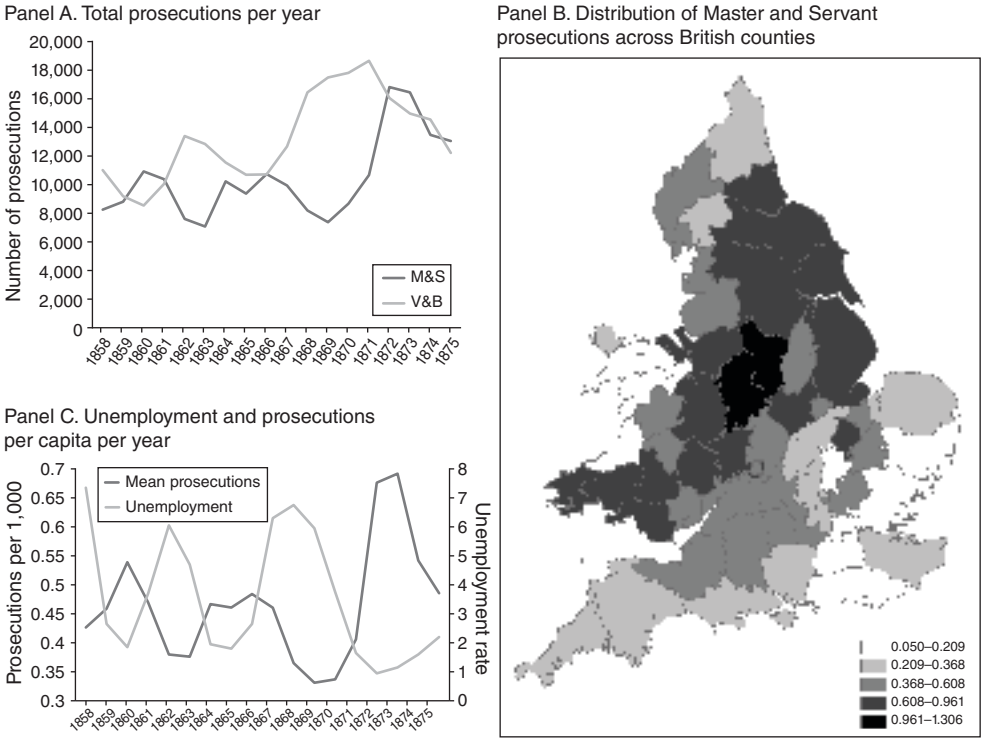

FIGURE 1

Notes: 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.

Sources: Judicial Statistics, England and Wales (panels A, B, and C), and (in panel C) the Beveridge unemployment series reported in Steinfeld (2001).

Our work theoretically and empirically studies the effects of Master and Servant law on contracting and wages in nineteenth 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 nineteenth 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

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

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," 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 Holmström 1982; Beaudry and DiNardo 1991).

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 US South (Fogel and Engerman 1974; 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 1887). 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 nineteenth century Britain. Historical evidence of the importance of criminal

[4] Malcomson (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 nineteenth century. There exist financial penalties for early termination of labor contracts, for both employees and (more often) employers, in US and UK labor markets. Noncompete clauses in contracts, direct descendants of the Master and Servant laws that we study, prevent employees from moving to competitor firms (see Marx, Strumsky, and Fleming 2007).

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

[6] What 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).

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 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 I. We present a model of contracting, contract breach, and prosecution in Section II. In Section III, we estimate empirical models motivated by the theory, examining the economic determinants of 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 IV, we summarize our findings and conclude.

I. 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 an extension of 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 (online Appendix 1A, in particular Table A1, provides a historical overview of the enactment of Master and Servant 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

[7] That 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.'"

[8] For 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).

[9] Master 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.

hard labor for up to three months for breaches of their labor agreements."[10] Because of its broad scope and potentially 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 ostensibly changed the maximal punishment for breach of contract, from 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 ultimately faced the threat of three months' imprisonment.[11]

A. *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 effort,[13] 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 percent. In the median district, the fraction of cases brought for absconding was over two-thirds.[15]

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 percent of their cases.[16]

[10] The 1823 Master and Servant Act is 4 Geo. IV c. 34; the quote comes from Steinfeld (2001, pp. 47–48). Hard labor included work at the treadmill and the crank.

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

[12] The 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 nineteenth century.

[13] A 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.

[14] The report is the *First Report of the Commissioners Appointed to Inquire into the Working of the Master and Servant Act, 1867* (Royal Commission on Labour Laws 1874).

[15] See online Appendix 1A for details.

[16] In the *First Report of the Commissioners* (Royal Commission on Labour Laws 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).

Prosecution allowed employers to harness the capacity of the local magistracy and constabulary to pursue employees who broke their contracts, and to secure their continued employment. In fact, in 70 percent of prosecutions between 1858 and 1875, an order of specific performance, or some more lenient penalty, was imposed. Around 20 percent of prosecutions ended with a fine imposed (and typically an order of specific performance as well), while the most severe penalties of imprisonment and whipping were imposed only rarely, in around 10 percent of prosecutions.[17]

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 conditions played in the employee's decision to breach a contract and the employer's decision to prosecute.[18] 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."[19] 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 online Appendix 1B.

B. *Unions and the* 1875 *Repeal of Criminal Sanctions*

In Section II, 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. 260–61) 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.[20] 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

[17] Imprisonment was imposed in 15 percent of prosecutions prior to the 1867 reform and around 5 percent after the reform. Other "lenient" penalties included wage abatements and orders for small sums of compensation to be paid to the employer for losses incurred from the breach. In some cases, the employee was allowed to leave the employer. Information on the outcomes of prosecutions comes from *Judicial Statistics*, *England and Wales*. See online Appendix 1A for details.

[18] *Report of the Select Committee on Master and Servant* (House of Commons 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.

[19] *Report of the Select Committee on Master and Servant* (House of Commons 1866, pp. 60–61).

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

at will), because they "would require greater security for the maintenance of their employment."[21]

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.[22] 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 1996, Fitzgerald 1988, and Church 1986).[23] 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., Holmström 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 nineteenth 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 nineteenth 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 vitiated 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 nineteenth century common law regarding trade unions and strikes was often ambiguous: unions existed and strikes occurred throughout the nineteenth century, though both were at times harshly treated by the legal authorities.[24] Unions were not secure prior to their unambiguous legalization in the Trade Union Act of 1871.[25] However, despite establishing unions' legality, the 1871 Act was passed alongside the Criminal Law Amendment Act, which made union activity illegal whenever the individuals involved committed a criminal offense.[26] An early twentieth century legal

[21] *Report of the Select Committee on Master and Servant* (House of Commons 1866, pp. 68 and 94).

[22] In the *First Report of the Commissioners Appointed to Inquire into the Working of the Master and Servant Act, 1867* (Royal Commission on Labour Laws 1874), we found that around 3 percent of cases were brought against employers; nearly all of these ended with the employer fulfilling his contractual obligations.

[23] Employers in the mid-nineteenth 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.

[24] See Webb and Webb (1902) for a discussion.

[25] 34 and 35 Vict. c. 31.

[26] The Criminal Law Amendment Act is 34 and 35 Vict. c. 32.

text describes the effect of the 1871 reforms as follows: "[W]hile a strike was lawful, practically anything done in pursuance of a strike was still criminal."[27] 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.[28] 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?"[29] 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.[30] Thus, the repeal of penal sanctions under Master and Servant law was part of the process of legalizing unions throughout the nineteenth century, though it affected contracting for both union members and nonmembers.

Finally, why did Parliament pass a law in the interest of workers, despite many employers' opposition to proposed reforms? Employers did try to exert their influence. One can read their views in letters to the *Times*, and see their preferences represented in Parliamentary debate—for example, MP William Forsyth's failed attempt to amend the 1875 Conspiracy and Protection of Property Bill to maintain broad conditions allowing for criminal prosecution of employees for breach of contract.[31] 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 2004, p. 209). Ultimately, the political influence of the TUC was greater than the employers': the Conservative Disraeli government that was formed after the 1874 election repealed criminal sanctions for contract breach the next year.

II. 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 simple extension of contracting models in which risk-neutral

[27] Tillyard (1916, p. 312).

[28] The Reform Act is 30 and 31 Vict. c. 102.

[29] HC Deb, March 4, 1875, vol. 222, c. 1,177.

[30] The 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.

[31] For example, one letter to the *Times,* titled "Proposed Commission on the Labour Law," March 19, 1874, argued that the 1867 reform of Master and Servant law reform was sufficient. Another letter, titled "The Labour Laws Commission," April 8, 1874, argued that Parliament did not give employers' views sufficient attention. Forsyth's proposed amendment can be found in HC Deb, July 12, 1875, vol. 225, cc. 1,341–61.

employers, who can commit to contractual terms, insure risk-averse employees (e.g., Baily 1974; Azariadis 1975; Harris and Holmström 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 $[0, 1]$.[32] 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.[33]

A. *Agents and Timing*

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

- In node 1 in Figure 2, the employer either offers an employee a contract specifying a pre-committed wage[34] $\overline{w}$ 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 and the employer hires labor at the outside wage, receiving payoffs $u(w)$, and $\pi - w$, respectively.[35]
- 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, $u(\overline{w})$, and the employer receives $\pi - \overline{w}$.
- 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").[36] It is important to note that prosecution was not always successful; it usually was (see Section I), but it might be difficult to locate an employee who left, or to prove that a binding contract was agreed to.

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

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

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

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

[36] Prosecution 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.

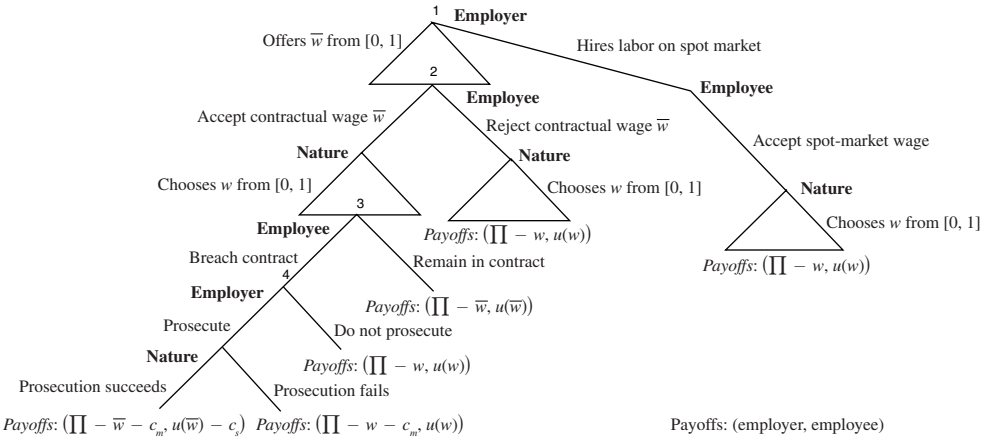

FIGURE 2. GAME TREE

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 $u(\overline{w}) - c_s$ (recall that c_s was the cost to the "servant" of being prosecuted successfully), while the payoff for the employer is $\pi - \overline{w} - c_m$.[37] The employee suffered his punishment and was then legally obligated to return to work at the contractual wage (see Section I).[38]
- 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).[39]

B. *Optimal Strategies and Equilibrium*

We focus on a pure strategy subgame-perfect equilibrium. For the employer, a strategy is of the form $(\mathit{offer}, \overline{w}, R(w))$: the employer chooses whether to offer a contract; the stipulated wage $\overline{w}$ 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 $(\mathit{accept}(\overline{w}), B(w, \overline{w}))$: the employee chooses whether to accept the contractual offer $\overline{w}$; 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.

[37] We 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.

[38] It 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.

[39] With the cost of prosecution sunk, the employer will choose to hire a worker from the spot market, again because $\pi > 1$.

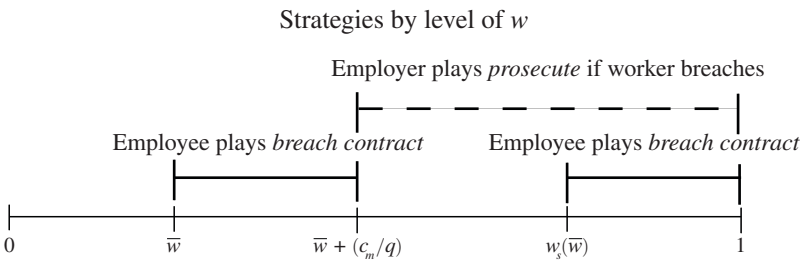

FIGURE 3. STRATEGIES ACCORDING TO THE VALUE OF THE SPOT MARKET WAGE

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

$$R(w) = 1 \Leftrightarrow w > \overline{w} + \frac{c_m}{q}. \tag{1}$$

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.

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:

$$\begin{aligned} B(w,\overline{w}) = 1 \quad \text{if} \quad & u(\overline{w}) < u(w)\big(1 - R(w)\big) \\ & + u(w)R(w)(1 - q) + \big(u(\overline{w}) - c_s\big)R(w)\,q. \end{aligned} \tag{2}$$

Using equation (1), we can show that

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

If the outside wage is less than the contractual wage, the employee never breaches the contract: there is no incentive to do so ($B(w,\overline{w}) = 0$). Equation (3) shows that there is a range of w such that $B(w,\overline{w}) = 1$ 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, $w > \overline{w} + c_m/q$), 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:

$$u(w) > \frac{u(\overline{w}) - q\big(u(\overline{w}) - c_s\big)}{1 - q}. \tag{4}$$

Thus, the employee chooses to breach the contract ($B(w,\overline{w}) = 1$) 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 $\overline{w}$:

$$u(w_s) = u(\overline{w}) + \frac{qc_s}{1-q}. \tag{5}$$

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

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

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 $w_s(\overline{w}) > \overline{w} + c_m/q$, though our results do not depend on it. We assume the following:

$$\text{ASSUMPTION 1:} \quad u(\overline{w} + \frac{c_m}{q}) < u(\overline{w}) + \frac{q\,c_s}{1-q} \tag{7}$$

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

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 $u(1/2 - r_s) = \int_0^1 u(w)\,dw$.

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 exists a* $\overline{w}$ *that satisfies the employee's and the employer's participation constraints*, *and a pure-strategy subgame perfect Nash equilibrium with the employer's strategy* $(\textit{make offer}, \overline{w}, R(w))$ *and the employee's strategy* $(\textit{accept}, B(w,\overline{w}))$.

PROOF:

See online 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 nineteenth 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.[40] 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 one to two weeks of a coal miner's wage, or around 2–4 percent of a year's salary.[41] 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 percent of the average spot market wage seems reasonable.[42]

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.[43] Though our model is an extreme simplification, it captures many of the basic elements of contracting in nineteenth century Britain.[44]

C. *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 online 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.

[40] As 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, or 1.5.

[41] See Bowley (1900, pp. 107–09). Because Master and Servant cases were summarily decided, legal and time costs to employers bringing cases were low.

[42] In 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 our choices of costs are conservative.

[43] With a coefficient of relative risk aversion of 0.95 and the parameter values in the text, our simple exercise generates prosecutions in 8 percent 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 percent of wage draws.

[44] Note 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).

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

PROOF:

See online 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.

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.[45] 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."[46] 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.[47]

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.*

[45] The 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.

[46] Emphasis in the original.

[47] Note 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.

PROOF:

See online 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.[48] 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 of over 25 percent), 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.

III. Empirical Evidence on Prosecutions and Wages

A. *The Data*

To estimate the relationship between labor demand and Master and Servant prosecutions, we combine data from a variety of historical sources.[49] 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.[50] 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.[51] 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

[48] Note 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.

[49] For a more detailed discussion of the data used and the various sources, please see online Appendix 4.

[50] Note 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.

[51] From "Minerals" (House of Commons 1856).

use several time series on prices, collected from *British Historical Statistics* (Mitchell 1988), *The History of the British Coal Industry* (Church 1986), 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.[52] 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 level, with county-level variables being applied to all districts within a given county.[53] 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.

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.[54] 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.[55] In our analysis of the relationship between wages and labor demand shocks we include controls for steel price shocks in some specifications.[56] Summary statistics of the variables used in our analysis of the consequences of repeal are presented in Table 1, panel B.

B. *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).[57] 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 nonwage input). Increases in these prices indicate that the marginal revenue product of labor is high in the three industries.

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

[53] Standard errors in our regressions are always clustered at the county level.

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

[55] While we have prosecutions data only for the 1858–1875 period, we can construct a panel of wages and prices for the period 1851–1905.

[56] Data are from McCloskey (1973).

[57] The 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 nineteenth century all seem to have fit this requirement.

TABLE 1—SUMMARY STATISTICS

Variable	Observations	Mean	SD
Panel A. Prosecutions analysis			
District panel data			
Master and Servant prosecutions	3,942	47.72	120.30
Vagrancy prosecutions	3,942	60.62	156.30
Urban dummy	3,942	0.74	0.44
County panel data			
Master and Servant pros./1,000	936	0.46	0.36
Vagrancy prosecutions/1,000	936	0.62	0.40
Population	936	412.38	595.91
Union membership	936	52.34	56.29
Illiteracy rate	900	0.25	0.07
Strike rate	640	0.00	0.01
ASE unemployment rate	640	0.02	0.03
Cross-sectional county data			
Fraction employed in textiles in 1851	52	0.05	0.07
Iron county dummy	52	0.48	0.50
Coal producing county dummy	52	0.38	0.49
Population density 1851	52	0.96	4.15
Income 1851	52	10.48	2.88
Wales dummy	52	0.25	0.44
Proportion urban	52	0.12	0.19
log iron ore production	52	5.05	5.52
Distance to Lancashire	52	160.82	86.94
Time-series data			
log cotton price ratio	18	0.72	0.28
log coal price	18	4.04	0.29
log iron price	18	4.11	0.25
Panel B. Repeal analysis			
County panel data			
log county wage index	2,860	4.46	0.14
Union membership	2,860	63.37	67.85
Population density	2,860	1.41	6.64
Proportion urban	2,860	52.34	56.29
log income	2,860	2.52	0.33
Population	2,860	476.72	740.14
Illiteracy rate	2,740	0.17	0.11
Strike rate	1,954	0.00	0.01
ASE unemployment rate	1,954	0.03	0.03
Cross-sectional county data			
log average prosecutions per 1,000 people, in 1858–1875 period	52	−0.98	0.72
Fraction employed in textiles in 1851	52	0.05	0.07
Iron county dummy	52	0.48	0.50
Coal producing county dummy	52	0.38	0.49
Population density 1851	52	0.96	4.15
Income 1851	52	10.48	2.88
Wales dummy	52	0.25	0.44
Proportion urban	52	0.12	0.19
Time-series data			
log cotton price ratio	55	0.94	0.25
log coal price	55	4.03	0.24
log iron price	55	3.99	0.22
log steel price	40	4.85	0.45

Sources: See online Appendix 4.

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.[58] 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:

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

The dependent variable is the number of prosecutions in district d in county c at time 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.[59]

In Table 2, columns 1–3, we present results of estimating the model for each industry individually (without the time-varying controls).[60] 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.[61] Columns 2 and 3 show that higher output prices

[58] We 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 online 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.

[59] Using 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.

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

[61] We 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

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

	OLS						2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction textiles 1851 × 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 × 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 × log(coal price)			68.32*** (15.90)	41.25*** (10.11)	35.63** (14.31)	27.50*** (8.428)	25.22* (14.92)	26.82** (12.05)
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	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	No	No	No	Yes	Yes	No	Yes
County-specific trends	No	No	No	No	No	Yes	No	No
Observations	3,942	3,942	3,942	3,942	3,942	3,942	3,942	3,942

Notes: 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 online Appendix.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

in the coal and iron sectors are associated with more prosecutions, precisely in counties where the relevant industry is prevalent.

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 (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.[62]

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

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.

[62] We examine the relationship between industry price shocks and prosecutions in a more general model that includes lagged and leading price shocks in online 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.

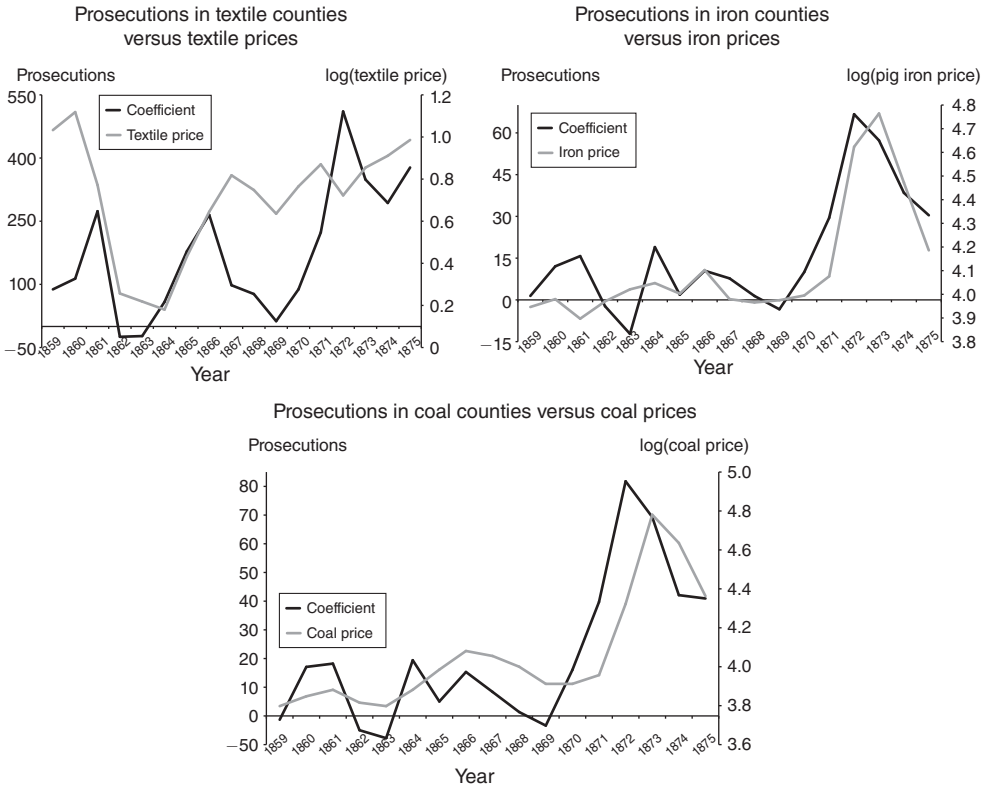

FIGURE 4

Notes: 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.

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 percent.

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. 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.[63]

The coefficients in column 6 indicate that a 25 percent 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

[63] In 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.

per district in a county with the highest amount of employment in textiles (28 percent), 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.

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.[64] 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).[65] As an exogenous determinant of pig iron production, we use the county's production of iron ore in 1855.[66] 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.[67] 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.

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 online 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.[68]

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 and 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

[64] See Ellison and Glaeser (1999) on industry agglomeration.

[65] Our strategy is similar to Badiani (2010).

[66] The source of the data is "Minerals" (House of Commons 1856). See online Appendices 2 and 4 for details.

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

[68] We 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.

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 × 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 × 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 × 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	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	Yes	No	Yes
Observations	936	936	936	936	930	930

Notes: 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

prosecutions.[69] 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 and 4, we use prosecutions per capita as our outcome variable.[70] In these specifications, again, we generally find large, positive and statistically significant effects 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 and 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 percent level across specifications. Owing to the log-log specification, the coefficients in these specifications are naturally interpretable as elasticities, once

[69] One 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.

[70] In 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 and 6, which use log prosecutions per capita as the outcome. Including the population control does not alter any results.

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.

C. *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.[71] 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.[72] 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.[73]

To examine the response of anti-vagrancy prosecutions to labor demand shocks, we estimate a specification similar to that in Table 2, column 4, but use anti-vagrancy prosecutions as the outcome. We present the results in Table 4, column 1, and find that the estimated coefficients on the labor demand shocks are very small, and statistically insignificant, both individually and jointly.[74] 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 a potential concern, and so in column 2 of Table 4, we include controls for union membership (membership in the ASE) as well as the fraction of

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

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

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

[74] In online 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.

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

	Vagrancy and begging prosecutions	Controlling for unions' activity			Labor market institutions and conditions		Town and country	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction textiles 1851 × log(cotton price ratio)	32.69	160.7***	125.4***	130.3***	150.8***	148.0***	136.5***	350.3**
	(78.37)	(43.89)	(40.76)	(43.34)	(29.77)	(41.11)	(45.87)	(135.1)
Iron county × log(iron price)	−14.71	61.25**	67.15**	73.14*	59.93**	68.03**	10.74	136.7**
	(30.18)	(23.71)	(31.32)	(36.29)	(22.97)	(28.77)	(10.15)	(58.40)
Coal county × log(coal price)	−23.12	37.09***	45.55*	44.19	33.19***	34.76**	18.09**	145.2***
	(28.48)	(11.30)	(22.97)	(27.20)	(11.25)	(13.03)	(8.397)	(45.75)
log(population)	164.5	104.4*	36.65	63.01	111.5*	77.08	34.37	249.9**
	(117.7)	(52.62)	(22.82)	(43.80)	(59.08)	(55.10)	(26.61)	(113.5)
F-statistic *p*-value on joint significance	0.839	0.000	0.008	0.022	0.000	0.000	0.013	0.002
Last year in sample period	1875	1875	1871	1871	1875	1875	1875	1875
Sample of districts	All	All	All	All	All	All	Urban	Rural
Controls for union membership and strike rate	No	Yes	No	Yes	Yes	Yes	No	No
Controls for illiteracy rate and unemployment rate	No	No	No	No	Yes	Yes	No	No
Time-varying controls	No	No	No	No	No	Yes	No	No
Observations	3,942	3,341	3,066	2,592	3,328	3,328	2,898	1,044

Notes: Dependent variable in column 1 is the number of vagrancy and begging prosecutions in a district. Dependent variable in columns 2–8 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

members receiving strike pay (the "strike rate").[75] 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.[76]

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 3 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 4, we both restrict

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

[76] The 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.

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 percent. 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 reflect 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 5, 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.[77] Including these controls does not change our results: all of our labor-demand shock coefficients are large and statistically significant. In Table 4, column 6, 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.[78] 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 7 and 8.

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, with the joint tests significant at almost 1 percent in the urban sample.[79] These results are strong evidence that labor market coercion not only existed in rural Britain as it industrialized, but was also a widely used response to labor demand shocks in urban areas, especially where textile production was located.

[77] Both 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 5 and 6.

[78] This 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.

[79] In 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 subsamples.

D. *Repeal of Penal Sanctions and Average Wages*

In 1875, the penal aspects of Master and Servant law were abolished.[80] Our model suggests that without penal sanctions to keep workers in their contracts, average wages should rise, 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.[81] 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:

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

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.[82]

[80] Importantly, 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.

[81] Most 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.

[82] The 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 these. Online 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 online Appendix.

TABLE 5—EFFECT OF REPEAL ON WAGE LEVELS, BY AVERAGE PROSECUTIONS

	OLS							Arellano-Bond
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-1875 × 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)	0.0437 (0.0500)
Lagged log (wage)				0.861*** (0.0198)	0.849*** (0.0125)	0.837*** (0.0111)	0.836*** (0.0110)	0.813*** (0.0207)
Time-varying controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Labor market controls	No	No	No	No	No	Yes	No	No
Post-1875 × county controls	No	No	No	No	No	No	Yes	No
County-specific recession effect	No	No	Yes	No	Yes	Yes	Yes	Yes
Observations	2,860	2,860	2,392	2,808	2,392	1,685	2,392	2,392

Notes: Dependent variable is log county wage. Standard errors (in parentheses) are clustered by county, except in the case of the Arellano-Bond estimator (Arellano and Bond 1991), 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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 percent increase in 1858–1875 prosecutions per capita, roughly 1 standard deviation, before repeal resulted in an almost 1.5 percent 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 increase in prosecutions per capita results in roughly 1 percent 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.

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.[83] The coefficient

[83] Including 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.

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 percent 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 percent 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 characteristics (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.[84]

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.

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 interaction terms around the time of repeal, along with the 95 percent 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.[85] Though we cannot rule out the possibility that some unobserved change occurred in high-prosecution counties concurrently with the repeal of Master and

[84] This specification, like column 5, includes the union membership control as well.

[85] Although 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.

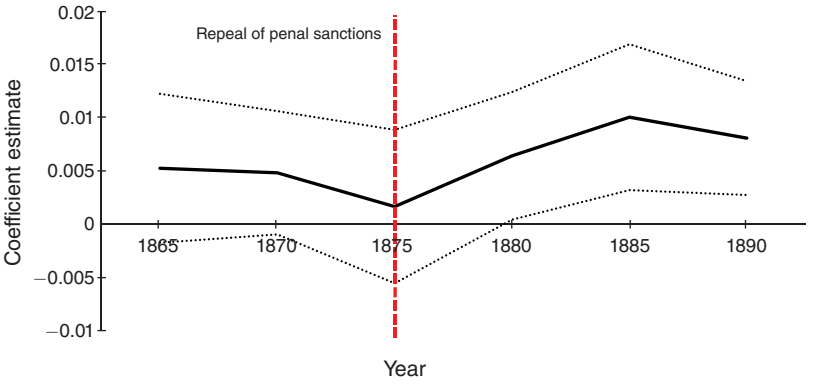

FIGURE 5. WAGES IN HIGH PROSECUTION COUNTIES RELATIVE TO LOW PROSECUTION COUNTIES, BEFORE AND AFTER REPEAL OF PENAL SANCTIONS

Notes: Wages in high prosecution counties, relative to low prosecution counties, before and after repeal of penal sanctions. Figure plots coefficients and their 95 percent confidence intervals (dotted lines) 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.

Servant's penal sanctions, our results suggest that repeal of penal sanctions did raise wages.[86]

E. *Labor Demand Shocks and Wages Following Repeal*

Our model predicts that wages should have responded weakly or nonmonotonically 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 II). 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.[87] In 1883, the ratio of steel to iron

[86] Note that the specification in Table 5, column 7, rules out the possibility that these sharp changes were strongly correlated with the presence of our three industries of interest or with the initial level of unionization in a county.

[87] While 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.

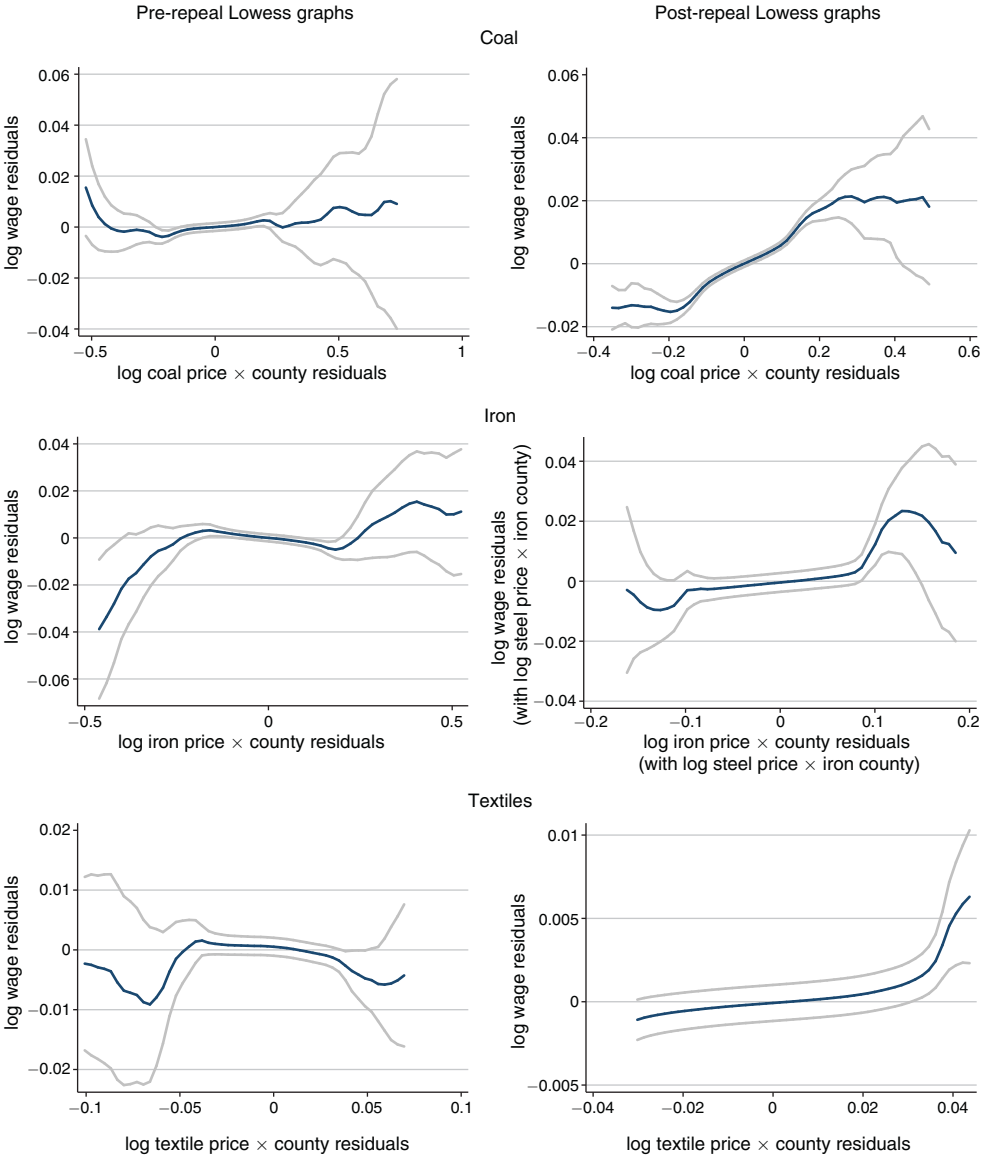

FIGURE 6

Notes: 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.

production was 14 percent, growing to 62 percent 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).[88]

The nonparametric graphical evidence is consistent with the hypothesis that wages responded nonmonotonically to labor demand shocks prior to the 1875 repeal of penal sanctions for contract breach. Our nineteenth 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.[89] 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 nonexistent, linear relationship between labor demand shocks and wages pre-repeal, because of both upward and downward wage rigidities, and regions of nonmonotonicity. 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.[90] 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.[91] 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 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

[88] In online Appendix 2, Figure A4, 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 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.

[89] The 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.

[90] In 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 online Appendix 2, Table A10, we present results excluding the steel price shocks from the iron price shock specifications.

[91] In online 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 1864).

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 × log (cotton price ratio)	−0.0071 (0.109)			−0.0017 (0.107)	0.278*** (0.0951)			0.102 (0.0925)
Iron county × log (iron price)		−0.0028 (0.0214)		−0.0081 (0.0215)		0.175*** (0.0633)		0.126** (0.0494)
Coal county × log (coal price)			0.0149 (0.0205)	0.0167 (0.0203)			0.101*** (0.0176)	0.105*** (0.0196)
Iron county × 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	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,300	1,300	1,300	1,300	1,560	1,560	1,560	1,560

Notes: 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

significant well below 1 percent.[92] 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.

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

[92] Estimating 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 online Appendix 2, Table A10.

(where repeal would have had the greatest impact). Formally, we estimate the following regression:

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

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 percent.

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 percent).

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-1875 × textile county × 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 × iron county × 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 × coal county × 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 × textile county × textile price × log (average prosecutions)							0.114* (0.058)
Post-1875 × iron county × iron price × log (average prosecutions)							0.0012 (0.006)
Post-1875 × coal county × coal price × log (average prosecutions)							0.0145 (0.016)
log (population)	0.0621** (0.024)	0.0692*** (0.023)	0.0081** (0.004)	0.0088*** (0.003)	0.0047 (0.006)	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 × 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	No	No	No	No	Yes	Yes	No
Steel price × iron county control	No	Yes	No	Yes	No	Yes	No
Interpolated controls	No	No	No	No	Yes	Yes	No
Union membership control	No	No	No	No	Yes	Yes	No
Trend × county characteristics	No	No	No	No	Yes	Yes	No
Observations	2,860	2,080	2,808	2,080	2,808	2,080	2,808

Notes: 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

differently over time according to the industrial composition or unionization level in a county.[93] 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

[93] In 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 online 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 online Appendix 2, Table A8 and Figure A3.

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 percent in both of these specifications.

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 four variables.[94] 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.

IV. Conclusion

Coercive legal restrictions on labor mobility existed in Britain well into the second half of the nineteenth 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 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

[94] We 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.

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 nineteenth 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.

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